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# THE AMERICAN ECONOMIC REVIEW

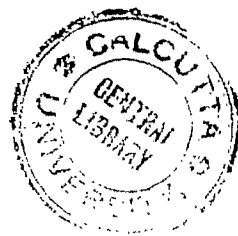
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Number 65 of a series of photographs of past presidents of the Association.



*John H. Calver*

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# The American Economic Review

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## INTEGRATION AND GROWTH OF THE WORLD ECONOMY IN HISTORICAL PERSPECTIVE\*

*By* GOTTFRIED HABERLER

We live in the age of integration. Every conceivable—or inconceivable—combination of countries has been proposed, more or less seriously, as a candidate for integration—other planets and outer space being almost the only areas that do not yet figure in any of the many plans and proposals. With the formation of the European Economic Community, the European Common Market as a going concern, integration has passed from the talking stage, where it had remained for many years, into the stage of concrete realization. The example of the Common Market, beside spawning a rival European combination (EFTA), has induced the creation of similar schemes in other parts of the world—in South and Central America—and still others are on the drawing board. I do not intend to offer yet another comparative analysis of any of those schemes. What I should like to do is to put the modern integration movement into broader historical perspective of the growing world economy.

Without wishing to engage in a lengthy discussion of the proper or useful definition of terms, I think I should first indicate very briefly what we mean by integration—I say “we” and not “I,” because I hope for fairly general agreement. We mean by integration closer economic relations between the areas concerned. This is clearly a concept capable of continuous gradation. One first stage is characterized by free commodity trade, greater division of labor, tending toward equalization of commodity prices. We have a higher stage of integration when factors of production can move freely with a consequent tendency for factor price equalization. (Allow me to skip the famous controversy whether complete factor price equalization would result from free commodity trade alone, with the remark that such equalization could indeed result, but only under such unrealistic assumptions that for practical purposes we can say that it would *not*. I hope that speaking

\* Presidential address delivered at the Seventy-Sixth Annual Meeting of the American Economic Association, Boston, December 27, 1963.

of "unrealistic assumptions" will not embroil me in another famous and as yet unresolved controversy—namely, whether it is possible to reach reasonable conclusions from absurd premises.) We have a still higher stage of integration if all or the most important phases of economic policy are coordinated, and positive steps are taken to equalize commodity and factor prices.

I shall refrain from arbitrarily designating, as some have done, any one stage as that of "true integration" and shall not go into the substantive question whether the achievement of a lower stage of integration by itself may have undesirable results which would involve probing the mysterious depth of the theory of the "second best."

### I. *Waves of Integration*

The recent movements toward integration are often regarded as an entirely new development which has been envisioned and proposed by many but has never before made headway on a significant scale. I submit that this is true only in a formal and narrow sense.

Looking over the evolution of the world economy during the last two hundred years or so, we can clearly discern three big waves of integration—preceding and dwarfing the regional integration movements of the last ten years. The secular trend toward integration and growth of the world economy was interrupted by one period of sharp decline and disintegration.

Let me first briefly identify these partly overlapping waves and then discuss them in turn.

The first wave was the internal integration of the economies of the nation states which we find today on the map of the world. The economy of Great Britain was the first to be unified and integrated, the French and American followed soon after. Later came other Continental European and overseas countries.

The second wave overlapping or, if you prefer, superimposed upon the first was the free trade movement that reached its high point at the end of the 1870's. The movement towards freer trade was reversed in 1878, but despite the fact that in most countries tariffs became higher and higher, world trade continued to grow rapidly up to World War I. For the underlying forces of rapid technological progress in transport and mass production, as well as massive migration of labor and capital toward the regions of "recent settlement" in the Western Hemisphere, Oceania, and South Africa, still dominated over increasingly protectionist commercial policies in many parts of the world. The more drastic modern methods of trade restriction, quotas and exchange control, had not yet made their appearance, and the movement toward collectivism, interventionism, and national economic planning, which

later became a formidable obstacle for world-wide integration, had not yet made much headway.

The first serious disintegration of the world economy, foreshadowed by the rising protectionism and economic nationalism from 1880 on, became a fact during and immediately after the World War of 1914-18. After a brief interlude of freer trade in the 1920's, real disintegration and rapid decline of world trade set in with the Great Depression. After an incomplete and hesitant recovery, disintegration reached its nadir in World War II.

The third wave of world-wide integration and growth started soon after the end of World War II, gathered momentum after 1948, and is still in progress. It has been centered on and propelled by the spectacular recovery and rapid growth of all industrial countries, including the United States and United Kingdom, notwithstanding their recent troubles and their lower growth rates compared with the Continent of Europe and Japan. Contrary to what is often said, the prosperity of the "industrial centers" of the world economy did spread, though not as rapidly and fully as one would wish, to the "periphery" of the less developed countries, to use Raul Prebisch's picturesque designation of the two areas. This wave of world-wide integration has had more powerful and beneficial effects than the much more advertised and talked-about series of regional integrations.

## II. *National Economic Integration*

Let me now add some observations on each of these phases of the growing world economy. Later I shall analyze recent developments in somewhat greater detail in the light of the historical perspective gained.

The internal integration of the British and French economies was vigorously pursued during the mercantilistic era. The economy of Great Britain emerged as a fully integrated unit in the eighteenth century and the economic integration of France was completed when the French Revolution swept away all still existing internal barriers to trade and migration. In both countries, as well as in almost all others, political and administrative unification and centralization preceded and promoted economic integration. Early attempts at close Anglo-French economic co-operation, sponsored by the physiocratic advisers of Louis XVI and by Adam Smith's influence on the younger Pitt, culminated in the Eden Treaty of 1786 which brought drastic tariff reductions. But the movement toward freer trade was premature and was soon interrupted by the French Revolution and the following wars. Not before 1860 was the time ripe to accomplish what had been tried 75 years earlier.

The American economy had the good fortune of having been integrated at a very early stage. When the War of Independence came to an end, the economic relations between the states resembled those of the German states after the Congress of Vienna. Under the Articles of Confederation each of the 13 states of the Union pursued its own tariff policy. But through the Constitution a huge free trade area for the unimpeded movement of goods and services, of labor and capital was created, which was surely a major factor in U.S. economic development.

On the Continent of Europe the major steps toward integrated national economies were the economic unification of Germany through the German *Zollverein* (1834) and the emergence of a unified Italian state in 1861.

Alfred Marshall called the *Zollverein* "the most important movement toward free trade that the world had ever seen, except the contemporary reform of the British fiscal system" [12, p. 399]. I think Marshall was right, for from the political and historical standpoint, if not from the economic one, national economic unification was an indispensable prerequisite not only for the economic development of the various countries concerned, but also for the growth of the world economy. This remains true even though we now know that the hopes and expectations so widely held in the mid-nineteenth century, that national free trade and integration would lead in straight and unbroken succession to world-wide free or near-free trade, were not fulfilled. To question Marshall's judgment concerning the *Zollverein* on the ground of the next hundred years of German history would imply extreme and untenable historical determinism.

There is no doubt that the Italian unification did for the development of the Italian economy as a whole what the German unification did for the German economy. This would be true even if the widely held theory were correct that the economic integration and internal free trade caused a retardation of the economic development of the Italian South as a region in an absolute sense, not merely relatively to the more progressive North.

The German and Italian national economic integration was preceded and followed by similar movements in many other European and overseas countries.

### III. *World-Wide Integration Through Freer Trade*

While this was going on, the second wave of world-wide integration, as I called it, started—namely, the free trade movement of the mid-nineteenth century. It was spearheaded by Great Britain, but spread widely (especially after 1860) to the Continent of Europe. Let me briefly recall a few landmarks—abolition of the Corn Laws in 1846,

the Cobden-Chevalier Treaty of 1860 between Great Britain and France, which brought a radical reversal from high protection to something near free trade in France, and following the Franco-British accord a number of commercial treaties which substantially reduced tariffs all over Western and Central Europe. The impact of the free trade movement was not confined to commercial policy and tariffs. Shipping policy, which was extremely restrictive everywhere in the early nineteenth century, was radically liberalized, first in Great Britain as early as the 1830's and later in other countries. Furthermore, the introduction of the "open door" policy in the British crown colonies and in others was a major step towards the liberalization of world trade.

As far as tariff policy is concerned, the movement towards freer trade came to an end in the last years of the 1870's. The two major factors that caused the reversal of liberal commercial policy, first in Germany (1878) and a little later in France and elsewhere, were the accelerated influx of agricultural products from overseas countries—United States, Canada, Argentina, Australia—and the impact of the severe depressions of the 1870's and 1890's. But although import tariffs in many countries showed a definite tendency to go higher during the last quarter of the nineteenth century and the first decade of the twentieth century, the volume of world trade rose rapidly throughout the whole period up to World War I. From about 1895 the rate of growth was indeed substantially higher than during the preceding two, and possibly three, decades.

A number of factors account for the growth of trade in the face of rising tariffs. The technological revolution in production and transportation continued, of course, without let-up. However this factor alone might not have been sufficient to support the growth of world trade as is shown by the development during the next period, namely the interwar years during which world trade did not show much growth, although one could hardly say that technological progress had slowed down.

The flow of capital and especially of labor from Europe to the Western Hemisphere and a few other areas continued at a rapid and increasing rate until 1914, after which it slowed down sharply. Moreover, while tariffs went higher after 1878, they were on the whole still moderate compared with those prevailing after World War I and, what is equally important, much more stable. Furthermore, the international payments mechanism improved with the adoption of the gold standard by many countries in the last quarter of the nineteenth century. International clearing of balances aided by the easy flow of short-term credit through London and other financial centers in Western Europe and elsewhere promoted multilateral trade around the globe. Payment restric-

tions were, of course, almost totally absent, and so were other quantitative trade restrictions such as quotas and export or import monopolies.

We are thus justified, it seems to me, to speak of a long period of world-wide integration during which most countries were knit together ever more closely in a network of multilateral trade and payments. It is true that many writers—most of them later with the benefit of hindsight, though one can also find a few contemporary prophets of doom—thought that they could detect tendencies of incipient disequilibrium and disintegration. As far as the economics of the matter is concerned, these are merely *ex post* rationalizations and explanations of what happened later. It is only in the field of ideologies, it seems to me, that a case can be made for the theory that later events were the likely and natural (though not the unavoidable) consequence of earlier tendencies. Concretely, it can perhaps be argued that the rising tide of economic and political nationalism and antiliberal thinking and policies in general led to World War I and its aftermath. But there surely was no inevitable economic nexus in the sense that a deep-seated real maladjustment had arisen in the world economy, something analogous to what is supposed by many business cycle theorists to develop in major if not all business cycle upswings—a real disequilibrium which makes a painful and long drawn out readjustment all but inevitable.

The statement that there was nothing basically wrong with the pre-1914 world economy will probably not encounter strenuous objections from the present generation of economists, although it is contradictory to Marxian-type theories, especially the neo-Marxian theory of imperialism, and to the Keynesian secular stagnation doctrine.

But the justification of what I have said must to a large extent rest on the plausibility of an alternative explanation of the events of the next thirty years.

#### IV. *Disintegration of the World Economy, 1914-45*

The period from 1914 to 1945, bordered by two world wars and bisected by a catastrophic depression, can surely be described as one of disintegration of the world economy and declining world trade, under any reasonable definition of these terms.

The disintegrating forces were so strong that it is surprising that world trade did expand at all. Let me briefly recall the major developments.

World War I was followed by acute financial disorders, rapid inflation in many countries and hyperinflations in some. In the United States, the period of inflation ended in the severe depression of 1920-21 which had repercussions in many countries around the world, while in

Europe inflation was stopped abruptly in a series of successful stabilization efforts between 1922 and 1925 without any immediate depressive effects.

By the middle of the 1920's the world was back on what looked like a normal course with most currencies again convertible. It is true, tariffs were higher and less stable than before and had a tendency to go higher; moreover, the integrated area of the Austro-Hungarian monarchy was fragmented into several pieces. But despite these unfavorable developments, world trade expanded fairly rapidly to something like 30 per cent above the prewar level. The effective exclusion of Russia from the world economy, although an event of tremendous political consequence, had a very limited economic impact on the world economy during the interwar period.<sup>1</sup>

Then came the Great Depression and the picture changed completely. The few remaining free trade countries, Great Britain<sup>1a</sup> first, switched abruptly to high protection. Tariffs everywhere were raised rapidly and almost all countries introduced quotas, exchange control, import prohibitions, bilateral clearings—methods of international trading which in peacetime had literally not been known for centuries. By 1932 the dollar value of world exports had tumbled to about a third of its 1929 level and its quantum was where it had been in 1913.

The quantum of trade in manufactured articles declined much faster than that of trade in primary products, but the value of the latter fell more, which reflects the extremely sharp deterioration of the terms of trade for the exporters of primary products.

In 1938 the quantum of trade of primary products was again 16 per cent above the 1913 level. While the world index of manufacturing activity in 1936-38 was 85 per cent above the 1913 level, the quantum of trade of manufactured articles was some 8 per cent below 1913 [3] [25, Ch. 2.]<sup>2</sup> This contrast can be regarded as a rough indication of the degree of disintegration of the world economy that had taken place.

For the purposes of this paper, as indicated above, I have to venture to express my views on the basic causes of the exceptional severity and length of the Great Depression. At the time when it happened most

<sup>1</sup> It is interesting that only at the lowest point of the depression in 1931-32 did trade with Russia temporarily assume a somewhat greater importance for some European countries.

<sup>1a</sup> It is true that during and immediately after World War I Great Britain applied a stiff dose of tariff protection (the so-called "MacKenna" and "Key Industry" duties). But they applied only to a short list of industrial articles, and some were temporarily removed by Philip Snowden, Chancellor of the Exchequer in the first MacDonald ministry in 1924.

<sup>2</sup> The concept "world manufacturing activity" is, of course, somewhat nebulous and its measure arbitrary and imprecise. But the statements in the text could be restated in terms of GNP, industrial production, and trade of the major countries, concepts that are less objectionable than "world production," etc.

economists did not know what had hit them. Marxian economists, of course, took the economic catastrophe simply as a confirmation of Marx's theory that depressions would become more and more severe until the capitalist system would come down in a final big crash. (In the 1960's this view has lost conviction, even among the faithful.) Others who lacked the prophet's guidance through the maze of world history had a harder time to explain the slump. Some spoke vaguely of big real maladjustments in the structure of production that had accumulated during the 1920's and others of the coincidence, accidental or otherwise, of the contraction phase of several superimposed types of concurrent economic fluctuations.<sup>3</sup> The Keynesians later fashioned their theory of secular stagnation.

From the present vantage point, the explanation of the exceptional virulence of the Great Depression seems much simpler. It was mainly due to the wholesale destruction of money, which in turn was largely the consequence of institutional weaknesses and incredibly poor policies, on the national and international level. Let me mention only a few of these. The collapse of the American banking system, the bankruptcy of thousands of banks, and the overly timid<sup>3a</sup> monetary policy which failed to counteract energetically and to stop the raging deflation explain, I venture to say, the "darkest hues" (Schumpeter) of the depression in the United States. Money supply contracted by something like 30 per cent from 1929 to 1933. The deflation could not have been nearly as severe if there had existed then, as there exists now, effective deposit insurance, or if the United States, instead of the archaic unit banking system, had had an efficient branch banking system like Great Britain,<sup>4</sup> not to mention "100 per cent banking."

The mistakes of the early New Deal of pressing hastily for often poorly conceived reforms, of fostering monopolies of business (N.R.A.) and labor and thus raising costs and frightening investors, instead of concentrating at first all energies on expansionary measures to bring about quick recovery (as Keynes among others recommended), explain why in 1939 unemployment was still over 17 per cent. Policy mistakes of a similar kind and magnitude were made in several other important countries, for example in pre-Nazi Germany and under the popular front government in France. Just as Roosevelt ignored Keynes' sug-

<sup>3</sup> Schumpeter himself did not really take seriously his suggestion that the coincidence of a Kondratieff, Juglar and Kitchin depression accounted for the severity of the 1930 depression. On the contrary, he said that "the darkest hues of cyclical depressions . . . are due to adventitious circumstances." See [19, p. 150].

<sup>3a</sup> The timidity of monetary policy should be judged by the magnitude of the problem (the intensity of the deflation which was to be offset) and not by historical standards.

<sup>4</sup> If anybody doubts this statement, let him read Jacob Viner's brilliant, convincing, and blunt but well-balanced analysis in [24]. Also see [1].

gestion that he postpone reforms until recovery had made good progress, so did Léon Blum the advice of his Scandinavian and British socialist friends who recommended that expansion take precedence over reform.<sup>5</sup>

On the international level the slow-motion, beggar-my-neighbor devaluation of all currencies of the world at intervals of a few years—sterling in 1931, the dollar in 1933-34, the gold bloc currencies in 1936 and so on—inflicted upon different groups of countries protracted periods of overvalued currencies, causing deficits and losses, further trade restrictions, and more depression. Although each of these devaluations in isolation can be defended as unavoidable, their time pattern stamped the whole approach as a sadistic policy, calculated to maximize pain and destruction. It was like cutting off the tail of a dog piece by piece instead of all at once, the way it surely would have been done had there existed the *International Monetary Fund*.

These institutional defects and horrendous policy mistakes have nothing to do with basic weaknesses or contradictions of the capitalist, free enterprise economy, or with a tendency toward secular stagnation, lack of investment opportunities and a chronic tendency toward over-saving, or gigantic real maladjustments. At any rate, wherever the monetary deflation was stopped by orthodox or unorthodox measures without at the same time sharply raising costs, the alleged real, structural contradictions, weaknesses, and maladjustments disappeared as fast as they had appeared a few years earlier.

It is true the methods used to stop deflation were very unorthodox in many countries (not in all), and there remained almost everywhere a legacy of high tariffs, tight quotas and many other restrictions on international trade and payments. But the point is that this was not necessary; deflation could have been stopped and the economy "re-flated"<sup>6</sup> in a more orderly, less unorthodox fashion with less structural change and without damage to the international division of labor. Even as it was, if in the meantime the threat of war had not arisen and the war had not come, surely a good part of the new trade barriers would have been removed and trading methods would have returned to a more normal state of affairs. Actually, the outbreak of World War II delayed the return to something resembling the pre-1929 conditions until the 1950's.

<sup>5</sup> On Keynes' criticism of the New Deal policies compare R. F. Harrod, *The Life of John Maynard Keynes*, New York, 1951, p. 447. Keynes was critical not of the reforms as such, but of the timing of their introduction and of the methods of putting them through, which, by frightening the businessman, unnecessarily delayed full recovery.

The information concerning Léon Blum I owe to Gunnar Myrdal.

<sup>6</sup> This does not mean that the price level would have to go back to the 1929 level.

Before going on to those recent developments, let us look back once more at the interwar period. As it recedes into the past and we gain perspective, it becomes increasingly clear, it seems to me, that the interwar or two-war period in general and the Great Depression in particular were in many important respects a singular historical phenomenon.

Let me become more concrete. I have already mentioned that the exceptional severity and length of the Great Depression were due to special circumstances, institutional weaknesses, and incredibly inept and timid policies on the national and international level. This has been misunderstood by the Marxians and Keynesians. The doctrine of secular stagnation, the widely held expectation that World War II would soon be followed by a deep depression—an expectation which was still entertained by many when the second and even when the third postwar recession came around—all these misjudgments as well as the theory of the stubborn dollar shortage and the low elasticity of international demand stem largely, if not wholly, from the misinterpretations of the depressed 1930's as a more or less regular business cycle downswing. A period of singular catastrophes plays havoc with trend extrapolations, and many an analysis has come to grief. Examples in the domestic field are the numerous estimates of saving propensities and investment opportunities for the postwar years on the basis of interwar experience and figures. Formidable difficulties arise for another type of analysis—namely that which assumes the existence of some sort of supercycle, be it Kondratieff or the now much more popular Kuznets cycle. It is difficult to see how an endogenous mechanism producing such swings or cycles could fail to be completely disrupted by the elemental forces of a world war or the Great Depression.<sup>7</sup> In the international field, the famous conjecture that the terms of trade have a secular tendency to deteriorate either for the less developed countries or for primary producers is to a large extent the consequence of not recognizing the interwar period with its two very sharp price declines—in 1920-21 and 1929-33—as a singular event. In the light of later developments, this conjecture has lost its statistical support entirely.<sup>8</sup> We shall encounter other cases of analyses that have gone astray for the same reason.

<sup>7</sup> It is possible by means of population or investment echos or some financial mechanism to make a plausible case for the proposition that a tendency for long swings is started by those powerful disturbances. But the attempt to carry the chronology of the long swings right through the disturbed period of 1914-45 and thereby to suggest that the mechanism, whatever it is, which produces the long swings should function more or less undamaged right through that period, punctured though it was by tremendously powerful exogenous shocks, seems to me entirely unconvincing and to compromise the long-swing hypothesis.

<sup>8</sup> The latest extremely careful evaluation is contained in Robert E. Lipsey's important book [11].

### V. *The Growth of World Trade Since the War: The Case of the Industrial Countries*

I now come to the third wave of world-wide integration or reintegration during the postwar period.

Since the end of the war, especially since 1948, world trade has grown very rapidly. World exports rose from some \$54 billion in 1948 to over \$140 billion in mid-1963, and for the decade 1950-60 the compound rate of growth of the volume of world exports was about 6 per cent a year.<sup>9</sup> It seems that for the first time in almost a hundred years world trade has grown faster than world production for a period of more than ten years. I mention this fact for two reasons, first, to convey an idea of the rapidity of the growth and, second, because so much has been made of the alleged fact that since the late nineteenth century international trade has in most countries grown less fast than national income. You will remember that years ago Werner Sombart tried to establish a "historical law" of the "declining importance of international trade."<sup>10</sup> This alleged tendency has held a strange fascination for many economists.<sup>11</sup> But I myself cannot see much importance in this particular criterion. Whether trade has grown somewhat more or less fast than GNP is hardly very significant unless the divergence is very pronounced as it was in the 1930's. Here again the contours of the trend have been blurred by the singular events of the Great Depression.<sup>12</sup>

The rapid rise in world trade is the consequence of, but has also powerfully contributed to, the rapid growth of world production. The performance with respect to growth and stability of the majority of national economies of the world has, on the whole, been satisfactory—not in an absolute sense of perfection, but satisfactory compared first with earlier periods (not only the definitely unsatisfactory interwar period), compared secondly with what was expected by many economists 20 years ago, and compared thirdly, I dare say, with what one

<sup>9</sup> In average of 1955-60 prices. See [4] [22].

<sup>10</sup> "*Die These von der abnehmenden Bedeutung der internationalen Handelsbeziehungen*" or "*der fallenden Exportquote*," Werner Sombart in [20, pp. 368-71].

<sup>11</sup> See [16] for a good, brief discussion and references to the recent literature.

<sup>12</sup> Lipsey [11, p. 44] found that as far as the United States is concerned "over the whole period [1879-1960] the only suggestion of a downward trend in the ratio of the quantity of trade to output was the low interwar export and postwar import ratios. Both now appear to have been temporary." Lipsey reached his results by using constant dollar rather than current value trade figures. The difference between current and deflated dollar value ratios of trade and output stems from the fact that both export and import prices have fallen in the long run compared with domestic prices. Hence a different deflator has to be used for the two items of the ratio. If "invisible" exports and imports could be added to merchandise trade, the statistical support for the theory of the declining trade-GNP ratio would be further weakened.

would gather from current statements of many experts, not to mention politicians, statesmen, and other laymen.

Let me first discuss the case of the highly developed industrial countries. As far as Continental Europe, Japan, and Australia are concerned, my optimistic statement will probably not be questioned. These countries can indeed look back to a decade and a half of almost unprecedented and unbroken growth and prosperity, which has definitely gone beyond the stage that could be reasonably regarded merely as recovery from the low levels reached at the end of the war. But I go on to say that an objective evaluation shows that, contrary to what is often said or implied, the over-all performance of the American economy, too, has been quite satisfactory in the sense defined above. This holds for the whole postwar period including the last five or six years of slightly slower growth and somewhat higher unemployment of which we have heard so much in recent years. True, the U.S. rate of growth has been much lower than that of Europe and Japan, but it has been somewhat higher, over all and per capita, than it was, not only during the whole interwar period, but for any period of similar length (leaving out the Great Depression) since 1909 when reliable deflated GNP figures begin. In the nineteenth century, aggregate GNP grew faster, but as far as we can tell per head or per man-hour growth was slower than during the postwar period.

The great improvement of the postwar years over earlier periods is, of course, the complete absence of deep depressions. This is common knowledge and it is, I think, fairly generally agreed now that there are excellent reasons for assuming that deep depressions are a thing of the past. I share this conviction and shall not repeat the well-known argument. But let us recall that this consensus, which now seems to extend even to Marxist economists, did not exist or was at least much less widespread only ten years ago.

But how about the last five years of somewhat slower growth and higher unemployment? The critics of our economic system call it semi-depression, just about the best one can expect under a capitalist free enterprise regime. Many others agree with the diagnosis of an intolerable stagnation, but think that it could be cured by bold expansionary measures. Some think that monetary and fiscal expansion alone would not be enough, that drastic structural reforms (retraining of workers on a massive scale, redistribution of income, and others) would be required to get the economy back to a high level of employment and to keep it there.<sup>13</sup>

To me any comparison with the 1930's, quantitative or qualitative, seems to be entirely unwarranted, and structural factors explain only

<sup>13</sup> The most extreme position has been taken by Gunnar Myrdal in [14].

a small fraction of the problem. The explanation of the slack is really quite simple: It is the consequence, directly and indirectly via somewhat tighter monetary and fiscal policies than otherwise would be necessary, of the deficit in the balance of payments.<sup>13a</sup>

To put the matter in the simplest terms, suppose the deficit in the balance of payments disappeared all by itself—as it well may through continued inflation abroad—can anyone doubt that our economy would expand substantially, partly from rising exports and import substitution, partly in response to easier money and fiscal policies which then would become possible?

It is really as simple as that although, looking at the size of the deficit compared with GNP itself or with its annual increases, it may sound astonishing that such a tiny tail should wag such a huge dog. But let me get back to my main theme by brushing aside a mountain of real and imaginary difficulties and controversies with the remark that it cannot be beyond the wit of men—that is, us economists—to get this tiny tail removed, with or without the surgery of a change in exchange rates, and without in the process restricting trade in goods and services or reducing growth itself below the otherwise attainable level.<sup>14</sup>

An indispensable condition for the rapid recovery and growth of the industrial countries and the expansion of world trade was the removal of the jungle of internal and external direct controls that had grown up in many countries during the depression and the war. Price controls, consumer rationing, allocation of raw materials, etc. were removed quickly in some countries such as Germany and more gradually elsewhere. By the middle 1950's the process was substantially finished almost everywhere and the price mechanism had again taken the place of the wartime system of direct controls. The liberation of the economies of the industrial countries in Europe and elsewhere from the shackles of direct control released great energies which led to the spectacular rise in output and consumer satisfaction. I add "consumer's satisfaction" because improvements in quality of products and a great

<sup>13a</sup> William McC. Martin, Jr., Chairman of the Board of Governors of the Federal Reserve System, has acknowledged the role of the balance of payments. "I am convinced that our failure to solve the problem [of the balance of payments] up to now has not only been damaging to our international relations but has also impeded the achievement of even high levels of output and resource utilization" (statement before the House Committee on Banking and Currency, July 22, 1963, *Fed. Reserve Bull.*, Aug. 1963, p. 1062).

Since the slack is small, it cannot be far off the mark to say that the external deficit has been the major cause of the slowdown.

<sup>14</sup> I do not wish to say, of course, that in the absence of a balance-of-payments deficit there will never again be a recession or a period of slack. For example, if at a higher level of employment wage-push became stronger and prices started to creep up again, the monetary and financial brakes would sooner or later be applied as they were in the late 1950's with the same general result. But demand inflation, wage-push, Phillips curves, income policy, and all that are not the subject of this talk.

increase in the variety of products (including services such as increased opportunity to travel) surely are factors adding greatly to economic welfare while finding inadequate expression in output figures.

In the international sphere the replacement of direct controls by the price mechanism was much slower and in some countries less complete than in the domestic area. But by 1959 the currencies of all industrial countries were again more or less convertible; in other words, exchange control was dismantled although restrictions on capital movements still exist in many countries. Moreover, import quotas were gradually removed, first in intra-European trade, but then largely also on dollar imports and later, although still incompletely, on imports from Japan.

This freeing of trade constitutes a movement towards world-wide integration that has preceded and overlapped the regional reduction of trade barriers and regional integration in the European Common Market and other similar schemes of which we hear so much. There can be hardly a doubt that, up to now, the quantitative effects on trade of the world-wide integration and liberalization have been much greater than those of the much more discussed and advertised regional schemes.

#### VI. *The Growth of World Trade and the Less Developed Countries*

Now to the controversial and extremely important question of the participation of the less developed countries in the over-all expansion of world trade. From a careful study of postwar facts and figures, the conclusion emerges, I believe, that the less developed countries have greatly benefited from the expansion of world trade and that the prosperity in "the industrial centers" has spread to "the less developed periphery."

This result is not surprising; both classical and Keynesian theory, speaking with complete harmony on this matter, lead to the strong presumption, though not the absolute certainty, that the rapid growth of the industrial countries will benefit the less developed countries by increasing the demand for their exports, by making a more liberal supply of capital and aid funds possible and, taking a broader and longer view, by developing numerous new technologies and products, many of which are extremely useful for the less developed countries. It is nevertheless necessary to dwell on the matter and carefully to establish the meaning, validity, and implications of the above statement because it runs counter to widely held pessimistic views. The extreme form of these heretical doctrines—heretical to the classical as well as the Keynesian theory—is represented by the transference to the international sphere of the Marxian theory of "increasing misery" (*Verelendungstheorie*). Since it really makes no sense any more to say that the American, European, or Japanese workers are getting poorer all the time, modern

Marxists have given up this theory for the developed countries. But it has survived in the international sphere. Thus Paul Sweezy admits that in the developed countries workers have a "tolerable if degraded life"; but the advanced countries "increasingly impose the burdens on the people of the colonies and the raw-material-producing countries" [21, p. 221]. A somewhat less extreme theory has been put forward by non-Marxist writers. Thus Gunnar Myrdal holds that "trade operates (as a rule) with a fundamental bias in favor of the richer and progressive regions (and countries) and in disfavor of the less developed countries." His thesis is not only that the poor countries derive less benefits from trade than the rich, but that, in the absence of strong protective policies, the poor become poorer if and because the rich get richer.<sup>15</sup>

Baldly stated like that, these extreme views are not widely accepted, but their influence in coloring the general thinking on these problems, especially in the less developed countries, should not be underrated. However, since they have been adequately criticized in the literature, I shall not discuss them further [13] [2].

The moderate version of the pessimistic view concerning the international position of the underdeveloped countries asserts that these countries have had no, or only a very inadequate, share in the expansion of world trade and that their international position has deteriorated continuously for the last ten years as evidenced by the lagging volume of exports and unfavorable terms of trade.

Looking at the broad facts, we find indeed that while the volume of exports and imports of the developed countries has grown at an annual rate of about 7 per cent from 1950 to 1960, the volume of exports of the "developing countries" (the official U.N. designation of less developed countries) has grown by 3.6 per cent and that of their imports by 4.6 per cent during the same period.<sup>16</sup> During the same period, the terms of trade of the developing countries in the aggregate have deteriorated by about 9 per cent as a consequence of a rise of about 10 per cent in unit value of the imports of the developing countries and a slight rise of unit value of their exports.<sup>17</sup>

<sup>15</sup> See [15]. Myrdal's policy conclusions are, of course, not the same as the Marxists'.

<sup>16</sup> See [22, p. 1]. Developed countries are North America, Western Europe, Australia, Japan, New Zealand, and South Africa. Developing countries are the rest of the world, excluding the "centrally planned economies" (Communist countries) in Europe and Asia.

<sup>17</sup> Measured from the peak in 1951 caused by the Korean War boom and massive U.S. stockpiling, the deterioration in the terms of trade would be substantially greater—14 or 16 per cent. The fact that the deterioration reflects a rise in import prices rather than a fall in export prices is significant because unit values of manufactured goods have a notorious upward bias, due to the fact that the gradual improvements in quality and the continuous appearance of new manufactured products are not, or very insufficiently, taken into account. In other words, a rise in import prices (of manufactured goods) is much less "real" than a fall of export prices (of primary products) would be.

To speak of developing countries in the aggregate—or, what comes to much the same thing, of a “typical” or “representative” underdeveloped country—is, of course, a bold abstraction from which purists will recoil in horror. The less developed countries are a much more heterogeneous group than the industrial countries. Among them there are semideveloped and wholly undeveloped countries; tropical and temperate zone agricultural countries; petroleum, mining, and agricultural product exporters; many pursue inflationary policies of varying degrees of severity, others are more disciplined; a few have convertible currencies but the majority suffer from overvalued currencies and their trade is restricted and distorted by controls and multiple currency practices of varying impenetrability.

In view of this great diversity of basic structures and policies, it is not surprising that the trade and growth performance of the less developed countries is very widely scattered. To search for systematic connections between the latter—performance—and the former—basic structures and policies—would be a challenging task but cannot be undertaken in one lecture. It would seem possible, however, to draw some useful conclusions from the over-all picture.

The most important fact is that the volume of trade of the less developed countries has grown and is growing at a substantial rate. The more rapid growth of the volume of imports than that of exports reflects the fact that these countries have been able to attract capital and have received substantial aid from the industrial countries.

This on the whole favorable picture is not changed by the fact that trade of, and between, the industrial countries has grown faster. The more rapid growth of the trade of industrial countries is not a new phenomenon. Even before 1914 it has often been observed, especially by free trade economists with obvious satisfaction, that “the industrial countries are their own best customers.” But during the Great Depression trade in manufactured products (which can be taken as a rough index of trade of developed countries) contracted more sharply than the volume of trade in primary products.

There are, however, good reasons to believe, as Professor Samuelson recently reminded us [17, p. 48], that lower total volume of trade between the developed and underdeveloped countries may bring more material benefits and consumer satisfaction to both partners than the larger volume of trade between the developed countries. The reason is that the intramarginal cost differences underlying the trade of manufactures between industrial countries are probably much smaller than those of which the trade between industrial countries and the exporters of primary products is based.<sup>18</sup>

<sup>18</sup> Alfred Marshall has alluded to that; Richard Schueller in [18] has commented on this

There is another factor involved. One of the reasons for the lower rate of growth of the trade of the less developed countries is surely that practically all of them pursue highly protectionist policies. In my opinion, they are going in their protectionism far beyond what can possibly be justified on infant-industry, terms-of-trade, or any other rational grounds. But whether one accepts this judgment or not, high protection reduces the volume of trade<sup>19</sup> and by the same token increases the *marginal* benefit from trade.

As for the terms of trade, the deterioration from the early 1950's is, of course, regrettable in the sense that the less developed countries would be better off if in 1960 their terms of trade were the same as in 1950. But we must keep a sense of proportion. The deterioration was not catastrophic and cannot be compared with what happened in the 1930's. The comparative order of magnitude is indicated by the fact that while, according to the U.N., from 1950 to 1960 the deterioration was 9 per cent, Kindleberger found that in 1938 the terms of trade of industrial Europe with a large part of the underdeveloped world stood at 149, with 1928 = 100 [22] [6].<sup>20</sup> To convey an idea of the absolute magnitude of the "burden" or "loss" involved, let me mention that the U.N. report computes that "had the terms of trade of the less developed countries been stabilized at their 1950 level the aggregate purchasing power of their exports in terms of imports in 1960 would have been greater to the extent of \$2.3 billion." This is not a negligible sum, but compared with the national income of the less developed areas of the

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fact. See also D. H. Robertson [16]. It should be observed, however, that freer trade between the industrial countries, although it may be less valuable than trade between industrial and tropical countries on comparative cost grounds, can be beneficial through stimulating competition and counteracting monopolistic tendencies. This effect of free trade has been stressed by J. S. Mill and reiterated by modern theorists of integration, e.g., by Tibor Scitovsky and Harry Johnson.

<sup>19</sup> In underdeveloped countries the theory has been widely accepted that in less developed countries import restrictions do not reduce the volume of imports or exports; they merely cause a shift of imports from, say, luxury goods to capital goods. In the developed industrial countries, it is said, the classical rule still applies that import restrictions reduce the volume of trade in both directions. This is the official doctrine of the U.N. Economic Commission for Latin America (ECLA). See, e.g., [23, esp. p. 61]. The reason usually given is that these countries spend all their export proceeds anyway. This, of course, overlooks that protection draws factors of production into the protected industries from the export sector. Only under extreme and entirely unrealistic assumptions concerning the existence of large masses of disguised unemployment (including that of skilled workers), of surplus capacity, and very elastic capital supply could the conclusion be overthrown that export supply will contract as a consequence of massive import substitution brought about by heavy protection.

<sup>20</sup> Kindleberger's figures are not quite comparable with the U.N. figures because his area breakdown is a different one. The figure quoted in the text refers to industrial Europe's terms of trade with "all other areas," i.e., the world excluding the rest of Europe, the United States, and the "countries of recent settlement." With the last-mentioned area the index of industrial Europe's terms of trade stood at 138 in 1938 (1928 = 100).

world or its annual increase it is quite small.<sup>21</sup> The incidence of the 9 per cent over-all deterioration of the terms of trade on different areas was, of course, very uneven. Latin America, for example, suffered a deterioration almost double the average, the Middle East none at all, and the rest of Asia (excluding Japan) one much smaller than the average. But even in the area hardest hit—Latin America—the “importing power of exports” (volume of exports multiplied by terms of trade, also called “capacity to import”) has substantially increased throughout the period of 1950 to 1960 [4, p. 61]. As has frequently happened in the past, the impact of the change in the terms of trade on the primary exporters was partly offset by a sharp decline in freight rates which were very high in the early 1950's.<sup>22</sup> Since the middle of 1962, the international trade situation of the less developed countries has improved greatly, prices of many primary products have risen substantially, the terms of trade did not show any further deterioration, and export earnings have registered sizable increases.

Nobody can be sure how long that improvement will continue and how far it will go. But assuming that the industrial countries avoid serious depressions and maintain a good rate of growth, barring further revolutionary technological changes (such as the invention of cheap synthetic coffee) and highly protectionist policies on the part of the industrial countries—under these not unreasonable assumptions the less developed countries can look forward to a continuously expanding market

<sup>21</sup> Some may question the relevance of a comparison with national income. In my opinion, it is relevant with certain qualifications: A large and sudden change in the terms of trade might cause serious transitional difficulties. Or, if a country by extreme protectionism has reduced its imports to a bare minimum and has neglected its export industries, a decline in the terms of trade may necessitate a further reduction of imports which have already been stripped to essentials. In that case the impact on GNP would be out of proportion to the money cost of the deterioration quoted in the text because the “marginal benefit” of imports would be much higher than the marginal benefit of domestic production. If my income falls by 5 per cent and I were forced to skimp on food rather than spread the shortfall over the whole range of expenditures, the result would be disastrous. But I doubt whether any country really finds itself in such a predicament, although extreme protectionism could conceivably put it there.

<sup>22</sup> Charles Kindleberger has pointed out to me that a veritable revolution has taken place in ocean transportation. Thus, while the world wholesale price index in dollars has about doubled since before the war, oil tanker rates are just about where they were, and dry cargo rates have gone up on a very rough average by 50 per cent. This great cheapening of transport cost constitutes a powerful integrating force in the world economy and has bestowed great benefits, especially on many less developed countries, by improving their f.o.b. terms of trade.

The drastic reduction in transport cost, made possible by technological advances, applies primarily to the very competitive international routes. Many countries rob themselves of those very large benefits by highly protectionist shipping policies. This is especially true of coastal shipping, including sometimes shipping between neighboring countries. For example, freight rates between Rio de Janeiro and Buenos Aires are often as high as between Buenos Aires and New York, because of nationalistic policies. See [8, p. 297].

The jet aircraft promises to produce another revolutionary advance in transport technology.

for their exports, and a sharp deterioration of their terms of trade comparable to what happened in the 1930's is virtually excluded.

I should add that this fairly optimistic appraisal relates only to the international trade position of the less developed countries.<sup>28</sup> Far be it from me to belittle the severe handicaps from which many less developed countries suffer, such as poor natural resources and bad climate, population pressure, political and social instability, and, I am afraid one has to add, exceedingly bad economic policies. All I have said was that a shrinking or stagnant world market has not been among the handicaps since the war.

### VII. *Outlook for the Future*

Instead of summarizing what I have said, let me try to peer a little into the future. Since economists are not endowed with the gift of prophecy, I shall not try to predict what will happen to world trade in the future but do the next best thing and suggest what would be the course of events if certain conditions are fulfilled.

I see no reason why the the growth of world trade and the ever closer integration of the major areas of the world should not continue if two conditions are met. The first, basic requirement is maintenance of a high level of employment and growth in the industrial countries. On this nothing more need be added, except to say that to me the chances look good that this condition will be fulfilled. But it is a necessary, not a sufficient, condition. At least in the larger countries economic activity could grow quite satisfactorily (although not at the highest attainable speed) without a corresponding growth of trade.

The second presupposition is further liberalization of trade or, as a minimum, avoidance of increases in trade barriers for balance-of-payments or protectionist reasons. I am afraid we cannot be so sure about this condition being met as about the first. Balance-of-payments

<sup>28</sup> W. Arthur Lewis has reached similar conclusions in two important papers [9] [10]. Speaking of Latin America, he said: "Puzzling are those cries which seem to be founded on the belief that it is particularly difficult to expand exports because the world is buying fewer and fewer exports. The opposite is true. World trade has never grown faster. Between 1950 and 1960 the quantum of world trade in primary products increased at an average rate of 6 per cent per annum, and the quantum of world trade in manufacturers by more than 7 per cent per annum. The terms of trade for primary products could not retain the heights to which they were raised by the speculative fever of the Korean War and the heavy American stockpiling in the early fifties; nevertheless the average terms of trade for the decade of the nineteen-fifties were better than for any previous decade in all the preceding hundred years. I do not know whether it is in fact true that in the 1950's, Latin America had difficulty in keeping her exports growing at the same rate as national income but if this was so, it cannot possibly have been due to failure of world demand to grow adequately, since the quantum of world trade was growing by about 7 per cent per annum throughout the 1950's. Taking the continent as a whole, rather than individual countries, failure on this score can only have been a failure of effort" [10, pp. 10-11].

troubles among industrial countries could easily become a serious road-block to freer trade, as is already the case to some extent in the United States through the tying of loans, buy-American policies, and similar protectionist devices. Paradoxically, attempts at regional integration in various parts of the world constitute an imminent danger to world-wide integration and further growth of multilateral trade.<sup>24</sup>

As to the balance of payments, I must confine myself to a few remarks. The current American difficulties will somehow be resolved, but will hardly remain the last case of serious disequilibrium among the industrial countries. With the rate of growth slowing down and inflation going on in some countries, with anti-inflationary and growth-promoting measures being taken in different countries with different degrees of vigor and success, it would be very surprising indeed if strains in the international payments system did not develop from time to time in different places.

The extreme reluctance to change any exchange rate and the stubborn rejection of floating rates not only for reserve currencies, which is understandable, but also for nonreserve currencies throw a very heavy burden on internal price and wage adjustments. The successful operation of the present system requires, if world-wide inflation is to be avoided, a degree of financial and wage discipline which few countries have so far been able to muster.<sup>25</sup> But the problem is not insoluble and, since the imposition of financial and wage discipline is, after all, the most important advantage that can be claimed for the system of stable exchange rates, we can only hope that sufficient discipline will in fact be forthcoming to bring about the necessary adjustments smoothly and speedily without causing severe restrictions of trade or serious unemployment in the process.

I suppose I have to mention international liquidity. I can only say that it does not strike me as a problem of major importance or difficulty. The policy-makers have a great number of plans to choose from—Bernstein, Posthuma, Roosa, Triffin, Zolotas, to mention only a few luminaries. Even the more modest ones would banish the danger of a general lack of liquidity, at least for many years. I wish, therefore, that a large part of the professional ingenuity that is now being spent so lavishly on that issue were diverted to the basic and much more difficult problems of internal price and wage restraints and adjustments.

<sup>24</sup> Another acute danger, which cannot be discussed here, is regimentation, contraction, and distortion of trade through the numerous international price-support schemes of important commodities now under active consideration.

<sup>25</sup> I would agree that a floating rate system, too, requires price and wage discipline for successful operation. But I would argue that the fixed rate system calls for much sterner discipline.

The regional integration schemes that are now in the process of realization have developed strongly protectionist features which go far beyond their unavoidable discriminatory effects. This is true of the European Common Market and even more so of its Latin American counterpart. In fact, as far as the latter is concerned, it is an understatement to say that protectionist tendencies have appeared. The Latin American Free Trade Area (LAFTA) has been conceived openly and frankly as a protectionist device from its beginning.<sup>24</sup> On the other hand, many of the managers and supporters of EEC inside and outside the six members sincerely regard the Common Market as a step towards freer trade. But good intentions even on the part of the managers are not enough. An international enterprise of that magnitude, once it has been launched with a tremendous effort and enthusiasm and has been institutionalized through the creation of a heavy politico-bureaucratic machinery, tends to develop its own logic and movement. After it has gathered momentum, it becomes very difficult to steer it in another direction. This has been discovered, to their sorrow, by the vocal and vigorous forces which, working from within and from without EEC, have been trying to persuade the Community to adopt an "outward looking attitude," as the phrase goes. A crucial test as to whether or not it can be made to move in the direction of freer trade will come next year when the negotiations begin in earnest on the so-called Kennedy-round of tariff reductions in Geneva.

Let us hope that this noble experiment will succeed, that tariffs between the industrial countries and on imports from the less developed countries will be drastically reduced. Then the future economic historian will be able to say of the European Economic Community what Marshall said of the German *Zollverein*—it was a big step toward freer multilateral trade.

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<sup>24</sup> This does not necessarily mean that it is undesirable. There are valid arguments for protection which theoretically can be applied to customs unions or free trade areas. In the case of LAFTA, the protectionist effects are, however, so strong that it would be very difficult to make a rational case for it on, say, infant-industry grounds. It could conceivably be defended with the help of a theory of the third or fourth best. Let me briefly indicate what I have in mind. By means of a policy of partly repressed inflation, tight exchange control, etc. a country can maneuver itself into a position where trade is so sharply contracted and distorted that even bilateral trade is better than no trade. Similarly, a regional trade expansion may be better than no expansion at all, although a relaxation of controls and general expansion of trade would be infinitely better.

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## RULES OF THUMB AND OPTIMALLY IMPERFECT DECISIONS

By WILLIAM J. BAUMOL AND RICHARD E. QUANDT\*

It is easy to jump to the conclusion that the widespread use of rules of thumb is good evidence of sloppy workmanship on the part of business management. We shall argue in this paper that, on the contrary, rules of thumb are among the more efficient pieces of equipment of optimal decision-making. We shall therefore investigate in fairly general terms how "good" rules of thumb can be designed and evaluated. The paper also reports on an illustrative application of the analysis. A simulation procedure is used to determine the relevant properties of a number of alternative pricing rules of thumb and to compare their performance as guides to decision-making in a single-product monopolistic firm.

### I. *Optimally Imperfect Decisions*

The more refined the decision-making process, the more expensive it is likely to be, and therefore, especially where a decision is not of crucial importance, no more than an approximate solution may be justified. Since all real decisions are made under conditions of imperfect information, calculation down to the last decimal place is pointless in any event. One can easily formulate the appropriate (though not very helpful) marginal condition for what one may call an *optimally imperfect decision*, which requires that the marginal cost of additional information gathering or more refined calculation be equal to its marginal (expected) gross yield. The expenditure of any further effort in this direction can be ascribed only to miscalculation of its returns or its costs or to what one economist has happily described as "an irrational passion for dispassionate rationality."

There seems to be a close affinity between this position and Professor Simon's view that the businessman's goal can be better described as "satisficing" than as some sort of maximization. Simon appears to be asserting, in effect, that businessmen usually do not even attempt to

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find maximal solutions<sup>1</sup> precisely because they recognize the limited accuracy of their information and excessive costs of persnickety calculation, and that, instead, they are well satisfied with viable solutions to their problems.<sup>2</sup>

For the purposes of this paper we define a rule of thumb to be a set of rules describing a decision procedure with the following characteristics:

(a) The variables which are employed in the decision criteria are objectively measurable.

(b) The decision criteria are objectively communicable, and decisions do not depend on the judgment of individual decision-makers.

(c) As a corollary to (b), every logically possible configuration of variables corresponds to a (usually unique) determinate decision.

(d) The calculation of the appropriate decision is simple, inexpensive, and well suited for frequent repetition and for spot checking by management in higher echelons.

A decision process with these characteristics seems to be designed as the instrument par excellence of optimally imperfect decision-making for routine and recurrent problems.<sup>3</sup>

## II. *Optimal Rules of Thumb: Definitional Problems*

Even conceptually, the ranking of rules of thumb is not as easy as one might think because problems of multidimensionality are inherently involved.

It may at first glance seem sufficient to say that, of two rules which are equally costly to operate, the one which yields the closer *average* approximation to the true maximum is to be preferred. But some measure of the dispersion of the results is, surely, also of comparable relevance. In fact, all the characteristics of the frequency distribution of errors are significant. The "best" rule of thumb can only be determined by taking all of these characteristics into account and assigning appropriate weights to them.<sup>4</sup>

<sup>1</sup> For our purposes it is convenient to distinguish between a *maximal* and an *optimal* solution. The former term shall be used throughout this paper to connote the exact solution which would be obtained if there were no data limitations or calculation costs, while the term "optimal solution" refers to the ideal approximation to the maximal solution.

<sup>2</sup> See e.g., Herbert Simon [5]. Simon is, of course, saying considerably more than this—he is proposing the hypothesis that businessmen, at least implicitly, set up a number of criteria of satisfactory performance and are willing to accept any decision that passes these tests. It is, as it were, constrained maximization with only constraints and no maximization!

<sup>3</sup> Note that the operations researcher's strategy is usually to replace his client's current rules with some others which he considers more satisfactory, *but which remain rules of thumb nevertheless*.

Moreover, even in computer simulation models, where the world is considerably simplified, and information gathering and refined calculations are relatively cheap and easy, it has in many cases proved impossible even for the sophisticated designers of the models to have their "firms" operate by means other than rules of thumb.

<sup>4</sup> The assignment of weights is, of course, equivalent to determination of an appropriate utility function.

It would appear to follow that there is no cut and dried mechanical way of determining which of two rules is the more satisfactory unless one of them is what might be called "Pareto preferable," that is, it is not inferior in any of its relevant characteristics and superior at least in some.<sup>5</sup>

Another difficult problem, intrinsic to the search for an optimal rule of thumb, arises out of the need to specify the range of alternatives to be tested. In other types of decision this task is sometimes fairly simple, as in a decision problem consisting in a choice of the (numerical) value of a variable, where one need at most specify the end points of the range to be considered.

But in selecting a rule of thumb we are to choose not a number but a functional relationship. For example, inventory might be regulated either by a usual business rule of thumb,  $I = kS$ , or by the simplest inventory theoretic rule,  $I = \sqrt{aS}$  (where  $I$  is the level of inventory,  $S$  is sales volume per unit of time, and  $k$  and  $a$  are constants), or it can be determined by any of a variety of alternative simple expressions. Unfortunately, there is no obvious way to supply a reasonably representative list, let alone an exhaustive list of such possibilities.<sup>6</sup>

### III. Calculation Problems

A crucial step in the determination of an optimal rule of thumb is the calculation of the expected consequences of the adoption of any particular rule of thumb. Ideally, this step should produce a frequency function of the errors to be expected of any such rule of thumb in its approximation to true maximal solutions.

In principle, it may be possible to determine these frequency functions analytically if the structure of the system and the probability distributions of the structural variables are known. However, in practice, such a procedure is almost always impossible. First, an a priori specification of probability distributions for the structural variables usually represents most heroic assumptions that rarely stand up under comparison with the facts. Secondly, even under these conditions, analytic methods of determination of the expected effects of a rule of thumb are likely to be prohibitively difficult.

This means that one must turn to some alternative methods of testing and calculation. The methods of simulation using artificial data and of "dry run" testing on the basis of past history seem well suited to

<sup>5</sup> It is worth noting that the possibility of intransitivity is one of the unpleasant consequences of the multidimensional character of the comparison of several rules of thumb. This is entirely analogous with the paradox of voting, and can arise whenever the choice of rule of thumb is decided by determining which of the candidate rules has a majority of preferred characteristics. For a discussion of the paradox of voting see Arrow [1, pp. 2-3].

<sup>6</sup> This observation also indicates a difficulty inherent in the optimality calculation. Because, in selecting a rule of thumb, one chooses a function rather than a value of a variable, the problem falls within the difficult territory of the theory of functionals.

the problem. By considering a sufficient number of simulated cases the analyst may be able to come away with a fair idea of the rule's likely performance. Unfortunately, invented case histories are apt to be very poor approximations to reality or they may constitute a biased selection out of the set of possibilities. Moreover, to test how well the rule of thumb performs, one must be able to compare it with the true maximal solution in the selected cases. In models that are somewhat complicated this is not always possible.

The alternative to simulation is a procedure for testing the rule with the aid of a number of cases appropriately selected from actual past experience. This procedure reduces the danger of selection of artificial and excessively simple or unrepresentative test cases, but it is perhaps even more likely than is a simulation to result in difficulties in computing the true maximum.<sup>7</sup>

#### IV. *Approaches to an Optimality Analysis*

Though, as emphasized in Section II, there exists no systematic method for specifying all the rules of thumb which should be tested in any particular case, the matter is not completely hopeless. Many functions can be approximated fairly well, at least for limited ranges of the independent variables, by one of a small set of simple algebraic functions, such as equations of the conic sections, polynomials of reasonably low degree, logarithmic functions, and so on.

This approach, combined with a simulation or a statistical estimation and testing procedure, can sometimes be used to generate rules of thumb that may plausibly be expected to yield a fair degree of approximation to the true maximum. That is the basic method employed in this paper.<sup>8</sup>

Before turning to our application of this method, it is appropriate to mention an alternative and promising approach to the selection of rules of thumb which is based on the use of axiomatic methods.<sup>9</sup> In effect, this procedure gives up the search for a truly maximal solution. Rather, it specifies sets of acceptability conditions, in the form of axioms, which serve as constraints capable of narrowing down the range of possible

<sup>7</sup> Fortunately, these problems are not always very serious. For example, in one statistical study based on actual records of approximative rules for transportation-routing decision-making the calculations were relatively straightforward, and rather definitive conclusions were obtained as to which of the proposed rules gave the best performance. See Kuhn and Baumol [3].

<sup>8</sup> We have also used it in other connections. For example, in one unpublished inventory investigation, numerical methods were employed to determine the least-cost inventory levels for a number of alternative sales levels. It was then found that the exponential expression  $I = cS^b$  provided a very good fit to the results but for a value of  $b$  considerably different from the 0.5 exponent of the standard simplified inventory theory relationship  $I = \sqrt{aS}$ .

<sup>9</sup> Thus it is possible to interpret the Chernoff or the Milnor approaches to the decision problem as illustrations of rule-of-thumb selection. See Chernoff [2] and Milnor [4].

solutions.<sup>10</sup> For example, in setting up a reasonably simple decision rule of thumb it may be proposed that any such rule which does not yield a transitive relationship is completely unacceptable. By a sufficiently skillful selection of such a set of requirements it is sometimes even possible to arrive at a unique solution, which, if it is simple enough in form, may then be adopted as the desired rule of thumb.

### V. Designing a Price-Setting Rule

Let us turn now to an illustrative application of our proposed procedure, to one of the microeconomist's favorite subjects—the price setting of a retail firm. The performance of a number of alternative pricing rules of thumb was tested by a simulation process employing a considerable number of artificial randomly generated demand and cost functions.

In planning the analysis several alternative types of pricing rules were considered for testing:

(a) *Markup rules* which set price by means of a more or less fixed markup over wholesale cost, as is apparently done in a variety of retailing establishments.

(b) *Imitative rules* in which each firm seeks to keep its price at the level set by its competitor. Such rules would fit in with the standard kinked-demand-curve analysis and are compatible with such observed retailing institutions as comparison shopping.

(c) *Learning rules* whereby, if a firm raises its price and then finds its profits have increased, it raises price again; while, if its profits have fallen, in the next period it lowers its price.

(d) *Pseudo maximizing rules* in which simple demand and cost curves are crudely fitted by quick and inexpensive methods to recently observed data and, from these simple curves, an approximative profit function is derived and used to determine the price formula (rule of thumb) which maximizes the value of that approximative function.

Our analysis was confined to the last of these four categories, the pseudo maximizing rules of thumb. Our approach proved to be totally unsuited to analysis of the first two types of rule. Testing of fixed markup rules requires some knowledge of the relationship between wholesale cost (which is the basis for the markup price) and the operating costs of the retailer (which must affect the profit-maximizing price). If the two types of cost bear only a random relationship to one another,

<sup>10</sup> Note the resemblance of the entire approach to Simon's satisficing hypothesis (see [5]). Sometimes the acceptability constraints are so demanding that they eliminate the feasible region altogether, so that no solution can possibly satisfy the acceptability conditions. An excellent illustration is the Arrow possibility theorem [1, Ch. 5]. In other cases the acceptability conditions will not be sufficiently strong to reduce the set of possibilities to a single choice. In that case the axiomatic approach may still have to be supplemented by some sort of optimality calculation which chooses among the remaining possibilities.

it is clear that a fixed markup price would be randomly related to the maximizing price.<sup>11</sup> In reality, the two types of cost are, no doubt, closely interconnected, but there seems to be no way in which this relationship can be captured by the random-number cost-generating mechanism.

We did not study pricing rules of type b (imitative pricing) for similar reasons. Imitation implies the presence of someone to imitate, and since we were generating data for only one firm, and not dealing with any sort of explicit oligopoly relationships, there was no way in which such rules could be investigated.

Rules of type c (the learning rules) could have been tested. We did not investigate these by simulation techniques because we felt the calculations had already gone far enough for our present purpose—illustration of a method of approach. However, a few observations on the properties and testing of such learning rules are offered in Appendix A.

The last type of rule of thumb, the type discussed in the remainder of this paper, is what we have called the pseudo maximizing rule. Details of each rule depend on the character of the functions used as estimates of the cost and demand functions. We considered four possible sorts of approximating function: linear, quadratic, logarithmic, and exponential. Of course, various combinations are possible—the demand curve can be linear and the cost curve logarithmic, etc. Thus 16 possible combinations were examined (and a few others in addition). Most of these were quickly abandoned because they required computations too complex to permit them to qualify as rules of thumb, and we emerged with four possibilities to use in our test.<sup>12</sup> Two other rather arbitrary “naïve” pricing rules were set up as standards of minimal performance.

We imposed one rather strong condition on the construction of our rules in order to assure that they were sufficiently simple to qualify as rules of thumb: in no case was the decision-maker asked to take account of more than two points on his demand curve and two points on his cost curve in his calculations. Very clearly this (intentionally) restricted us to

<sup>11</sup> Thus, it may be argued that the randomly chosen price rule of thumb (Rule 2) which we did test is the best approximation to a fixed markup rule that our analysis can achieve! The reason for our inability to test a markup rule can be made clearer in the simple case where marginal and average cost are constant and hence equal to one another. By the standard marginal revenue expression,  $MR = p(1 - 1/\epsilon)$  where  $\epsilon$  is the elasticity of demand. Hence, if price,  $p$ , is optimal so that  $MR = MC = AC$ , we must have  $p(1 - 1/\epsilon) = AC$ . The optimal markup over average cost is thus  $1/(1 - 1/\epsilon) - 1 = 1/(\epsilon - 1)$ . This well-known result shows that the optimal markup is constant only if  $\epsilon$  is constant. Moreover, a fixed markup price will have a random relationship to an optimal price so long as  $\epsilon$  is a random number. Similar arguments hold when  $MC$  is not equal to  $AC$ .

<sup>12</sup> Several other cases might have qualified. For example, the linear cost function  $D + Eq$  and the demand function  $p = A - Bq^{k-1}$  can readily be shown to yield the maximum pricing rule  $p = (kA - A + E)/k$ .

the crudest sort of approximation procedures, for we wanted to see how far one can get with such extremely imperfect tools.

### VI. *Description of the Experiments: the Cost and Demand Functions*

Before describing the details of our experiments, it is convenient to summarize the basic methods that were used. First, a demand (average revenue) curve facing the entrepreneur and his total cost curve are generated by a random process. The resulting pair of demand and cost curves are regarded as the "true" curves, describing correctly the actual state of affairs. The entrepreneur is assumed to know only two points on each of these curves. He uses some rule of thumb to calculate from these two pairs of points the price he is to charge in the market. We can determine from the true demand and cost curves the profit-maximizing price and the maximum amount of profit as well as the entrepreneur's actual profit resulting from his rule-of-thumb price. The comparison of actual and maximum profit provides an estimate of the efficiency of the rule of thumb.

The detailed discussion of our basic experiments can conveniently be divided into three parts: (1) the methods that were used to generate demand and cost functions, (2) the rules of thumb that were employed, (3) the method that was used to obtain estimates of the performances of various rules of thumb.

1. *The demand (average revenue) function.* The hypothetical product we deal with is assumed to be such that the quantity demanded is zero when the price,  $p$ , is greater than or equal to 21. The demand function (as well as the cost function) is discrete. The quantity demanded is defined only for integral values of  $p$ . The demand function was obtained by requiring the graph to start off at the point with coordinates  $p=21$ ,  $q=0$  and then calculating *increments* in the quantity demanded corresponding to successive unit reductions in the price. The increments were selected in one of two ways:<sup>13</sup> (i) in Runs 1, 2, 3, 4, and 5 the increments are uniformly distributed over the integers 1, . . . , 64; (ii) in Run 6 the  $i^{\text{th}}$  increment is  $X_i + 2i$  where  $X_i$  is uniformly distributed over the integers 1, . . . , 64. Note that the total revenue function need not be concave everywhere, and that a finite amount is demanded when the price is zero.

<sup>13</sup> These two methods generate demand functions whose expected values are, respectively, linear and quadratic. That is, in Runs 1-5, with each unit increase in price the expected (average) decrease in demand is  $(1+2+\dots+64)/64=32.5$ . Hence the expected  $dq/dp$  is constant and the expected demand curve is linear. In Run 6 the expected sales increment when price is  $p=21-i$  is  $\Delta q=32.5+2i$ , so that we can take  $\Delta q/\Delta p=-32.5-2i$  (approx.), and hence the expected demand function generated in Run 6 is quasiquadratic. Similar interpretations hold for the cost functions that were used in the various runs. More information about the nature of these functions is given in Appendix B.

2. *The total cost function.* The demand function was obtained by associating with each price,  $p$ , a quantity  $q=f(p)$ . The cost function associates with each of these  $q$ 's a total cost figure  $C=g(q)$ . With zero output we associate a cost figure which is chosen from the uniform distribution over the integers 1, . . . , 64. Each successive cost figure (associated with the successively higher output levels) is obtained from the immediately preceding one by the addition of an increment. Depending on the run, the  $t^{\text{th}}$  increment is (i) chosen from the same distribution, (ii) a multiple of an increment chosen from the same distribution, or (iii) expressible as  $Y_t+8t$  where  $Y_t$  is chosen from the same distribution.

It is obvious that maximum profit and the optimal price are easily determined from these functions, simply by examining in turn, each of the 22 possible price levels ( $p=0$  to  $p=21$ ) and calculating, in each case, the associated profit figure. The six experiments are summarized in the table below, showing the manner in which the  $t^{\text{th}}$  increments for the demand and cost functions were generated. Letting  $X_t$  and  $Y_t$  be random variables distributed uniformly over the integers 1, . . . , 64, we have:

	Demand function increment	Cost function increment
Run 1	$X_t$	$Y_t$
2	$X_t$	$4Y_t$
3	$X_t$	$8Y_t$
4	$X_t$	$Y_t+8t$
5	$X_t$	$4Y_t+8t$
6	$X_t+2t$	$Y_t$

3. *Entrepreneurial estimates of the demand and cost functions.* For the purpose of calculating rule-of-thumb solutions to the profit-maximization problem we assumed that, on the basis of recent experience, the entrepreneur knew exactly two points on his demand function and two points (with the same abscissae as the points on the demand function) on his cost function. The entrepreneur was then assumed to calculate his pseudo optimal price on the basis of the information contained in these four points. It is to be noted that if the pseudo optimal price,  $\hat{p}$ , turned out to be fractional, it was rounded to the smallest integer greater than  $\hat{p}$ . The profit accruing to the entrepreneur under a particular rule of thumb was then calculated from the true demand and cost functions.

It is clear that the entrepreneurial estimates of the demand and cost functions will depend, in general, upon which two of many possible points on the demand and cost functions the entrepreneur is assumed to

know. In order to evaluate the *average or expected* effectiveness of a rule of thumb statistically, the experimenter must therefore present the entrepreneur, seriatim, with various alternative pairs of points and observe his behavior in each of these possible cases. The manner in which such pairs of points were selected for the purposes of the experiment is described in Section VIII.

### VII. *The Rules of Thumb*

The six rules of thumb which were investigated were chosen essentially on the basis of their (relative) simplicity. No attempt was made a priori to choose rules which would in some sense be highly reasonable. In particular, the first two of the rules which are about to be described may be considered totally arbitrary and unreasonable. These "naïve rules" were designed only to provide minimum standards of performance, in that any rule which does not provide results better than those offered by our naïve rules should, doubtless, be rejected out of hand.

(*Naïve*) *Rule 1. Fixed price.* Irrespective of the location of the two known points on the demand function and the cost function, charge a price  $p=11$ , which is the midpoint of the range of relevant positive prices. Strictly speaking, this is not even a rule of thumb under our definition because no decision machinery is provided whereby the decision-maker can actually select his unvarying price on the basis of objectively measurable data.

(*Naïve*) *Rule 2. Random price.* Choose a price from the uniform distribution over the integers 3, . . . , 17, irrespective of the two known points from the demand and cost functions.

*Rule 3. Linear demand, linear cost.* Fit to the two known points on the demand function the linear relation  $p=a-bq$ , and to the two points on the total cost function the linear relation  $c=d+eq$ . If the two pairs of points are given by  $(p_1, q_1)$ ,  $(p_2, q_2)$  and by  $(c_1, q_1)$ ,  $(c_2, q_2)$ , estimates of  $a$ ,  $b$ ,  $d$ , and  $e$  are obtained from the expressions

$$b = \frac{p_1 - p_2}{q_2 - q_1}$$

$$a = p_1 + bq_1$$

$$e = \frac{c_1 - c_2}{q_1 - q_2}$$

$$d = c_1 - eq_1.$$

Here, for example, the expressions for  $b$  and  $a$  are obtained directly by elimination from the two equations  $p_1 = a - bq_1$  and  $p_2 = a - bq_2$ . The entrepreneur is then assumed to calculate his pseudo optimal price

by maximizing profit on the basis of the estimated demand and cost functions. Profit is

$$\pi = aq - bq^2 - d - eq,$$

and maximizing,

$$\frac{d\pi}{dq} = a - 2bq - e = 0$$

so that

$$q = \frac{a - e}{2b}$$

and, from our demand equation,<sup>14</sup>

$$p = a - b \left( \frac{a - e}{2b} \right) = \frac{a + e}{2}.$$

By virtue of the method by which the true demand function is generated,  $b > 0$  and the second-order condition for a maximum is always satisfied.

*Rule 4. Linear demand, quadratic cost.* In Rule 4 the demand function was estimated in the same manner as in Rule 3. The cost relationship fitted was the quadratic function  $c = dq + eq^2$ . The coefficient estimates were given by

$$\hat{e} = \frac{c_1 q_2 - c_2 q_1}{q_1 q_2 (q_1 - q_2)}$$

$$\hat{d} = \frac{1}{q_1} [c_1 - \hat{e} q_1^2].$$

Maximizing profit

$$\pi = aq - bq^2 - dq - eq^2$$

yields the first-order condition

$$\frac{d\pi}{dq} = a - 2bq - d - 2eq = 0$$

<sup>14</sup> The clerk who actually makes the rule-of-thumb calculation does not have to make an explicit estimate of the demand and cost equations to determine price. From our next equation we have  $p = (a + e)/2$  and so, substituting the preceding estimating expressions for  $a$  and  $e$ , we obtain  $p = (p_1 q_2 - p_2 q_1 + c_2 - c_1) / 2(q_2 - q_1)$ . This equation permits calculation of price directly in terms of the observed prices, cost and sales levels. A similar observation applies to our other rules.

whereby

$$q = \frac{a - d}{2(b + e)}, \quad \text{and} \quad p = a - b \left( \frac{a - d}{2(b + e)} \right).$$

The second-order condition is

$$2(b + e) > 0.$$

We observed the following conventions:

(a) Where the second-order condition failed (which can happen since  $e$  may be negative), a price  $p=21$  was charged.<sup>15</sup>

(b) If the calculated optimal quantity was nonpositive, a price  $p=21$  was charged.

(c) Marginal cost was constrained to be positive for the entire relevant range of outputs.

*Rule 5. Linear demand, logarithmic cost.* In Rule 5 the demand curve was obtained as before. But this time the cost function was

$$c = dq + e \log (q + 1).$$

From the two given points on the cost function, the coefficients  $d$  and  $e$  were estimated by

$$\begin{aligned} e &= \frac{c_1 q_2 - c_2 q_1}{q_2 \log (q_1 + 1) - q_1 \log (q_2 + 1)} \\ d &= \frac{1}{q_1} [c_1 - e \log (q_1 + 1)]. \end{aligned}$$

Maximization of the profit function

$$\pi = aq - bq^2 - dq - e \log (q + 1)$$

yields the first-order condition

$$\frac{d\pi}{dq} = a - 2bq - d - \frac{e}{q + 1} = 0,$$

whence

$$q = \frac{(a - d - 2b) \pm \sqrt{(a - d - 2b)^2 + 8b(a - d - e)}}{4b}.$$

<sup>15</sup> This convention and the corresponding conventions for Rules 5 and 6 may plausibly be considered to have introduced some bias against these rules in our final evaluation. For with  $p=21$ , sales are zero and profits negative; presumably in cases where a rule breaks down in so obvious a manner even a moderately intelligent clerk could often do better than that, so that it is probably unfair to credit these rules with no more than a zero profit contribution in these cases.

The second-order condition is

$$-2b + \frac{e}{(q+1)^2} < 0.$$

Various problem possibilities arise in this calculation, and they were handled by the conventions indicated below:

(a) Double roots. If the single distinct  $q$  was negative, a price of 21 was charged. If the single distinct root was positive but the second-order condition was not satisfied, a price of 21 was charged, yielding zero sales.

(b) If roots were complex, a price of 21 was charged.

(c) If the roots were real and distinct, a price of 21 was charged either if both roots were negative or if the single positive root corresponded to a minimum.

If a positive  $q^*$ , corresponding to a maximum of the profit function, existed, the entrepreneur charged the price  $p = a - bq^*$ .

*Rule 6. Hyperbolic demand, quadratic cost.* In Rule 6 the demand function is approximated by  $p = a - b/q$ , and its coefficients were estimated from the two "observed" points by

$$\hat{b} = - \frac{p_1 - p_2}{q_2 - q_1} q_1 q_2$$

and

$$\hat{a} = p_1 + \frac{\hat{b}}{q_1}.$$

The cost function was estimated as under Rule 4.

The profit function now is

$$\pi = aq - b - dq - eq^2$$

and, maximizing, we obtain

$$\frac{d\pi}{dq} = a - d - 2eq = 0,$$

yielding

$$q = \frac{a - d}{2e},$$

and the second-order condition is now

$$e > 0.$$

If either the second-order condition was violated or the pseudo optimal  $q$  was nonpositive, the pseudo optimal price  $p=21$  was chosen. Otherwise the entrepreneur was taken to charge

$$p = a - b \left( \frac{2e}{a - d} \right).$$

### VIII. *Methods of Testing*

As indicated earlier, Rules 1 and 2 yield results which are independent of the particular information assumed to be immediately available to the entrepreneur. Therefore one and only one rule-of-thumb solution is provided by each of these two rules for any given randomly generated demand function-cost function pair.

This is not the case with Rules 3, 4, 5, and 6. The particular solution obtained from these rules depends on which two out of the possible 22 points on the demand and cost functions are used in fitting the demand and cost functions.

We have assumed that a zero output or an output salable only at a zero price is never used by the entrepreneur in this calculation. Hence 20 points remain, out of which "nature" was taken to choose two for the entrepreneur to observe. In order to evaluate the performance of a rule of thumb we calculated its solution for each possible pair of points chosen out of the total of 20. There are 190 such pairs and therefore 190 profit figures corresponding to a single "true" demand function-cost function combination, i.e., to a single rule of thumb. For each particular true demand function-cost function combination we thus obtained the profit accruing under Rules 1 and 2, and each of these was compared with the *average* profit figure accruing under Rules 3, 4, 5, and 6.

Altogether, 24 different true demand function-cost function combinations were generated for each run.

### IX. *Results of the Calculations*

Table 1 contains a summary of the results of the calculations. The table is divided into six sets of rows, each set corresponding to one of the six runs, and seven columns, corresponding to true profit maximization and the six rules of thumb. In each set of rows corresponding to a particular run the first row contains the mean profit (for the 24 cases) under true profit maximization and the various rules of thumb; the second row records mean profit as a percentage of maximum profit; the third row contains the standard deviations of the profit expectations for the 24 cases; the fourth row reports the standard deviation divided by the mean; the fifth row contains the standard deviation over the 24 cases of the difference between maximum profit and mean profit achieved (a measure of *dependability* of the rule in approximating the

TABLE 1—SUMMARY OF RESULTS

	Max. Profit	Rule 1	Rule 2	Rule 3	Rule 4	Rule 5	Rule 6
RUN 1							
Mean Profit	3,398.2	3,204.7	2,350.8	2,980.9	2,971.8	2,985.0	223.5
Mean Profit/Maximum Profit	1.000	.943	.692	.877	.875	.878	.066
Standard Deviation	535.4	500.7	803.6	459.6	458.6	459.0	290.7
Standard Deviation/Mean Profit	.158	.156	.342	.154	.154	.154	.130
Standard Deviation of (Mean Profit—Maximum Profit)		190.3	890.5	168.0	152.0	169.4	441.8
Lowest Profit		2,016.0	483.0	1,925.6	1,951.6	1,928.5	-55.0
RUN 2							
Mean Profit	2,446.5	2,091.1	991.3	1,934.4	1,898.6	1,947.1	— .9
Mean Profit/Maximum Profit	1.000	.855	.405	.791	.776	.796	—
Standard Deviation	614.2	554.8	1,151.4	516.8	565.9	521.4	220.8
Standard Deviation/Mean Profit	.251	.265	1,161	.267	.298	.268	.237.46
Standard Deviation of (Mean Profit—Maximum Profit)		321.4	1,271.1	212.4	220.5	202.6	481.7
Lowest Profit		870.0	-1,770.0	1,001.5	1,118.1	1,040.9	-220.0
RUN 3							
Mean Profit	1,340.8	606.2	-821.3	741.3	650.2	770.7	-181.7
Mean Profit/Maximum Profit	1.000	.452	—	.553	.485	.576	—

TABLE 1—(Continued)

	Max. Profit	Rule 1	Rule 2	Rule 3	Rule 4	Rule 5	Rule 6
Standard Deviation	720.2	717.6	1,747.1	618.8	752.8	587.3	205.8
Standard Deviation/Mean Profit	.537	1.184	2.127	.835	1.158	7.62	1.133
Standard Deviation of (Mean Profit—Maximum Profit)		489.7	1,849.8	273.4	340.1	259.3	601.7
Lowest Profit		-658.0	-4,774.0	118.8	-658.7	.129.0	-440.0
RUN 4							
Mean Profit	3,048.9	2,764.7	1,617.5	2,519.9	2,608.4	2,541.0	522.5
Mean Profit/Maximum Profit	1,000	.907	.531	.826	.856	.833	.171
Standard Deviation	550.9	500.7	1,168.1	450.0	471.4	451.9	251.4
Standard Deviation/Mean Profit	.181	.181	.722	.179	.181	.178	.481
Standard Deviation of (Mean Profit—Maximum Profit)		277.3	1,275.7	212.8	169.4	204.2	428.6
Lowest Profit		1,576.0	-1,037.0	1,661.7	1,708.9	1,673.2	-25.7
RUN 5							
Mean Profit	2,186.3	1,651.1	258.0	1,574.7	1,639.7	1,608.0	26.7
Mean Profit/Maximum Profit	1,000	.755	.118	.720	.750	.736	.012
Standard Deviation	610.1	554.8	1,567.5	472.3	561.4	480.0	205.9

TABLE 1—(Continued)

	Max. Profit	Rule 1	Rule 2	Rule 3	Rule 4	Rule 5	Rule 6
Standard Deviation/Mean Profit	.279	.336	6.076	.300	.342	.298	7.867
Standard Deviation of (Mean Profit—Maximum Profit)		380.3	1,688.3	272.9	226.0	257.6	469.6
Lowest Profit		430.0	-3,290.0	913.7	791.5	983.3	-220.0
RUN 6							
Mean Profit	4,337.8	4,194.7	3,287.1	4,152.3	4,104.0	4,155.0	311.1
Mean Profit/Maximum Profit	1.000	.967	.758	.957	.946	.958	.072
Standard Deviation	517.1	500.5	883.2	485.6	487.6	487.6	498.4
Standard Deviation/Mean Profit	.119	.119	.269	.117	.119	.117	1.602
Standard Deviation of (Mean Profit—Maximum Profit)		125.0	853.4	73.0	126.0	73.6	614.3
Lowest Profit		3,006.0	1,167.0	2,969.2	2,958.5	2,967.2	-32.6

maximum), and, finally, the last row indicates the least profit encountered in the 24 cases.

The figures in Table 1 suggest that Rules 2 and 6 can be eliminated immediately on the basis of their poor performance. Rule 2 does not perform badly in Run 1 but tends to be much worse in most later runs. The reason is that, because of the upward shift in the cost function in Runs 2-5, the average optimal price is higher in these runs; hence the average deviation (average mistake) involved in using Rule 2, which uses random price setting, tends to increase in the later runs. The hyperbolic demand function involved in Rule 6 is bad because the true demand function is pseudo linear (except in Run 6) in the sense that if, in generating the demand function, we replace the random element by its expected value, we would indeed obtain a set of points lying on a straight line. In fact, the various actual demand functions generated can be judged visually to be linear to a reasonable level of approximation. Fitting a hyperbolic function to a linear set of points (particularly on the basis of two points only!) is likely to lead to poor results.

The relatively good performance of naïve Rule 1 (especially in Run 1) is not as inexplicable as it may appear. It is largely an artifact of the nature of our simulated cost and demand functions, as is shown in Appendix B. Indeed, it was the surprising profitability of Rule 1 which indicated some of the biases of our simulation model and led to the analysis in the Appendix.

Rules 3, 4, and 5 involve linear demand functions and linear, quadratic, and logarithmic cost functions, respectively. Since the total cost function is generated in essentially the same way as the demand function, it is pseudo linear (except in Runs 4 and 5 where it is pseudo quadratic) and we would, on the basis of previous arguments, expect Rule 3 to perform best in Runs 1, 2, 3, and 6. In terms of mean profit this is not quite the case but, on any criterion, the performance of Rule 3 is difficult to distinguish from that of Rule 5. All three of these rules yield high average profits, Rules 3 and 5 providing the largest returns in Runs 1, 2, 3, and 6. In Runs 4 and 5 the cost function is pseudo quadratic, and Rule 4 performs best in terms of mean profit, as indeed it should. On the basis of other criteria they do quite well too. With the exception of Rule 6, profits under Rules 3, 4, and 5 exhibit the smallest standard deviations, indeed in most cases *smaller than the standard deviation of the true optimal profit figures*. In most cases (except Run 3, Rule 4) the standard deviation as a fraction of mean profit is of the same order of magnitude for Rules 3, 4, and 5 as for maximum profits. The standard deviation of the difference between mean and maximum profit is lowest (except for one minor exception) for Rules 3, 4, and 5 and varies from run to run in such a manner as to preclude an unambiguous ranking

of these three rules. The lowest profit figures produced by Rules 3, 4, and 5 are also relatively high, easily beating the alternative rules of thumb except in a few cases.

Thus Rules 3, 4, and 5 seem to be the best of our rules of thumb, whether judged on variability of profit yield or on the basis of the maximum criterion. On the grounds of simplicity and lowest cost of computation, Rule 3 comes out ahead because it uses only linear functions. It is noteworthy that the simplest rule is also among the best as judged by the other criteria.

It may be noted in conclusion that the performance of these rules of thumb in terms of mean profit is indeed remarkable in view of the fact that the profit calculations were based on demand and cost functions fitted only to *pairs* of points on the two functions. Since the demand and cost functions are pseudo linear or pseudo quadratic in the sense described, we would expect reasonably good estimates of the true functions when the estimation procedure involves a pair of points lying relatively far from one another. However, we would expect rather poor estimates of the functions when the points upon which they are based are close together. On the whole, therefore, the mean profit results (which are averages of "good" and "bad" estimates) are surprisingly good, even those of Run 3. Although, when taken as per cent of maximum profit, the yields of the rules fell considerably in Run 3 (e.g., in that run Rule 3 earned only 55.3 per cent of maximum profit on the average), the absolute discrepancy between maximum profit and mean profit remained substantially unchanged through the three runs, the difference between maximum profits and Rule 3 profits being 417.31 in Run 1, 512.09 in Run 2, and 599.70 in Run 3.

#### *X. Concluding Comments*

The primary goal of this paper has been to open up a new area of investigation rather than to produce any final conclusions. We have discussed the general considerations which must be involved in the choice of effective rules of thumb. The difficulties inherent in the problem, both practical and analytical, were examined in some detail. Because of these problems, and the relative crudeness which characterizes portions of our investigation, even its limited results should, perhaps, be considered rather remarkable. The tests utilized in the analysis seem to have shown themselves capable of producing an effective ranking of alternative rules of thumb.

We are well aware that our results are highly dependent on the nature of the frequency distribution implicit in our random-number generators, on the set of rules which were chosen for testing, and on our methods of construction of the cost and demand functions. Future experimenters

might well try a number of alternatives, including demand and cost data involving random errors of observation and shifting cost and demand functions. The learning rules described in Appendix A would appear to merit further examination, and alternatives to the simulation methods employed in this paper should be considered. Empirical evaluation studies of rules of thumb used in industry might also be undertaken. It is obvious, then, that our main result is a set of suggestions for further research and some degree of assurance that this line of investigation is capable of yielding meaningful conclusions.

There still remains the question, how can the results of such a study be used? Surely, on the basis of our artificial and fragmentary experimentation no serious recommendation can be offered to anyone who is responsible for pricing decisions in practice. One path which may be more promising would require a far larger number and more diversified set of simulation calculations. It might then be possible for any given rule of thumb to characterize the types of situation in which it can be expected to perform relatively well. If this is done with sufficient care and the characterizations are carefully designed to be operational, it may even be possible to apply these conclusions to real decision problems.

At any rate, we have tried to illustrate the surprisingly difficult nature of a problem which, though often mentioned obliquely, seems never before to have been subjected to systematic attack. We must leave it to the reader to judge whether these initial results can be considered encouraging.

#### APPENDIX A: LEARNING RULES OF THUMB

We undertook no simulation study of rules of thumb in which the profit-maker attempts by trial and error to grope his way toward an optimum. Part of the reason is that the simulation model used by us would not have constituted an adequate trial of such a learning rule. With no shifts in the profit function, a well-designed learning rule can usually be relied on to converge toward the optimum as will be illustrated presently. Indeed, experience with analogous mathematical procedures such as Newton's method for approximating the roots of an equation suggests that they will often converge in fairly short order. Only in a model with frequent and unpredictable shifts in the cost and profit function is a learning rule put to the acid test, for only then can we see whether its convergence towards an optimum proceeds more quickly than the obsolescence of the past information which has been used in constructing it. With a fixed-profit function a good learning rule must almost certainly perform more satisfactorily in the long run than any other type of rule of thumb, but whether it will do so in practice depends entirely on the volatility of the relevant functions.

The learning-model pricing procedures can usually be represented by difference equations. However, these equations must be at least of second

order.<sup>16</sup> Such a rule states that the change from the price of this period to the next depends on the preceding price change and the resulting change in profit. Symbolically, we then have  $p_{t+1} - p_t = f(p_t - p_{t-1}, \pi_t - \pi_{t-1})$  where  $p_t$  and  $\pi_t$ , respectively, represent price and profit in period  $t$ , and where we can eliminate the profit figure from the equation by substituting in the profit function,  $\pi = g(p_t)$ .

This difference equation will normally be nonlinear because the profit function is normally nonlinear; indeed, it must be so if (in the absence of constraints) there is to be any profit-maximizing price.

In addition, a nonlinearity will be imparted by the fact that the sign of next period's price change depends both on the sign of last period's price change and on the sign of last period's profit change. More specifically, we require that if profits have gone up, next period's price change will be of the same sign as that of last period. But if profits have fallen, next period's price change will be of sign opposite from that of the preceding period.

One simple relationship which meets this requirement is

$$p_{t+1} - p_t = k \frac{\pi_t - \pi_{t-1}}{p_t - p_{t-1}}$$

if  $p_t \neq p_{t-1}$  and  $p_{t+1} - p_t = 0$  otherwise, where  $k$  is a speed-of-adjustment parameter. But this is only one of many possible equations which will satisfy our learning rule-of-thumb requirements. A simulation approach did not seem necessary since the consequences of such a learning rule can be ascertained analytically in at least some cases, as will now be shown.

(i) *A discrete model with quadratic profit function.* Define  $\Delta p_t$  as  $p_t - p_{t-1}$  and let the profit function be

$$\pi_t = ap_t - bp_t^2$$

where it may reasonably be assumed that  $a \geq 0$  and  $b \geq 0$ . The learning rule then gives

$$\Delta p_{t+1} = k \frac{a(p_{t-1} + \Delta p_t) - b(p_{t-1} + \Delta p_t)^2 - ap_{t-1} + bp_{t-1}^2}{\Delta p_t},$$

which simplifies to

$$(1) \quad p_{t+1} - (1 - b')p_t + b'p_{t-1} = a'$$

<sup>16</sup> A Newton's-method type of learning rule must be of third order. For to find the point of maximum  $\pi$  it seeks a value of  $p$  at which  $\Delta\pi/\Delta p$  approximates zero. This it does by taking two successive values  $(\Delta\pi/\Delta p)_t$  and  $(\Delta\pi/\Delta p)_{t-1}$  and fitting a linear relationship  $(\Delta\pi/\Delta p) = a + b'p$  to these observations. The value of  $p$  which satisfies  $a + b'p = 0$  is the first approximation to the optimal price, and the corresponding marginal profit figure is taken as  $(\Delta\pi/\Delta p)_{t+1}$  for the next iteration. Hence we have a second-order relationship in the  $\Delta\pi/\Delta p$ , which we may write  $(\Delta\pi/\Delta p)_{t+1} = f[(\Delta\pi/\Delta p)_t, (\Delta\pi/\Delta p)_{t-1}]$ . But since  $(\Delta\pi/\Delta p)_t = (\pi_{t+1} - \pi_t)/(p_{t+1} - p_t)$ , this relationship is of third order in our initial variables  $\pi_t$  and  $p_t$ . In other words, each iteration of such a Newton's-method rule requires *three* profit and price observations,  $\pi_{t-1}$ ,  $\pi_t$ ,  $\pi_{t+1}$ ,  $p_{t-1}$ ,  $p_t$  and  $p_{t+1}$ .

where  $b' = kb$  and  $a' = ka$ . It is readily shown that the second-order linear difference equation given by (1) converges to the equilibrium solution  $a/2b$  if and only if  $0 < b' < 1$ . That is to say, the solution converges if (a) the speed of reaction,  $k$ , is sufficiently small or (b) for given value of  $k$ , if the demand function and the marginal cost function are both relatively steeply inclined. For any given value of  $b$  it is then possible to construct a convergent learning rule by selecting  $k < 1/b$ , i.e., by employing a sufficiently moderate speed of reaction.

(ii) *A continuous model.* The learning model can be reformulated as follows:

$$(2) \quad \begin{aligned} \dot{p} &= k \frac{\pi}{p} && \text{if } p \neq 0 \\ \dot{p} &= 0 && \text{otherwise} \end{aligned}$$

where the dot denotes differentiation with respect to time and  $k$  is positive. Equation (2) is the continuous analogue of the discrete model discussed earlier.

Assume now that the profit function is a concave, differentiable function of  $p$ , that is

$$\pi = f(p)$$

and

$$f''(p) < 0.$$

Equation (2) then becomes

$$(3) \quad \dot{p} = k \frac{f'(p)\dot{p}}{\dot{p}} = kf'(p).$$

The stability of (3) is now examined from the global point of view. A system is globally stable if it converges to equilibrium irrespective of the initial condition of the system. Stability can be established by showing that the distance between the solution of (3) and equilibrium declines monotonically with time.<sup>17</sup>

Since equilibrium is given by  $\dot{p}_t = 0$ , we choose as our distance function the Euclidean norm

$$(4) \quad V(t) = \frac{1}{2}(\dot{p} - 0)^2$$

$V(t) \geq 0$  for all  $t$ . If, then, we show that  $\dot{V}(t) < 0$  for all  $t$ , we will have proved global stability.

Differentiating (4) we obtain

$$\dot{V}(t) = k^2 f'(p) f''(p) \dot{p}$$

<sup>17</sup> See Struble [6].

and, since  $kf'(p) = p$ , we have  $\dot{V}(t) = kf''(p)p^2 < 0$  by the concavity of the profit function, and the system is globally stable.

#### APPENDIX B: PROPERTIES OF THE SIMULATED PROFIT FUNCTION

We are fortunate that the distribution of the random variables on which our simulation is based is sufficiently simple to be amenable to direct analysis, and that, on this basis, the performance of Rule 1 can be predicted. For by comparing observed results with those derived from analytic calculations, one can obtain an estimate of the degree of reliability of the simulation approach employed.

Let  $x_i$  and  $y_i$  be random variables distributed uniformly over the integers  $1, \dots, 64$ . The  $x_i$  are the variables which (together) constitute the demand figure, whereas the  $y_i$  add up to the cost figure at any level of price. Let  $t$  be a new variable defined by  $t = 21 - p$  where  $p$  is the price. It is then easy to verify that the method of generating the demand and cost functions yields the profit function:

$$\pi_t = (21 - t) \sum_{i=1}^t (x_i + 2ai) - \sum_{i=0}^t (by_i - 8ci)$$

where the values of  $a$ ,  $b$  and  $c$  in the six runs are as follows:

	Run 1	Run 2	Run 3	Run 4	Run 5	Run 6
$a$	0	0	0	0	0	1
$b$	1	4	8	1	4	1
$c$	0	0	0	1	1	0

Since<sup>18</sup>  $E(x_i) = E(y_i) = 32.5$  and<sup>19</sup>  $\sigma^2(x_i) = \sigma^2(y_i) = 341.25$ , the expected profit and variance can be expressed as

$$E(\pi_t) = (21 - t)(32.5t + at(t + 1)) - b(32.5(t + 1)) - 4ct(t + 1)$$

and

$$\sigma^2(\pi_t) = (21 - t)^2 t \sigma^2(x_i) + b^2(t + 1) \sigma^2(y_i)$$

yielding, for  $p = 11$ , the following figures:

	Run 1	Run 2	Run 3	Run 4	Run 5	Run 6
$E(\pi_t)$	3,217.5	2,145.0	715.0	2,777.5	1,705.0	4,428.0
$\sigma(\pi_t)$	645.4	687.7	808.1	645.4	687.8	645.4
$\sigma(\bar{\pi}_t)$	132.0	140.6	165.3	132.0	140.6	132.0

<sup>18</sup>  $E(x_i) = E(y_i) = \frac{1}{64} \sum_{i=1}^{64} i = 32.5$ .

<sup>19</sup>  $\sigma^2(x_i) = \sigma^2(y_i) = \frac{1}{64} \sum_{i=1}^{64} (i - 32.5)^2 = 341.25$ .

The observed mean profit under Rule 1 approximates well the theoretical mean in all runs, although all observed means are greater than the corresponding theoretical quantities. The observed standard deviations tend to be lower than the theoretical ones but, using the test quantity  $ns^2/\sigma^2$  distributed as  $\chi^2$ , we cannot reject the hypothesis on the .10 significance level that the observed distributions are generated by theoretical distributions with the stated means and standard deviations. Whatever relatively bad agreement exists between some of the observed and theoretical quantities must be attributed to the random-number generator.

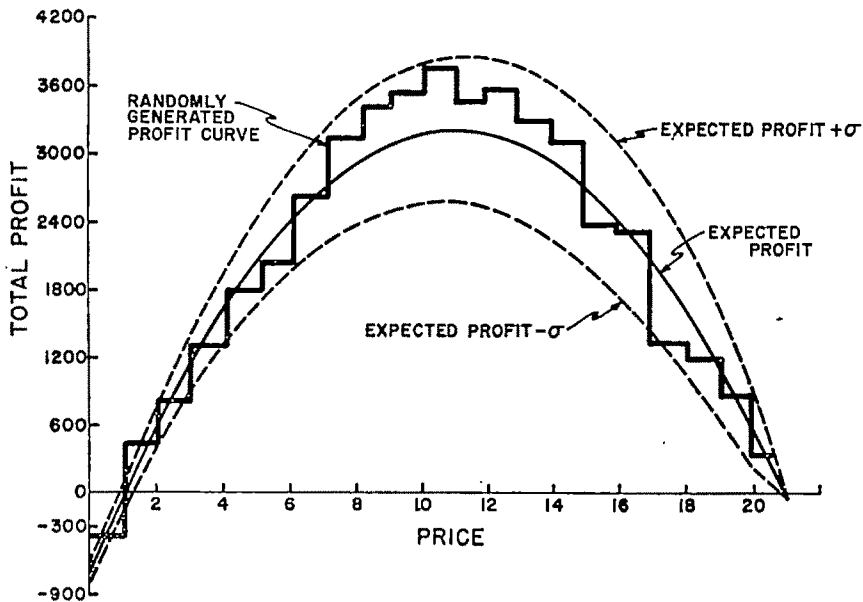


FIGURE 1

The relative goodness of Rule 1, particularly in Runs 1 and 6, becomes understandable in view of the fact that the expected profit function is fairly flat in the neighborhood of  $p=11$ : for example, for a price of \$14 expected profit diminishes in Run 1 from 3217.5 to 2925.0, less than 10 per cent. The situation can be made clearer with the aid of a graph. Figure 1 represents our simulated profit function. For Run 1 the expected profit function is a parabola with its maximum at  $p=11$ . If we draw dotted curves one standard deviation above and below the expected profit curve, we obtain a band between these lines that is rather narrow near the tails of the parabola but widens considerably in the neighborhood of the maximum point. This means that the simulated profit figures almost certainly must rise just to the right of the origin and that they almost certainly must fall as the expected profit graph approaches the horizontal axis. However, in the middle range, the stochastic curve can be relatively flat. This means that even when the peak of the expected profit curve is shifted to the right (as

it was in Runs 2 and 3) a fixed price  $p=11$  may still yield a total profit figure very close to the maximum profit level. It also becomes clear why Rule 1 does not do so well in Runs 2 and 3: the expected profit functions corresponding to these runs achieve their maxima at higher and higher prices. As a matter of fact, the observed mean optimal price was 10.25 in Run 1, 13.42 in Run 2, 14.58 in Run 3, 13.00 in Run 4, 13.83 in Run 5, and 10.88 in Run 6.

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## EXPORT CONCENTRATION AND FLUCTUATIONS IN EXPORT EARNINGS: A CROSS-SECTION ANALYSIS

By BENTON F. MASSELL\*

In recent years there has been considerable discussion of the problem of fluctuations in commodity markets and the impact of these fluctuations on countries producing primary products. The newly developing countries, in particular, many of whom are heavily dependent upon earnings from the sale of primary commodities to finance much-needed capital goods imports, have evidenced substantial concern with the instability of their export proceeds. In large part, this concern derives from the fact that commodity prices—and, consequently, foreign exchange earnings—have often exhibited a tendency towards secular decline; but in part it stems also from the shorter-run fluctuations around the trend.<sup>1</sup>

To a considerable extent, the source of fluctuations in export proceeds—at least, from the standpoint of a single country—is often believed to be concentration on an unnecessarily narrow range of products for export. There are many examples of so-called “one crop economies,” such as Ghana (cocoa), Mauritius (sugar), and Sudan (cotton). It is sometimes argued that if these economies were to diversify their exports, their export earnings would exhibit a greater degree of stability over time.<sup>2,3</sup>

In the present study an attempt is made to determine whether diversification is likely to provide a greatly increased measure of stability in export earnings.<sup>4</sup> This is done by examining empirically, in a sample of

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<sup>1</sup> Given the prices paid for imports, the trend in export proceeds determines the long-run gains that accrue to a country from international trade, and hence is an important determinant of the secular rise in the country's material welfare. Nevertheless, whatever the trend in export earnings may be, there are obvious advantages in reducing fluctuations around this trend. Uncertainty and unreliability of annual foreign exchange earnings may well create complexities in development planning. Furthermore, fluctuations in exchange proceeds tend periodically to exert strains on the balance of payments, possibly resulting alternatively in inflationary and deflationary pressures on the economy.

<sup>2</sup> “Fluctuations in proceeds from particular exports are more serious if the country concerned cannot rely on an averaging of the price movements of many different types of goods” [6, p. 9].

<sup>3</sup> The United Nations has noted, “A high degree of specialization is often a result of small ‘economic size,’ and is accompanied by a high degree of dependence on foreign trade, so that fluctuations in export proceeds have a devastating effect on the domestic economy” [6, p. 10].

<sup>4</sup> Two recent publications which deal with related issues are Michael Michaely's book [3]

36 countries, the extent to which fluctuations in a country's export earnings tend to be associated with concentration of the country's exports. A linear regression model is used which expresses export instability, the dependent variable, as a function of several independent variables. Section I considers the definition and measurement of an index of instability, while Section II develops an index of concentration. The empirical results are presented in Section III, and some conclusions, suggested by these results, in Section IV.

It is worth noting that a statistical study such as the present one has certain limitations. Aside from the usual qualifications which necessarily attach to the conclusions of any study based on regression analysis, there is a further qualification which stems from the uniqueness of countries and of commodities—or, more precisely, from the uniqueness of the particular choice confronting any country. Whatever the observable statistical relationship between concentration and instability in a cross-section of countries, a single country may rightly feel that its own position is sufficiently atypical to make these results of little interest. Thus, Ghana's decision whether or not to produce coffee in addition to cocoa should be based on the relationship between coffee and cocoa prices (among other things), rather than on generalizations based on aggregates of countries and of commodities.

Nevertheless, there is reason to believe that the conclusions presented in the following pages may provide general guidelines of some use in development planning by yielding a better understanding of certain fundamental economic relationships. Moreover, if the results cannot be given prescriptive value, perhaps they can at least explain existing intercountry disparities.

### *I. The Measurement of Export Receipt Instability*

The observable changes over time in the value of exports result from the interaction of a variety of market forces, both on the supply and the demand sides. It is an arbitrary, but nevertheless convenient, procedure to distinguish between long-run forces, which can be said to determine the trend, and short-run forces, which can be viewed as determining fluctuations around the trend.<sup>5</sup> If a measure of instability is used which does not distinguish between the two sets of forces, then a country with a rapid secular increase (or decrease) in its export earnings will exhibit

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and Joseph D. Coppock's book [1]. Unfortunately, the present study was essentially completed before I had access to either of these works. However, at the end of Section III the relationship between these two works and the present one is briefly discussed.

<sup>5</sup> It should be noted that intra-year fluctuations, which are appreciable in some cases, are not considered in this study.

greater instability than a country whose export earnings are secularly unchanging. For our purposes, this result would be undesirable. It appears more appropriate to measure annual fluctuations not around the mean but around the trend.

One way to do this is to fit a regression line to export earnings expressed as a function of time, and then measure exports as deviations from this estimated trend. Thus, we can write:

$$(1) \quad Z_t = \beta_0 + \beta_1 t,$$

where  $Z$  = export earnings,  $t$  = time, and where the  $\beta$ 's can be estimated by least squares.<sup>6</sup>

Using equation (1), the linear time trend was estimated for each of the 36 countries in our sample for the period 1948-59, with  $Z$  defined as the money value (in current monetary units) of the earnings received by the country from the sale of all merchandise exports.<sup>7</sup> The trend coefficients,  $\beta_1$ , together with the respective correlation coefficients,  $r$ , are presented in Table 1. The  $\beta_1$ 's range from .159 for Japan to -.019 for Argentina, the only country in the sample with a negative rate of growth for the period.<sup>8</sup>

As an index of instability, two measures, both trend-corrected, appear equally satisfactory. One is the standard error of estimate (square root of the unexplained variance), divided by the mean of the observations. This measure, which we shall term the "normalized standard error," is a pure number and is thus independent of the over-all level, or of the rate of growth, of a country's exports. We can write this variable:

$$(2) \quad I = \frac{\sqrt{\frac{\sum(u_i)^2}{n}}}{\bar{Z}}$$

where  $u_i = Z_t - (\beta_0 + \beta_1 t)$ ,  $n$  = the number of years in the series, and  $\bar{Z} = \sum Z_t / n$ .

Alternatively, we can use the average annual percentage rate of change in the value of exports, also trend-corrected, as given by:<sup>9</sup>

<sup>6</sup> In using equation (1) it is assumed that the trend can most appropriately be approximated by a linear function of time. In fact, an exponential trend was also fitted to the data, but inspection of the residuals suggested that the linear trend provided a better fit.

<sup>7</sup> In this estimate 1948 was chosen as the initial year in the belief that it represented the first "normal" postwar year. All the data used in this study were obtained from United Nations [8].

<sup>8</sup> In fact, the UAR also had a negative rate of growth, but this rounded off to zero for three decimal places.

<sup>9</sup>  $I^*$  is also a pure number.

$$(3) \quad I^* = \frac{\sum w_i}{n},$$

where

$$(4) \quad w_i = \frac{|u_{i+1} - u_i|}{\max. [Z_i, Z_{i+1}]}.$$

In the subsequent analysis we shall use both measures.

The variables  $I$  and  $I^*$  were computed for each country and are

TABLE 1—TIME TREND COEFFICIENTS AND TIME CORRELATION  
COEFFICIENTS FOR 36 COUNTRIES

Country	Time Trend Coefficient $\beta_1$	Correlation Coefficient $r$
Japan	.159	.984
Austria	.157	.988
Finland	.108	.921
Netherlands	.108	.987
France	.107	.940
Trinidad and Tobago	.105	.986
Iceland	.096	.954
Italy	.096	.967
Malta	.092	.903
Norway	.082	.931
Sweden	.078	.925
El Salvador	.076	.926
Cyprus	.073	.836
Ireland	.073	.941
Belgium	.066	.922
Thailand	.066	.870
Mauritius	.059	.875
Nigeria	.058	.857
United Kingdom	.057	.953
Portugal	.057	.886
New Zealand	.051	.881
Ghana	.051	.792
Dominican Republic	.050	.851
Philippines	.049	.895
Canada	.047	.943
United States	.045	.844
Australia	.040	.641
Panama	.039	.856
Colombia	.038	.582
Ceylon	.034	.664
Burma	.032	.627
Malaya	.023	.266
India	.013	.380
Brazil	.003	.090
United Arab Republic	-.000	-.004
Argentina	-.019	-.403

TABLE 2—TWO MEASURES OF INSTABILITY OF EXPORT RECEIPTS IN A SAMPLE OF 36 COUNTRIES

Country	Ratio of Standard Error of Estimate to Mean <i>I</i>	Average Annual Percentage Rate of Change, Trend-Corrected <i>I</i> *
Malaya	.284	.202
Colombia	.184	.096
Australia	.166	.160
Cyprus	.164	.123
Finland	.158	.135
Malta	.152	.176
Argentina	.150	.150
Burma	.138	.110
Ghana	.136	.133
France	.134	.121
Ceylon	.132	.095
Thailand	.129	.112
Brazil	.125	.101
United Arab Republic	.123	.096
Nigeria	.118	.094
Mauritius	.113	.104
Norway	.111	.111
Sweden	.111	.096
India	.108	.097
Dominican Republic	.108	.123
El Salvador	.106	.086
Iceland	.103	.146
Portugal	.103	.104
Japan	.101	.105
United States	.099	.105
Belgium-Luxembourg	.097	.100
New Zealand	.095	.083
Ireland	.091	.073
Italy	.087	.076
Philippines	.085	.121
Austria	.084	.091
Panama	.082	.087
United Kingdom	.063	.051
Netherlands	.061	.052
Trinidad and Tobago	.060	.057
Canada	.057	.062

presented in Table 2, columns 2 and 3, respectively. With the exception of Malaya ( $I = .284$ ), the values of  $I$  are relatively uniformly distributed between .057 (Canada) and .184 (Colombia). There seems to be a tendency for the countries commonly regarded as "underdeveloped" to have a larger value of  $I$  than the more economically advanced nations, although it is noteworthy that some underdeveloped countries have quite low values of  $I$ —for example, Trinidad (.060) and Panama (.082).

Turning to  $I^*$ , the values range from .051 (United Kingdom) to .202

(Malaya). For most countries, the value of  $I^*$  corresponds rather closely to that of  $I$ , but there are notable exceptions to this, for example, Malaya and Colombia. Spearman's Rank Correlation Coefficient was computed to test the significance of the country rankings according to the two indexes and was found to equal .718, with standard error .17, which is significant at the .01 level. The two measures of export instability are, of course, conceptually quite distinct; where  $I^*$  is a measure more of year-to-year changes,  $I$  is a measure of the variation of the series as a whole around the trend line. Either measure is influenced by the appropriateness, for a particular country, of fitting a linear time trend, but a poor fit would affect  $I$  more than  $I^*$ .

## II. A Measure of Export Concentration

One measure of export concentration, recently used by Michael Michaely [4], is the Gini coefficient, written

$$(5) \quad C = \sqrt{\sum (x_i/x)^2}$$

where  $x_i$  = the value of exports of commodity  $i$  in some specified year,<sup>10</sup> and  $x = \sum x_i$ . If a country that produces a given number of products for export divides its resources more evenly among these products, this will result in a reduction in  $C$ .  $C$  will also be reduced if the country produces an *additional* product for export provided that:<sup>11</sup>

$$(6) \quad 0 < \sum_{i=1}^m d_i \leq p'_i \leq p_i \leq 1, \text{ all } i,$$

where  $p_i$  = the proportion of the economy's resources initially devoted to the production for export of commodity  $i$ ,  $p'_i$  = the proportion after a new commodity has been added, and  $d_i = p_i - p'_i$ .

As Michaely has pointed out, the value of  $C$  depends in an important way on what commodity-classification scheme is employed. In particular,  $C$  will be higher, the greater the level of aggregation over commodities, for at a higher level of aggregation products which are relatively dissimilar are classified together. Thus, a country, all of whose exports fall within a single large group but are highly diversified within this group, may have a value of  $C$  higher than that of another country which is virtually a one-crop economy, but which produces also some product in a different group. A corresponding problem obviously arises if a highly disaggregated classificatory scheme is used. We have chosen, consequently, to compute  $C$  for the 36 countries in the sample on the

<sup>10</sup> In most cases, 1959 data were used in this study; where 1959 data were unavailable or incomplete, 1958 data were used.

<sup>11</sup> For a more detailed discussion of this point, see my forthcoming paper, "A Note on Export Diversification."

TABLE 3—MEASURES OF EXPORT CONCENTRATION IN A SAMPLE OF 36 COUNTRIES

Country	$C_2$	$C_1$	$P^a$	$G^b$
Mauritius	.991	1.000	1.000	.847
Trinidad and Tobago	.810	.826	.972	.461
Colombia	.780	.815	.981	.705
Panama	.740	.991	1.000	.967
Iceland	.715	.780	.998	.322
El Salvador	.704	.762	.956	.483
Ghana	.704	.733	.911	.393
Malaya	.697	.784	.845	.347
Burma	.691	.831	.967	.330
United Arab Republic	.685	.729	.885	.264
Ceylon	.649	.723	.985	.390
Brazil	.568	.777	.978	.465
Thailand	.490	.691	.981	.324
Dominican Republic	.490	.888	1.000	.593
Cyprus	.455	.665	.963	.454
New Zealand	.449	.691	.968	.619
Nigeria	.439	.646	.982	.571
Malta	.428	.581	.781	.500
Australia	.427	.608	.875	.387
Philippines	.414	.675	.955	.611
Finland	.373	.590	.495	.336
Ireland	.365	.533	.686	.814
Argentina	.303	.729	.952	.340
India	.280	.539	.565	.347
Belgium-Luxembourg	.264	.600	.165	.328
Sweden	.252	.522	.372	.291
Austria	.242	.529	.279	.352
Portugal	.237	.453	.540	.285
Norway	.233	.493	.402	.321
Canada	.227	.489	.547	.601
Japan	.215	.527	.120	.354
France	.214	.448	.248	.280
Italy	.207	.429	.305	.270
United Kingdom	.195	.521	.132	.212
Netherlands	.170	.420	.469	.334
United States	.160	.431	.337	.292

<sup>a</sup>  $P$  is a measure of a country's concentration on primary products.

<sup>b</sup>  $G$  is a measure of a country's geographical concentration of exports.

basis of two classification schemes, differing in the level of aggregation involved: (1) on the basis of the Standard International Trade Classification (SITC) one-digit industries, and (2) on the basis of the SITC three-digit industries—denoted  $C_1$  and  $C_2$ , respectively.<sup>12</sup>

We computed both measures of concentration for the group of 36 countries. The values of  $C_2$  appear in Table 3, column 2. These values

<sup>12</sup> Another problem is the appropriateness of the SITC commodity breakdown to the analysis presented here. For example, if industrial products are classified more finely than agricultural commodities, primary-product exporting countries will be biased towards greater concentration.

TABLE 4—A COMPARISON OF EXPORT CONCENTRATION AS MEASURED IN THIS STUDY AND AS MEASURED BY M. MICHAELY

Country	$C_3$ (Massell)	$C_3$ (Michaely)	$G^a$ (Massell)	$G^a$ (Michaely)
Mauritius	.991	.988	.847	.776
Trinidad and Tobago	.810	.727	.461	.500
Colombia	.780	.840	.705	.798
Panama	.740	.628	.967	.955
Iceland	.715	.803	.322	.291
Ghana	.704	.835	.393	.479
Malaya <sup>b</sup>	.697	.498	.347	.260
Burma	.691	.744	.330	.476
United Arab Republic	.685	.842	.264	.260
Brazil	.568	.612	.465	.415
Thailand	.490	.683	.324	.435
Nigeria	.439	.493	.571	.740
Australia	.427	.508	.387	.412
Finland	.373	.381	.336	.340
Ireland	.365	.383	.814	.897
Argentina	.303	.306	.340	.321
Belgium-Luxembourg	.264	.255	.328	.294
Sweden	.252	.281	.291	.284
Austria	.242	.277	.352	.318
Portugal	.237	.247	.285	.274
Norway	.233	.255	.321	.280
Canada	.227	.249	.601	.639
Japan	.215	.248	.354	.240
France	.214	.180	.280	.218
Italy	.207	.205	.270	.211
United Kingdom	.195	.192	.212	.187
Netherlands	.170	.169	.334	.270
United States	.160	.188	.292	.275

<sup>a</sup>  $G$  is a measure of a country's geographical concentration of exports.

<sup>b</sup> Michaely's figures include Singapore.

Sources: Massell, Table 3; Michaely [4].

range all the way from .160 (United States) to .991 (Mauritius). With few exceptions, the less-developed countries tend to have higher values of  $C_3$ , as one would expect.

For comparison we present in Table 4, together with our own values of  $C_3$ , those obtained by Michaely for many of the same countries, based on 1954 exports.<sup>13</sup> As one can see, Michaely's figures tend to correspond fairly well with ours, although he obtains higher values of  $C_3$  for 19 of the 28 countries compared. The difference between the two sets of figures may be due to any of three factors: (1) To some extent, the structure of exports of several of the countries may have altered in the interval 1954 to 1959. (2) The country's methods of reporting the data, while supposedly standardized, may have been changed during this

<sup>13</sup> Michaely [4] computed  $C$  for the SITC three-digit classification only.

interval; for example, reporting exports in greater detail in more recent years would tend to make our  $C_3$  lower than Michaely's. (3) Perhaps, also, differences in processing the published data may have accounted for some of the discrepancy.

In column 3 of Table 3 are shown the values of  $C_1$ . One can see that, from the definition of the  $C$  indexes, the following inequality must hold for any country:

$$(7) \quad C_1 \geq C_3.$$

$C_1$  tends to be appreciably larger than  $C_3$ , reflecting the difference in aggregation.  $C_1$  ranges from .420 (Netherlands) to 1.000 (Mauritius).

### III. *Empirical Results*

To estimate the relationship between the instability and concentration of exports, regression analysis was used, with  $I$  and  $I^*$ , alternatively, as dependent variables, and  $C_1$  and  $C_3$  as independent variables. The regression equations are written:

$$(8) \quad I = a_{i0} + a_{i1}C_i$$

$$(9) \quad I^* = a_{i0}^* + a_{i1}^*C_i$$

where  $i=1, 3$ .

The entire sample of 36 countries was used to estimate the coefficients in (9), but for (8) Malaya was deleted, as the value of  $I$  for this country was sufficiently greater than the next highest value to suggest that it might represent a special case which it was safer to omit.<sup>14</sup>

The estimated regression coefficients for  $C_1$  and  $C_3$  are presented in Table 5, with the standard errors in parentheses beneath their respective coefficients. With a one-tailed test, none of the slope coefficients is significant at the .05 level.

Before concluding that the insignificance of the simple regressions implies the rejection of the hypothesized relationship, let us add an additional variable to the model. We begin by noting that concentration can refer not only to the commodity composition of a country's exports but also to the geographical composition of established markets for these products. A country whose exports were destined principally for only one or two countries would be termed highly concentrated in the latter sense; diversification would involve seeking a greater variety of markets or spreading exports more evenly over existing markets.

To measure the geographical concentration of exports, we turn again to the Gini coefficient, as given by

$$(10) \quad G = \sum (Y_i/Y)^2$$

<sup>14</sup> See Table 2 for the range of values of  $I$ .

TABLE 5—RELATIONSHIP BETWEEN EXPORT INSTABILITY AND CONCENTRATION

Equation number in the text	Dependent Variable	Independent Variables: Regression Coefficients (standard errors in parentheses)				Coefficient of Determination <sup>a</sup> $R^2$	$F$ -ratio <sup>b</sup>
		$C_0$	$C_1$	$P$	$G$		
(8)	I	.037 (.032)				.090	3.26
(8)	I		.043 (.032)			.046	1.59
(11)	I	.055 (.027)			-.043 (.033)	.117	2.12
(11)	I		.073 (.041)		-.046 (.036)	.093	1.64
(13)	I			.052 (.019)	-.056 (.032)	.201	4.03
(9)	I*	.060 (.041)				.033	1.16
(9)	I*		.068 (.042)			.038	1.35
(12)	I*	.046 (.027)			-.053 (.034)	.100	1.82
(12)	I*		.056 (.038)		-.046 (.033)	.078	1.35
(14)	I*			.048 (.019)	-.067 (.033)	.172	3.42

<sup>a</sup> The coefficient of determination is the proportion of the total variance of the dependent variable which is "explained" by the regression.

<sup>b</sup> The  $F$  ratio is given by:  $F = R^2(n-k-1)/(1-R^2)k$ , where  $n$  is the number of observations in the sample and  $k$  is the number of independent variables. The only values of  $F$  significant at the .95 level are those for equations (13) and (14).

where  $Y_i$  = exports to country  $i$ , and  $Y = \sum Y_i$ . The values of  $G$  for the 36 countries appear in Table 3, column 5. These values range from .212 (United Kingdom) to .967 (Panama). In Table 4, above, the values of  $G$  are compared with those obtained by Michaely [4].<sup>15</sup> For 10 of the 28 countries compared, Michaely arrives at a higher value of  $G$ .

One could argue that, if geographical concentration indicates the absence of flexibility, then this variable might be expected to be positively correlated with instability. On the other hand, it is quite possible that countries whose exports are highly concentrated geographically

<sup>15</sup> The measure  $G$  was also computed by Albert Hirschman [2].

tend to have more effective methods of smoothing out the fluctuations in export receipts, perhaps because bilateral commodity arrangements may be more prevalent in such cases. Those countries in the sample with high values of  $G$  tend to ship a major part of their exports to the United States or the United Kingdom. In many cases, it is likely that some form of commodity agreement between the exporting and importing countries tends to reduce fluctuations in export receipts. Perhaps the dominant trading partner in these cases either pegs the price of the staple exports or else imports a guaranteed amount, in either case insulating the exporter from the full impact of market forces.

By adding the new variable, the regression equations can be rewritten:

$$(11) \quad I = \gamma_{i0} + \gamma_{i1}C_1 + \gamma_{i2}G,$$

and

$$(12) \quad I^* = \gamma_{i0}^* + \gamma_{i1}^*C_1 + \gamma_{i2}^*G.$$

The estimated regression coefficients are presented in Table 5.

Turning first to the regressions on  $I$ , we see that both  $\gamma_{i1}$  and  $\gamma_{i2}$  are significant at the .05 level, with a one-tailed test. Further, the  $\gamma_{i0}$ , though insignificant at the .05 level with a two-tailed test, are both negative. Thus, the results suggest that  $I$ , while not simply correlated with either  $C_1$  or  $C_2$ , is correlated with each of these two variables *net* of geographical concentration. In other words, given the degree of geographical concentration of a country's exports, the instability of its export receipts is positively correlated with commodity concentration. As indicated in Table 5 by the  $F$ -ratio<sup>18</sup> (or by the value of the coefficient of determination,  $R^2$ ), a greater proportion of the intercountry variation in  $I$  is explained by employing  $C_2$  and  $G$ , rather than  $C_1$  and  $G$ , as the pair of independent variables, although even with the former pair nearly 90 per cent of the variation remains unexplained. The partial correlation coefficient between  $I$  and  $C_2$  is .338, while that between  $I$  and  $C_1$  is .300.

With  $I^*$  as the dependent variable, Table 5 shows results similar to those just discussed. However, the only significant regression coefficient is  $\gamma_{i2}^*$ , and the  $\gamma^*$ 's are slightly lower in value than the  $\gamma$ 's. The  $F$ -ratio is also lower, suggesting that the independent variables provide a better explanation of intercountry variation in  $I$  than in  $I^*$ .

The low correlation between instability and concentration seems to contradict established doctrine, and thus merits further attention. To some extent, the low correlation may result from large differences in the volatility of different commodities. Some products—notably primary

<sup>18</sup> Neither value of  $F$  is significant at the .95 level.

products—are subject to large shifts in the demand schedule because of changes in expectation or because of the cyclical character of industrial activity in the more mature economies, while other products have a relatively stable demand pattern. Moreover, products differ in the extent to which they are subject to fluctuations on the supply side—for example, some primary products are affected by the weather or by the incidence of various types of plant disease. The over-all result is that the extent of fluctuations in price and in volume traded—and in the product of the two—differs markedly from one product to another. In the context of the present inquiry, this means that the countries that specialize in the more volatile products will tend to show a more marked instability of export revenues, for a given concentration of exports, than economies whose exports consist primarily of the more stable products. But, to the extent that one would expect a systematic relationship between concentration of exports and volatility of the goods exported, this relationship should strengthen the positive correlation between instability and concentration. Primary products can be expected to be more volatile than manufactured goods, and the exports of primary producing countries are typically more highly concentrated than those of industrial economies.<sup>17</sup> However, if the disparity in the instability of earnings from the sale of different products is sufficiently great, then, while the regression line will have a positive slope, there will nevertheless be considerable scatter around the line—and this is in fact what has been observed.

Another point, though, should be considered. The rationale for diversification is based on the assumption of statistical independence between annual changes in earnings from the export of any two products. But if the cross elasticity of demand for two commodities is high, then a shift in the demand for one is likely to be accompanied by a comparable shift in demand for the other, and the proceeds from the two products will tend to be intercorrelated. If the intercorrelation is sufficiently great, the value of diversification will be greatly reduced. Of course, shifts in the demand schedule account for only part of the fluctuations in export earnings; another part is due to shifts in the supply curve, and there is less reason to expect a high degree of correlation between the supply schedules of different products.<sup>18</sup> Thus one can expect some random element to be present in the determination of export earnings from the production of different commodities.

While we cannot investigate here the statistical independence among products, it is of some interest to examine briefly the price changes for a selected group of commodities for the period 1950–58, shown in

<sup>17</sup> See below; also, cf. Michaely [4].

<sup>18</sup> Although a drought will result in reduced production of most agricultural commodities.

TABLE 6—EXAMPLES OF FLUCTUATIONS IN COMMODITY PRICE INDEXES

Commodity	Price Index (January-June 1950 = 100)								
	1950	1951	1952	1953	1954	1955	1956	1957	1958
Cocoa	122	135	135	141	220 <sup>a</sup>	141	103 <sup>b</sup>	117	167
Coffee	106	113	113	121	165 <sup>a</sup>	119	122	119	102 <sup>b</sup>
Tea	99	91	83	91	125 <sup>a</sup>	105	104	90	81 <sup>b</sup>
Cotton	123	201 <sup>a</sup>	141	100	112	102	93	92	88 <sup>b</sup>
Wool	123	149 <sup>a</sup>	95	110	96	80	85	94	66 <sup>b</sup>
Rubber	172	263 <sup>a</sup>	147	103 <sup>b</sup>	105	174	148	135	122
Tobacco	100	104	111	113 <sup>a</sup>	109	113 <sup>a</sup>	91 <sup>b</sup>	95	95
Copper	111 <sup>b</sup>	138	163	160	158	223 <sup>a</sup>	209	139	125
Tin	125	180 <sup>a</sup>	161	122	120 <sup>b</sup>	124	132	126	122
Manganese	104 <sup>b</sup>	125	144	161	145	151	155	175 <sup>a</sup>	174

<sup>a</sup> High for period.

<sup>b</sup> Low for period.

Source: United Nations [7 p. 181].

Table 6. We see that four commodities reached a high in 1951, three in 1954, one each in 1955 and 1959, and one both in 1953 and in 1955; while two reached lows in 1950, one each in 1953 and 1954, two in 1956, and four in 1958. While there is apparently some intercorrelation among the time paths, this intercorrelation is far from perfect.

Nevertheless, there may be some merit in examining an alternative hypothesis: that fluctuations in export earnings result not from high export concentration, as defined by the Gini coefficient, but from concentration on the export of primary products, as opposed to industrial goods. This might be the case if intercorrelation between the earnings time paths of two primary products tends to exceed that between the earnings time paths of two industrial goods, or of an industrial good and a primary product, perhaps because of a similarity in the market forces affecting many internationally traded primary products.

To measure the extent of a country's concentration on primary products, we have used the ratio,  $P$ , of primary-product exports to total exports, where primary products were defined as SITC Groups 0 to 4. This variable, which we shall term the primary-product ratio, was computed for the sample of countries and is shown in column 4 of Table 3. The values range from .120 for Japan to 1.000 for several countries.

The regression equations can now be written:

$$(13) \quad I = \xi_0 + \xi_p P + \xi_g G$$

and

$$(14) \quad I^* = \xi_0^* + \xi_p^* P + \xi_g^* G.$$

The coefficients appear in Table 5.

Using a one-tailed test, one finds a significant correlation at the .05 level between either  $I$  or  $I^*$  and  $P$ , net of  $G$ .<sup>19</sup> An increase in  $P$  by 20 percentage points, given the value of  $G$ , is associated with a 1 per cent rise in either  $I$  or  $I^*$ , indicating that  $P$  explains only a small part of the variation among countries in export instability. As indicated by the  $F$ -ratio, the pair of variables,  $P$  and  $G$ , provide a better explanation of variation in  $I$  than in  $I^*$ .

The partial correlation coefficient between  $I$  and  $P$ , in equation (13), is .445 compared with a value of .338 between  $I$  and  $C_3$  in equation (11). This suggests that  $P$  provides a better explanation than  $C_3$  (which, in turn, is better than  $C_1$ ) of the variation in  $I$ . The  $F$ -ratios confirm that the pair of variables,  $P$  and  $G$ , provides the best explanation of inter-country variation in either  $I$  or  $I^*$ . In both cases,  $F$  is significant at the .95 level.

An attempt to include both  $P$  and either  $C_3$  or  $C_1$  as independent variables yielded no significant results, possibly because of multicollinearity. In fact, it can be observed that  $C_3$ ,  $C_1$ , and  $P$  all appear to be intercorrelated, thus indicating that primary-producing countries tend to have more highly concentrated exports.

While the low correlation between  $I$  and either  $C$  index is not at all what one would expect, the weak relationship between  $I$  and  $P$  is, perhaps, even more surprising. These results suggest that we must reject the view that fluctuations in export earnings affect only the primary-producing countries; apparently the industrial countries share this instability in nearly the same measure. One should note, though, that some commodities are now subject to control, so that the statistical results reflect, in part, these institutional arrangements. The low degree of correlation between  $I$  and any of the independent variables must not be interpreted as suggesting that the commodity stabilization schemes now in effect are gratuitous; indeed, it may be that the partial success of these schemes has contributed to the low observed correlations. Fluctuations in the export earnings of primary-producing countries might have been considerably worse in the absence of these commodity control arrangements.<sup>20</sup>

It is of some interest to discuss briefly the relationship between this study and those of Coppock [1] and Michaely [3]. Coppock's book is broader in scope than the present study, as his title suggests. However, his Chapters 5 and 6 are concerned with explaining the instability of export proceeds, as is the present work. Included in the nearly 40 independent variables which Coppock considers are both commodity con-

<sup>19</sup> With  $I$  as dependent variable,  $G$  is also significantly negative at the .05 level, using a two-tailed test. A simple regression of  $I$  or  $I^*$  against either  $P$  or  $G$  fails, however, to be significant.

<sup>20</sup> This is particularly so if—as seems likely—the most volatile commodities are the ones which have been made subject to international agreements.

centration and geographical concentration of exports (although not the primary-product ratio). Although Coppock's multiple correlation results are not comparable with ours, due to fundamental differences in methodology, it is interesting to note that he finds neither commodity nor geographical concentration important in a simple regression.

We referred earlier to the relationship between our measures of concentration and those of Michaely. Here we shall consider briefly the relationship between Michaely's Chapter 5 ("Commodity Concentration and Price Fluctuations") and the present study. In his Chapter 5, Michaely constructs an index of export *prices* for the countries in his sample. He finds that fluctuations in this index are positively correlated with concentration of the country's exports and with the primary-product ratio, taking each independent variable separately; the respective correlation coefficients are .397 and .252. However, on the basis of partial correlation analysis, Michaely concludes that the simple relationship between export instability and the primary-product ratio is "solely due to the strong association between commodity concentration and the extent of specialization in primary goods" [3, p. 78]. Michaely also looks at the relationship between fluctuations in export *volume* and concentration of exports, and finds a positive rank correlation.

Our results above indicate a positive (but insignificant) simple correlation between export *proceeds* and concentration. For example, from Table 5 we see that the simple correlation coefficient for equation (8) is .3. But, because Michaely uses *rank* correlation for part of his study, and because he does not consider the statistical significance of many of his coefficients, it is difficult to say to what extent his results are consistent with ours.

#### IV. Conclusions

Several points appear to follow from the preceding analysis. First, it is clear that the relationship between instability of export earnings and concentration of exports is a tenuous one indeed.<sup>21</sup> As the degree of concentration of a country's exports explains such a small proportion of the intercountry variation in export instability, one would not expect a policy aimed at diversification generally to result in a marked reduction in fluctuations—although such a policy may well be effective in certain individual cases.

As the relationship between instability and the primary-product ratio is also relatively weak, this investigation gives little support for in-

<sup>21</sup> Moreover, the results suggest that the observed relationship between instability and concentration may result from (1) the relationship between instability and the primary-product ratio, together with (2) the tendency for primary-producing countries to have highly concentrated exports.

dustrialization as a method of reducing export instability. But the fact that neither diversification nor the degree of industrialization appears to explain much of the variation in export instability has other implications. Why, one might ask, do the primary-producing countries typically exhibit so much concern with the problem of instability if, as the data indicate, the industrialized countries have been subject to export fluctuations to nearly the same extent? One reason may be that primary-producing countries tend also to be low-income countries and are consequently more disturbed by fluctuations than wealthier countries. Also, primary-producing countries tend to depend more heavily on exports as a source of income than do industrialized countries, so that a given degree of export instability has a greater impact on the economy of a primary-producing country.

Furthermore, fluctuations in commodity prices may have a real cost to primary producers quite apart from the visible effect on annual earnings. To some extent, perhaps, the producers have responded to price changes by means of offsetting changes in volume offered for sale [5]. Thus a rise in price may have been met by a fall in volume, and a fall in price by an increase in volume. We shall not examine here whether this seems to be the case, but will merely note that, in instances where this may have occurred, the primary-producing countries have achieved greater revenue stability, but at a cost. However, diversification would presumably not reduce this cost for the individual producer.

We conclude by noting that it is unlikely that either instability of export earnings or the disutility arising from such instability will be eliminated by simple policies, such as producing a wider range of exports. To the extent that the disutility created by fluctuations in exports is intensified by the low incomes of the primary-producing countries, the problem is basically a manifestation of poverty and, as such, will be eliminated or reduced as the country achieves economic development.

The general case for diversification (or, indeed, for industrialization) as a cure for fluctuations in export earnings receives little support from this investigation. For a given country, however, there may be some gain in choosing a *particular product* that will help reduce the fluctuations in its exports receipts. And of course diversification may be beneficial in other ways, for example, in providing the economy with greater flexibility in adapting the structure of its production to changes in market conditions.

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## OPTIMIZATION AND SUBOPTIMIZATION IN FISHERY REGULATION

By RALPH TURVEY\*

The purpose of this article is to show that fishery regulation is one of those spheres of economic policy where what is the best thing to do depends on what can be done. This is usually illustrated by the analogy that, if one wants to climb as high as possible but cannot climb all the way up the highest mountain, the best thing to do may be to walk in the opposite direction and climb to the top of a lower one. If the *optimum optimorum* is to be reached (the highest mountain scaled), then regulation must extend not only to the scale but also to the mode of operation.

That this rather general point has an important application in fisheries management became apparent to me during a conference in Ottawa of economists, biologists, and administrators organized by the Food and Agricultural Organization in 1961. The proceedings of this conference have now been published [8]. The two general survey papers by Professor Scott and Dr. Dickie at the beginning of this volume afford an admirable survey of the whole subject, including a review of the literature. I shall therefore not attempt to summarize their contributions, and what follows is a self-contained argument which may, however, incidentally serve to introduce readers to a fascinating subject where economists and biologists are clearly complements and only talk as though they were substitutes when they misunderstand one another.

What follows is a static (i.e., steady state) analysis of a single-trawl fishery where there is only one fish stock, fished from ports which supply a common market and which are equidistant from the fishing ground. It is assumed that the port market is competitive and that there are no restrictions on entry into the fishery. These assumptions serve to make the exposition reasonably simple; their removal would not destroy the essential argument of the paper.

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### I. *Catch*

The determinants of the weight of catch, under these assumptions, are twofold,<sup>1</sup> given constant natural population parameters. The first is the rate of capture (the fishing mortality rate) of the fish liable to capture, which is proportionate to the amount of fishing effort. This is defined as the product of the number of hours' fishing (or some other index of fishing time) and of fishing power, where the fishing power of any particular ship is defined by reference to a standard vessel by comparing their catches when fishing at the same time and place. In practice, the fishing power of trawlers is broadly related to the gross tonnages, so that fishing effort in a given time interval may be measured as the average gross tonnage of the fishing fleet multiplied by the total number of hours' fishing. The relevance of fishing effort is simply that, if it is decreased, the stock of fish will grow and the average age, weight, and size of the fish will increase, making fishing easier, i.e., increasing catch per unit of fishing effort.

The second determinant of the weight of catch is the size (age) at which the fish become liable to capture, i.e., are "recruited" to the fishery. In a trawl fishery, which is the example we take here, this is regulated by the mesh size of the cod-ends of the trawls used. This determines the minimum size of fish caught; the larger the mesh, the older, larger, and heavier will be the fish liable to capture. Thus, other things remaining the same, an increase in mesh size will initially reduce the catch but may ultimately raise it by increasing the stock. Here, since the analysis is static, it is only this ultimate effect which concerns us.

Biologists have formulated a number of models which provide a function relating the weight of catch to the two variables, mesh size and fishing effort. These models deal with the rates of natural mortality, growth, and recruitment, each of which is a function of the size of the stock and its age distribution. As in economics, metrics is several steps behind theory, however, so that not all the parameters of the more complicated models can at present be estimated, owing to lack of data. Things which are known to be relevant, such as the spatial distribution of fishing effort, just have to be ignored. If we confine ourselves to the operational models used for most fishery assessments, the simplified biological interrelationships that have been introduced are the following: (a) natural mortality a function of age; (b) growth of the fish, by weight and length, a function of its age; (c) recruitment to the fish stock, i.e., the number of fish reaching catchable size, treated as exogenous; and (d) fishing mortality rate proportional to fishing effort.

<sup>1</sup> The standard work on all this is by R. J. H. Beverton and S. J. Holt [1]. On the measurement of "fishing effort" see also J. A. Gulland [3]. A useful survey article, of which Dr. Parrish has kindly lent me a translation, is by G. Hempel and D. Sahrhage [4].

The steady-state catch in weight from a fish stock is then determined by the interaction between the rates of these natural processes and the fishing mortality rate. The relationship between weight of catch and age of recruitment (mesh size) and fishing mortality rate (fishing effort), estimated from a simple, dynamic model in which the rates of the natural processes are assumed constant, takes the form shown in Figure 1, where fishing effort is measured horizontally and weight of catch is measured vertically. Each yield curve shows catch as a function of fishing effort for a given age of recruitment to the fished stock. (The dotted line will be explained later.)

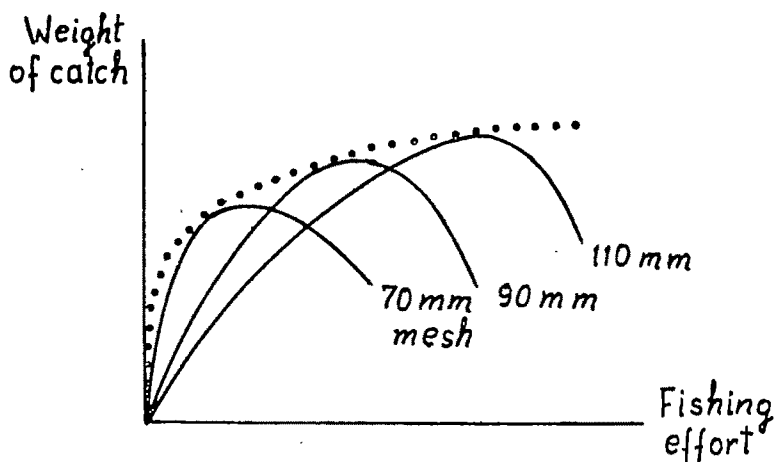


FIGURE 1

## II. Costs

The study of fishing costs is a complicated matter. To illustrate, a large part of fishermen's remuneration is a share in profits rather than a wage.<sup>2</sup> Such matters are not relevant to the argument of the present paper, however, so I shall exclude them by simply assuming that over the relevant range of catch, the total cost of fishing effort rises more than proportionately to the amount of fishing effort because the minimum earnings necessary to attract and retain labor and capital at the margin rise as more of these resources are employed in fishing. Total cost is defined to include the "rents" of the intramarginal factors, i.e., the excess of their actual earnings over the minimum required to retain them in the industry. This is the only aspect of costs that we require in the present argument, so I need not discuss whether few ships fish

<sup>2</sup> See Zoutewij, "Fishermen's Remuneration" in [7]. Economies of scale in shore installations constitute another example.

many hours or vice versa, since fishing effort as defined above is the independent variable of the cost function. What is significant in this formulation is the implicit assumption that mesh size has no noticeable effect upon the costs of fishing effort. This is probably true within the range of mesh size that is relevant in practice.

### III. *Revenue*

The price of fish, given income levels and the prices of other foods, depends not only upon the total weight of the catch but also upon its size distribution and upon its freshness. Here we shall assume that there is some given minimum marketable size of fish; otherwise I neglect these points.<sup>3</sup> Thus I postulate a given function relating total revenue to weight of catch.

### IV. *Steady-State Bionomic Equilibrium*

Let us postulate that: (1) any fluctuations in the natural processes are small and have no trend; and (2) there is free entry of resources to the fishery. Then, given all the various functions discussed above, the long-run equilibrium position can be deduced.

On the economic side, the equilibrium condition is that total revenue equals total cost including the rent of intramarginal factors. We put it this way rather than in terms of average revenue and long-run marginal cost because average revenue is related to weight of catch and marginal cost to fishing effort. In order to relate average revenue to marginal cost, we would have to convert catch into effort, and write the condition as:

$$\text{Price} \times \text{Catch per unit effort} = \text{Marginal cost of fishing effort excluding rent}$$

$$\text{i.e., Average revenue} \times \text{Catch per unit effort} = \text{Average cost including rent}$$

which gives us:

$$\text{Price} \times \text{Catch} = \text{Average cost including rent} \times \text{No. of units of effort}$$

i.e., Total revenue = Total cost including rent.

If total revenue exceeds total costs, resources not in the industry will find that it is worth while to move into it, while in the reverse case some resources at the margin will be earning less than is required to retain them in the industry, and they will leave it.

Each fisherman will want to maximize the marketable value of his catch at any given level of costs. This means, on our assumptions, that

<sup>3</sup> The way in which price depends upon size and freshness can, of course, be observed but it would probably be extremely difficult to estimate how the price pattern would change following alterations in these variables. Note that freshness is related to length of stay at sea, which in turn is related to the size of ship and hence affects costs.

he will wish to maximize the weight of his catch of fish above the minimum marketable size for any given level of costs. Thus for the present argument we will assume that he will choose a mesh which limits his catch to fish above that size. If he used a smaller mesh, he would have to throw part of his catch back into the sea, which would involve unnecessary trouble.

We can now describe the equilibrium position. The minimum marketable size of fish determines mesh size. This, in conjunction with fishing effort, determines the weight of catch. This, in turn, determines total revenue which must equal the total costs of the amount of fishing involved. All this is shown in Figure 2 whose N.E. quadrant corresponds to Figure 1 and where the equality of total revenue and total costs is shown by using a 45° line in the S.W. quadrant.

As Figure 2 is drawn, either a general fall in cost or a rise in the demand for fish will cause an increase in the equilibrium catch. But if fishing effort has been carried beyond the point of maximum yield this standard conclusion does not follow, as Figure 3 shows. Here the dashed line shows one equilibrium position and the dotted line shows another; the latter corresponds to a higher level of demand, represented by the dotted total revenue curve. In this case the higher level

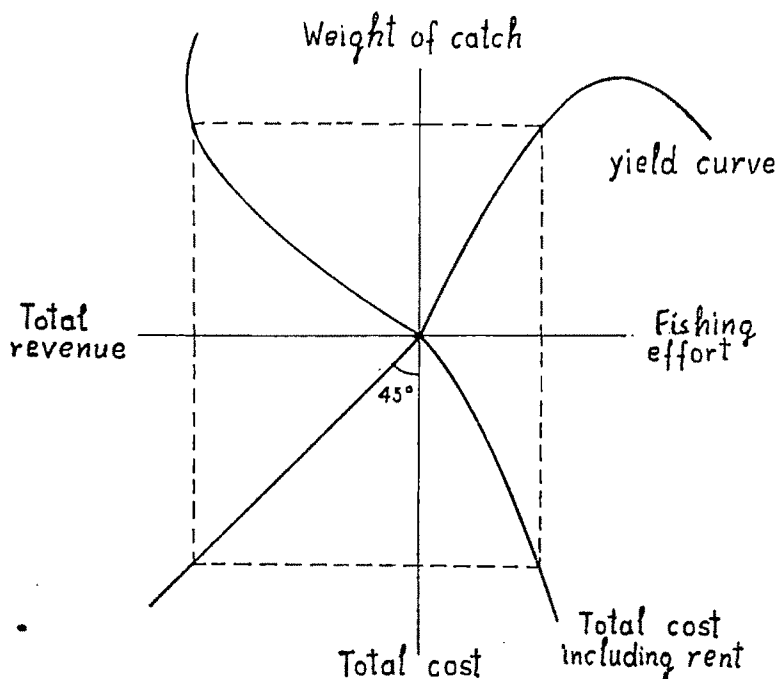


FIGURE 2

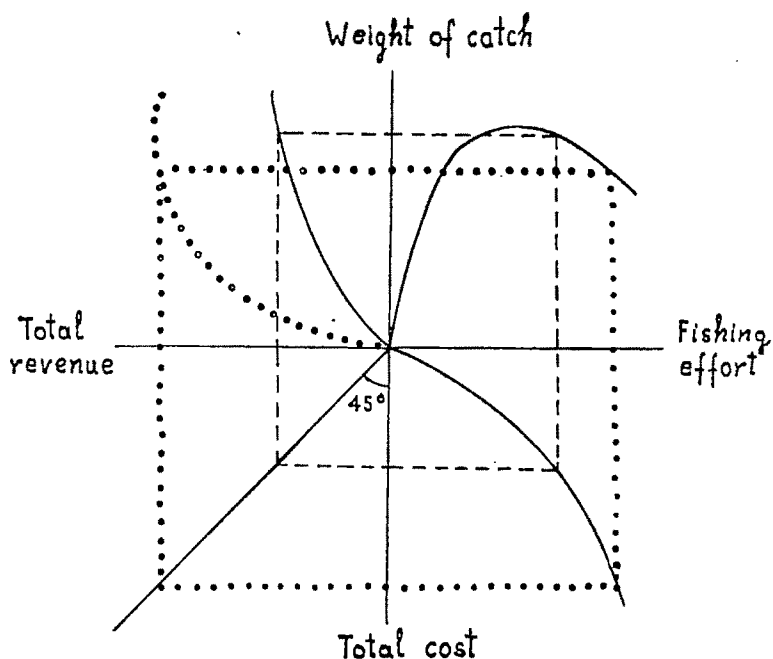


FIGURE 3

of demand means a greater employment of resources in the industry but a smaller total product!

### V. *External Diseconomies*

Such a peculiar result indicates that there is something special about the industry. There is indeed. But from what has been said it should be apparent that two problems are involved, not one. The first is that while the catch of the individual fisherman is proportionate to his own fishing effort, the same is not true of all fishermen together; i.e., the yield curves for all fishermen in Figure 1 are not straight lines through the origin.<sup>4</sup> Each fisherman imposes an external diseconomy upon his brethren; the marginal private product of his fishing effort exceeds the marginal social product.

The second problem involves mesh size. By catching small fish, fishermen are reducing the number of large fish to be caught later. If an individual fisherman were to raise his mesh size he would lose by increasing the number of hauls necessary to achieve any given weight of catch. Yet in the long run, his use of a larger mesh may lower the costs

<sup>4</sup> They are straight lines in an underdeveloped fishery where natural mortality is infinite relative to fishing mortality.

of all fishermen together and, if all of them used larger meshes, all would benefit. Here again, social and private product diverge.

### VI. *The Optimum Optimorum*

When external economies are involved both in the level of fishing effort and in the choice of mesh size, it is clear that to achieve the optimum resource allocation requires regulation of *both* these variables.

Let us assume for the present that the conditions for optimal resource allocation are fulfilled in the rest of the economy, so that no problems of "second best" arise. Let us further assume, first, that the effect of changes in the price of fish upon the distribution of real income between fish consumers and others is unimportant and, second, that a level of earnings in the industry which is equal (at the margin) to the earnings those resources could obtain elsewhere is socially acceptable. Finally, let us consider only those cases where the fishery is a very small part of the economy.

Under these conditions, the *optimum optimorum* is reached when  $G$  is maximized,  $G$  being the excess of the value of the catch to consumers over the value to them of the alternative goods and services sacrificed by devoting resources to fishing. Now the value of the catch to consumers is the maximum they would pay rather than go without it, i.e., what they do pay (total revenue) *plus* consumer surplus:  $TR + S$ , the area under the demand curve. The value of goods and services sacrificed is equal to the contribution to production that the resources used in fishing would make if they were not so used and this, on our assumptions, is what they could earn elsewhere. It is therefore measured by the total costs of the fishery *less* the rents of the intramarginal resources,  $TC - R$ . Thus the *optimum optimorum* is to be reached by maximizing:

$$G = (TR + S) - (TC - R).$$

A necessary, but insufficient, condition for maximization is that mesh size be such as to maximize the catch for the actual level of fishing effort. In terms of Figure 1 this means that the fishery must be "on" the dotted envelope curve, known as the "eumetric yield curve" (Beverton and Holt [1]). It is a property of this curve that, unlike the individual yield curves for given mesh sizes, it is asymptotic to the horizontal, thus rising throughout its length.

Figure 4 shows the determination of the optimum. The level of fishing effort,  $OF$ , must be so chosen as to maximize  $G$ . This is achieved when the slope of the  $TR + S$  curve (i.e., price per unit catch) multiplied by the slope of the eumetric yield curve (i.e., marginal social yield of fishing effort) equals the slope of  $TC - R$  (i.e., the marginal cost of

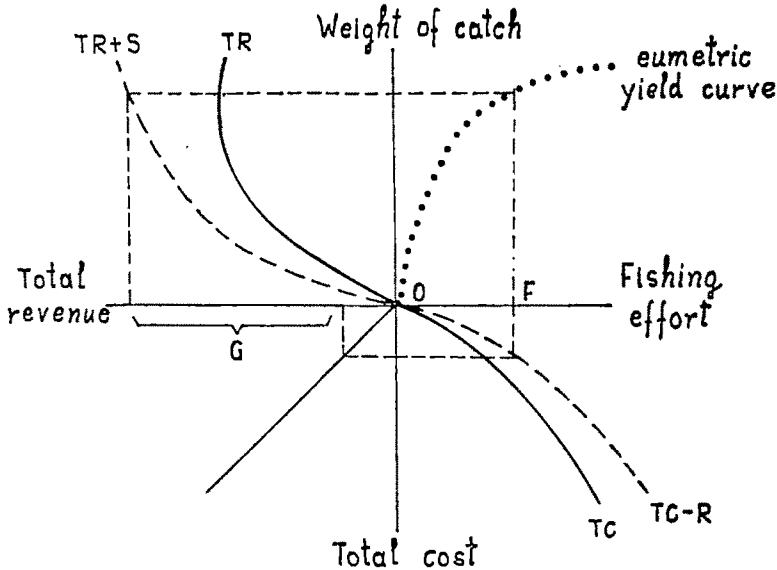


FIGURE 4

fishing effort). When this is the case, i.e., when:

$$\begin{aligned} \text{Price} \times \text{Marginal social yield} &= \text{Marginal cost of fishing effort excluding rent} \\ \text{i.e., Price} \times \text{Marginal social yield} &= \text{Average cost of fishing effort including rent} \end{aligned}$$

it must be true that:

$$\text{Price} \times \text{Average yield} > \text{Average cost of fishing effort including rent}$$

(since the average yield of fishing effort *exceeds* the marginal social yield),<sup>6</sup> which means that total revenue exceeds total costs including rent.  $G = S + R + (TR - TC)$ , the maximum gain, is the sum of consumers' surplus, producers' surplus (the rents of intramarginal labor and capital), and the rent of that other scarce resource, the fish stock.<sup>6</sup>

In an unregulated fishery, where resource allocation is nonoptimal, free entry means that total revenue will equal total costs so that no rent of the fish stock is achieved. Under these circumstances, the total gain from the fishery  $G = S + R$ .<sup>7</sup> Whether a shift from this situation to the *optimum optimum* will raise or lower the total catch (and hence

<sup>6</sup> I.e., the eumetric curve is concave from below.

<sup>6</sup> Cf. H. S. Gordon [2]. Note that if capital and fishermen of equal skill are both in infinitely elastic supply and if the fishery meets a very small part of the total demand,  $R$  and  $S$  respectively will be zero. Nevertheless, the *optimum optimum* still requires regulation of both effort and mesh size.

<sup>7</sup> Which will equal zero in the special case of the previous footnote!

$S$ ) and whether it will raise or lower the amount of fishing effort (and hence  $R$ ) cannot be determined a priori.

### VII. *Suboptimization of Mesh Size*

In order to show this it is convenient to consider the regulation of mesh size and the regulation of fishing effort separately and to show that in some circumstances these may have opposite effects. It is of great importance to consider the suboptima that can be reached by regulating only one or the other of these variables since, in practice, political and administrative considerations frequently make one or the other type of regulation impossible to introduce. This is the case, for example, with the regulation of international trawl fisheries, where agreement upon mesh size is much easier to attain than agreement about the share of fishing effort (and hence catch) to be apportioned to each nation.

When mesh size alone can be regulated, it is obvious that the fishery ought to move on to the eumetric curve, since this is the boundary of the production function. At the original level of fishing effort and costs this will raise the weight of catch but may either lower total revenue (elasticity of demand less than unity) or raise it (elasticity greater than unity). In the first case resources will leave the industry, so that fishing effort is reduced; conversely in the second case. Even in the first case, however, catch will be greater in the new suboptimum equilibrium than it was in the initial nonoptimum equilibrium. If it were not greater, total revenue would be higher or the same as before while fishing effort and total costs would be lower, so that total revenue would exceed total costs—a state of affairs incompatible with equilibrium when entry of resources is unrestricted.

Suboptimization with respect to mesh size thus involves maximizing:

$$G = S + R$$

subject to the constraint  $TR = TC$ . Moving to the suboptimum, equilibrium must raise  $S$ , since catch rises, but may either raise or lower  $R$ . If  $R$  falls, the rise in  $S$  must exceed the fall.<sup>8</sup> But if there were constant costs ( $R$  always zero) and an infinitely elastic demand ( $S$  always zero), there would be no point in regulating mesh size.

### VIII. *Suboptimization of Fishing Effort*

While, as we have just seen, suboptimizing mesh size must raise catch but may either raise or lower fishing effort and costs, suboptimizing

<sup>8</sup> Because  $TR$  and  $TC$  fall equally (by assumption);  $R$  must fall less than  $TC$  (when the supply curves of factors are rising);  $S$  must rise more than  $TR$  falls (since  $TR + S$  rises with movement down any downward-sloping demand curve).

fishing effort will always lower fishing effort and costs but may either raise or lower catch. The analysis of this case follows the same lines as that of the *optimum optimorum* (see Figure 4), save that the yield curve is not the eumetric curve but a constant-mesh yield curve. Now such a curve, as shown in Figure 1, has a maximum. If the initial non-optimum equilibrium is to the left of this maximum, suboptimizing with respect to fishing effort will *reduce* both fishing effort and catch. But if the initial equilibrium is to the right of this maximum, so that the marginal social product of fishing effort is actually *negative*, suboptimization will increase the catch. North Sea plaice and haddock are examples of trawl fisheries where fishing effort is to the right of the maximum for the mesh size in use. The classic case of this is not a trawl fishery but is said to be the Pacific halibut fishery.

How can fishing effort be regulated? The answer is surely to charge fishermen a rent for fishing, so that they economize in the use of the scarce resource. This rent would take the form of a charge per unit of weight of catch proportionate to the difference between marginal private yield (catch per unit of effort) and marginal social yield (the slope of the yield curve). The effect of this would be to raise the marginal private cost of catching fish to equal the marginal social cost.

An alternative method would be to impose quantitative restriction upon the amount of fishing effort. In practice, such restriction would produce an inferior suboptimum, unless it were so all-embracing as to amount to the institution of a sole-owner of the fishery. The reason is that any restriction must be framed in terms of a limit upon one or more of the resources used in the industry. But to ration the supply of any resource is to invite the substitution of other resources for it. Thus if the number of boats were limited, larger engines would be installed, bigger crews carried, and more resources devoted to securing a rapid turn-around in port. None of this would be efficient, in terms of resource allocation; it would simply increase the total costs of all alternative levels of fishing effort. Furthermore, it would concentrate the rent to be had from fishing economically, i.e., the gain from suboptimizing fishing effort, in the hands of the owners of the boats. In some circumstances this would be less desirable from the point of view of income distribution than a charge that placed the rent in the hands of the government which could dispose of it in a variety of ways. Thus a tax on catch is generally necessary for suboptimization. To reach the *optimum optimorum*, however, such a tax is necessary but not sufficient.

There is a great deal more that can be said about the effects of restricting entry to a fishery in ways that lead to economic inefficiency, but I shall not pursue the matter here. My purpose is merely to assert

the fundamental principle that either mesh regulation or the control of fishing effort is better than nothing but that regulation of both is still better.

### IX. *Second Best*

If the assumptions listed at the beginning of the section on the *optimum optimorum* are not met, allowance must be made for this fact in deciding policy. I do not believe that the distributional effects of changes in fish prices are worth bothering about, but there might well be circumstances in which regulation adversely affected the earnings of fishermen to an undesirable extent. Where this resulted from the creation of an excess of total revenue over total costs, the government would acquire more than sufficient funds to compensate the displaced fishermen. Where mesh regulation alone was involved, however, no such funds would be available, and the problem acquires important political elements.

Leaving these distributional questions aside, the recommendation to optimize or, if that is impossible, to suboptimize depends crucially upon the assumptions that the costs of fishing effort do reflect the value of resources in alternative uses and that the price of fish does measure the marginal contribution of fish to consumer welfare. The economist who is concerned to help formulate policy rather than to maintain his theoretical purity will, however, seek to make rough corrections for all the imperfections that he can think of in closely related markets and will then go on to make his recommendations. Thus the level of unemployment in fishing ports and subsidies or tariffs on meat and eggs are likely to draw his attention. He will, on the other hand, remain indifferent to the effects upon fishing costs of restrictive practices among suppliers of marine paint or to the effect upon the demand for fish of resale price maintenance in the confectionery trades.<sup>9</sup>

Even the purists, however, will agree that on resource-allocation grounds there are two things that can be recommended unreservedly. One is a shift to the eumetric curve and the other, as an alternative, is a reduction in fishing effort whenever the fishery is on a falling yield curve. Either of these will raise the catch and/or reduce costs, providing that "something for nothing" which alone appears to gladden the hearts of our erudite welfare economists.

There are complications, however. Even when cost, demand, and natural conditions remain constant through time so that steady state equilibrium can be achieved, the introduction of the regulation of mesh size or fishing effort will initially reduce the catch. This initial, tempo-

<sup>9</sup> Cf. E. J. Mishan [5].

rary loss clearly has to be weighed against the eventual, permanent gain. A rate of social time discount is required to make such weighing-up possible. All this complicates matters, but introduces no interesting new principles.

### *X. Alternative Methods of Regulation*

There is probably no fishery conforming to our simplifying assumptions. It is not worth while removing them one by one in a general discussion such as this, however, since the particular assumptions that should replace them will vary from one fishery to another. Thus, instead of going on to complicate the analysis,<sup>10</sup> I shall conclude by mentioning other forms of regulation than those dealt with so far. The main alternatives that have been applied in practice are: limitation of total catch, closed seasons, closed areas, minimum size limits, and the prescription of particular types of gear. These have in common that they involve no restraint on entry to the industry so that, taken alone, none of them results in the creation of a rent of the fishery. Despite this imperfection, such measures may often be better than no regulation at all, so that if one of them is all that is politically or administratively practicable there is a suboptimum to be found.<sup>11</sup> In some fisheries, two of these measures, size limits and the closure of nursery grounds, are fairly close substitutes for the regulation of mesh size, so that the type of analysis presented above can be applied. With any of them, however, total revenue and total costs will be equal in equilibrium so that the social gain to be had from the fishery is limited to the sum of consumers' and producers' surpluses:  $G = S + R$ .<sup>12</sup>

An important conclusion follows from this, as it did in the case of suboptimizing mesh size. Any measure that raises weight of catch without affecting the total cost curve will raise  $G$ . This is not unambiguously an improvement in a second-best world if resources are drawn into the industry; but if demand has an elasticity of less than unity, so that the increased catch lowers total revenue, resources will leave the industry, and the matter is simpler. Unless the immediate loss outweighs the long-run gain, the only possible objection to getting more fish at a lower cost would be that the resources in the industry deserved the rents they would lose.

<sup>10</sup> For an example, see Beverton and Holt's discussion of fisheries based on two species caught by the same gear but having different eumetric yield curves [1, pp. 388, 421, ff.].

<sup>11</sup> Furthermore, existing measures have initiated cooperation and involved a start on some of the necessary research, without either of which there is no hope of further developments.

<sup>12</sup> We are still assuming that the fishery is only a small part of the economy.

XI. *Conclusion*

I have not attempted any general survey of the economics of fishery conservation, though I hope that this article may serve as an introduction to Professor Scott's paper on the subject and Professor Crutchfield's case study of the Pacific halibut fishery, both in the proceedings of the Ottawa conference [8]. Although my exposition is mainly in terms of trawl fisheries, it can be applied more generally to other fisheries when selectivity is controllable. My purpose has been to provide an interesting and important example of the notions of optimization and suboptimization and of the proposition that coping with external diseconomies will sometimes involve interfering with the nature as well as the scale of private productive activities [6].

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## BUSINESS FIRM GROWTH AND SIZE

By YUJI IJIRI AND HERBERT A. SIMON\*

It is well known that highly skewed frequency distributions, similar to the observed distributions of sizes of business firms, can be generated by a number of related stochastic processes. All of the processes have at their core something like Gibrat's law of proportionate effect—the postulate that expected rate of growth is independent of present size. Several of these processes have been offered as explanations of the observed size distributions.

Stochastic explanations for the size distribution of firms have considerable interest for economic theory and policy. They interpret these distributions in terms of the dynamics of the growth process rather than in terms of static cost curves. If the assumptions on which they rest are correct, the models call for new statistical measures of the degree of concentration and new interpretations of the economic implications of concentration. These implications have been discussed in an earlier article [4]. The present article is concerned with demonstrating that certain assumptions, known to be incompatible with the empirical data, that were used in the derivation of the earlier model (or in the description of that model in [4]) can be replaced by weaker and more realistic assumptions.

The adequacy of any theoretical explanation can be judged on two grounds: (a) the plausibility of its assumptions, their agreement with known facts; and (b) the goodness of fit of the derived distributions. Evaluating the goodness of fit of mathematical models like the ones under consideration here involves all of the theoretical difficulties of testing extreme hypotheses. Since these difficulties, although well known to mathematical statisticians, have received little discussion in the literature, we should like to pause to indicate their nature.<sup>1</sup> Suppose we wish to test Galileo's law of the inclined plane—that the distance,  $s(t)$ , traveled by a ball rolling down the plane increases with the square of the time:  $s(t) = kt^2$ , where  $k$  is a constant. We perform a large series of careful observations, obtaining a set of  $[s, t]$  pairs from which we es-

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<sup>1</sup> A succinct discussion with references will be found in [2, pp. 254–56].

timate  $k$ . The actual observations cluster closely around the fitted curve, but do not, of course, all fall exactly on it. To decide whether we have confirmed or refuted Galileo's law, we test whether the observed deviations of the observations from the fitted curve could have arisen by chance. Suppose the answer is that they could. Then we may conclude either (a) that Galileo's law is correct (i.e., at least not incompatible with these data) or (b) that our observations were not accurate enough to reveal its inadequacies. The more random "noise" there is in our data, in fact, the more likely that we will decide the law has been "confirmed."

Suppose, on the other hand, that the statistical test rejects the hypothesis. Then we may conclude either (a) that Galileo's law is substantially incorrect or (b) that it is substantially correct but only as a first approximation. We know, in fact, that Galileo's law *does* ignore variables that may be important under various circumstances: irregularities in the ball or the plane, rolling friction, air resistance, possible electrical or magnetic fields if the ball is metal, variations in the gravitational field—and so on, ad infinitum. The enormous progress that physics has made in three centuries may be partly attributed to its willingness to ignore for a time discrepancies from theories that are in some sense substantially correct.

Since no one has ever formalized the criteria for ignoring discrepancies of this kind, and since the received body of statistical theory provides no suitable means for testing extreme hypotheses, we shall not test statistically the goodness of fit of the stochastic models. In the case of size distributions of firms, the observed distributions certainly "look like" Pareto, Yule, or log normal distributions, but there is no known satisfactory way to objectify the degree of resemblance. Since the observed distributions are radically different from those we would expect from explanations based on static cost curves [4], and since there appear to be no existing models other than the stochastic ones that make specific predictions of the shapes of the distributions, common sense will perhaps consent to what theory does not forbid—accepting the stochastic models as substantially sound.

Under these circumstances, however, our confidence in the proposed stochastic explanations of the size distributions may depend quite as much on how plausible we find the assumptions underlying the models as on our judgments of goodness of fit. If the assumptions are very strong, and particularly if they contradict known facts about business size and growth, we will be inclined to dismiss the stochastic explanations. If the assumptions are weak, and consistent with our empirical data, our confidence in the stochastic models will be correspondingly strengthened.

In Section I of this paper, we shall describe briefly the simplest stochastic models that have been used to derive the size distribution of firms and shall point out in what respects the assumptions incorporated in these models are contradicted by the empirical facts. In the following section we shall propose a model that removes what is perhaps the least acceptable assumption in the simple models: the assumption that the growth rates of individual firms in one period of time are uncorrelated with their growth rates in preceding periods. In the remainder of the paper, we shall examine the distributions that are generated by the improved model.

### I. Simple Stochastic Models

The simplest kind of stochastic process that will yield skew distributions like the observed ones is based on the following assumption:

*Year-to-year changes in firm sizes are governed by a simple Markoff process in which the probabilities of the size changes of any specified percentage magnitudes are independent of a firm's present absolute size.* That is, each firm, under this assumption, has the same probability as any other firm of increasing or decreasing in size by 5 per cent, 10 per cent, or any other relative amount. This is Gibrat's law in its simplest and strongest form.

Let  $i$  be a measure of firm size—total assets, say—and  $F(i)$  be the number of firms of size  $i$  or larger. (Thus, if there is only one firm of exactly size  $i$ , for all  $i=1, 2, \dots, n$ ,  $F(i)$  will be the rank of that firm in the industry.) Then, the process we have just described leads to an equilibrium size distribution which for large  $i$  is approximately:

$$(1) \quad F(i) = Ai^{-(\rho+1)}$$

where  $\rho$  is a constant that depends on the rate at which new firms enter the industry, and  $A$  is another constant. (For details, see [3] and [4].) Function (1) is the Pareto distribution and is also, for large  $i$ , an approximation to the Yule distribution. Converting to logarithms, it yields the linear relation:

$$(2) \quad \log F(i) = -(\rho + 1) \log i + \text{a constant.}$$

The observed size distributions of business firms fit equation (2) rather well (see [4]). However, distributions that are quite similar can be derived from processes based on weaker assumptions. First of all, it has been shown that we can replace the strong form of Gibrat's law, stated above, with the assumption that the expected percentage change in size of the *totality of firms in each size stratum* is independent of stratum. On this weaker assumption, the Yule, Pareto, or lognormal distributions considered in [4] can still be derived. (See [3] for more detailed discussion.) The stochastic model based on the weaker assump-

tion is obviously also consistent (as the model based on the stronger assumption is not) with the following well-known facts:

1. The transition matrices for changes in firm size from year to year show different relative (i.e., percentage) variance for the firms in different size groups—the relative variance decreasing with increase in size. Only the expected percentage changes for the totality of firms in a size group are independent of size. (See Mansfield [1]).

2. The expected rates of change are certainly not equal, or nearly equal, for all individual firms. The simplest way to show this is to examine substrata for differences in expected rate of change—for example, substrata corresponding to different industry groups.

It is not clear, however, that the weaker assumption is consistent with a third fact:

3. The expected rates of change for individual firms are not independent of changes in the prior years—there is serial correlation in the growth rates over at least short time periods.

The main purpose of the study reported here was to see whether a stochastic process based on some variant of Gibrat's law, but allowing for serial correlation from one period to another in the growth rates of individual firms, could also lead to distributions like those actually observed. If such a process could be found, it would open an additional direction for the generalization of stochastic explanations of firm size to fit a wider range of empirical findings. If it could not, grave doubt would be cast on the adequacy of stochastic models of this general kind for explaining the size of business firms.

We leave to later investigation some other important directions of generalization. In particular, the models described here do not admit mergers or decreases in size of individual firms. Our intuitions suggest that so long as the probabilities of merger or decline in size are roughly independent of stratum, they will not change much the equilibrium distributions, but this conjecture remains to be tested in future work.

## II. *A Model with Serial Correlation*

The implications of serial correlation in firm growth are not easy to trace analytically. Stochastic models admitting serial correlation have proved to be too complex to be solved explicitly in closed form for the equilibrium distributions. To cast some light on the question of whether a stochastic process with serial correlation could lead to distributions like those actually observed, we have carried out some Monte Carlo calculations with a class of processes of this kind.

In the models to be discussed here, the identity of each individual firm is maintained from one time period to the next. The change in size

of each firm is governed by a stochastic process, which depends on the size to which the firm has grown, but also upon the times at which its growth has taken place. For simplicity in our computations, we assume growth to take place in increments of unit magnitude. The probability that a firm will experience an increment in size during the next time period is assumed proportional to a weighted sum of the increments it has experienced in the past, where the weight of an increment decreases geometrically at a rate  $\beta$ , with the lapse of time since its occurrence.

We formalize these notions as follows: Let  $x_j(t)$  be the change in size of the  $j^{\text{th}}$  firm during the  $t^{\text{th}}$  time interval, where  $x_j(t)$  is either unity or zero (the firm either experiences a unit increment in size or remains the same size during any given time interval). Then the size of the  $j^{\text{th}}$  firm at the end of the  $t^{\text{th}}$  interval is simply:

$$(3) \quad \sum_{\tau=1}^t x_j(\tau).$$

The expected increment in size of the  $j^{\text{th}}$  firm during the  $(t+1)^{\text{st}}$  interval is:

$$(4) \quad w_j = p[x_j(t+1) = 1] = K(t) \sum_{\tau=1}^t x(\tau)\beta^\tau$$

where  $K(t)$  is a function of time that is the same for all firms, and  $\beta$  is the fraction that determines how rapidly the influence of past growth on new growth drops out.

Under these assumptions, large firms will, *ceteris paribus*, grow proportionately more rapidly than small firms—the *ceteris paribus* assumption being that the previous growth of the firms being compared took place at about the same times. On the other hand, firms that have experienced recent growth will grow more rapidly, *ceteris paribus*, than firms of the same size whose growth took place earlier.

We complete our model by adding assumptions about the total number of increments per time period, and the rate of entry of new firms. For simplicity in illustrating the behavior of models of this kind, we shall select the unit time interval so that there is exactly one increment per time period in the total assets of the entire industry. We shall assume that there is a constant probability,  $\alpha$ , that this increment will be allocated to a new firm (which is therefore assumed to be one unit in size at the outset).<sup>2</sup> Under these assumptions, new firms will enter at an expected rate proportional to the rate of growth of the industry.

<sup>2</sup> Thus the size unit may be interpreted as the minimum efficient size of a firm in the industry (see [2, p. 608]); and a firm of, say, 15 units may be thought of as 15 times the minimum efficient size.

### III. *Details of the Simulation Procedure*

Before we present some of the statistical results, we should like to describe in more detail exactly how the simulation of the model just described was carried out. Readers who are not primarily interested in the procedure may prefer to skip the present section.

Suppose we wish to simulate the growth of an industry from its birth to an aggregated asset size of  $N$  units. (As suggested above, a "unit" may be interpreted as the total assets required for a firm of minimum efficient size in the industry.) We allocate the  $N$  units of assets one at a time; and for convenience, we shall describe the process as if the total size of the industry grows by one unit in each "time period." Stated otherwise, we take as the  $t^{\text{th}}$  time unit ( $t=1, 2, \dots, N$ ) the period during which the industry grows from  $(t-1)$  to  $t$  in total assets. This  $t^{\text{th}}$  unit of assets is assigned to some particular firm, either a new firm (which then achieves a size of one unit) or an existing firm (which then grows by one unit during the  $t^{\text{th}}$  time period).

The allocation of the  $t^{\text{th}}$  unit is made in two stages:

*Stage I.* This stage determines whether the unit is to be allocated to a new firm or an existing firm. We first draw a random number,  $r_1$ , from a rectangular distribution between 0 and 1. If  $r_1 \leq \alpha$ , where  $\alpha$  is a given constant, we create a new firm, the  $n(t)^{\text{th}}$  firm, where  $n(t)$  is the number of firms generated during the first  $t$  periods. We assign the  $t^{\text{th}}$  unit of assets to this new firm, thus completing the allocation process for the  $t^{\text{th}}$  time period without going through Stage II. (Note that the total number of firms,  $n(t)$ , has been increased by one:  $n(t) = n(t-1) + 1$ .) If, however,  $r_1 > \alpha$ , then we assign the  $t^{\text{th}}$  asset unit to one of the existing firms, as determined by Stage II.

*Stage II.* For each firm in the industry, we keep track of two factors: the current *size* of the firm, and the current *growth potential* of the firm. By the size of the  $j^{\text{th}}$  firm at the end of the  $(t-1)^{\text{th}}$  time period,  $i_j(t-1)$ , we mean the total number of asset units that have been allocated to the  $j^{\text{th}}$  firm up to that time. By the growth potential of the  $j^{\text{th}}$  firm at the end of the  $(t-1)^{\text{th}}$  time period,  $w_j(t-1)$ , we mean a weighted sum of the asset units that have been allocated to that firm up to that time. The weights are assigned as follows: If we take as 1 the weight of the asset unit assigned during period  $(t-1)$ , then the weight of the unit assigned during period  $(t-k)$  will be  $\beta^{(k-1)}$ , where  $\beta$  is a proper fraction. That is, prior growth is assumed to create potential growth at a rate that falls off geometrically with the lapse of time since the prior growth occurred. We also keep track of the sum of the growth potentials for all firms:  $w(t-1) = \sum_j w_j(t-1)$ .

To assign the  $t^{\text{th}}$  asset unit to a firm, we draw another random number

$r_2$  from a rectangular distribution between 0 and  $w(t-1)$ , and assign the unit to the  $k^{\text{th}}$  firm, where  $k$  is the largest integer which satisfies

$$\sum_{j=1}^k w_j(t-1) \leq r_2.^3$$

Then, the probability that the asset unit will be assigned to any particular one of the existing firms is proportional to the weight of that firm.

Thus, at the end of the  $N^{\text{th}}$  period, all  $N$  units of the assets of the industry will have been allocated among  $n(N)$  firms. The distribution of assets is given by  $i_j(N)$  ( $j=1, 2, \dots, n(N)$ ). Then  $F(i)$  is the number of firms whose size at the end of the  $N^{\text{th}}$  period is greater than or equal to  $i$ .

Since Stage II of the process requires that there already be some firms in existence, the scheme is started off by providing a small initial population of firms. The initial conditions used in most of our simulations specified three already existing firms, one of 5 units, one of 3 units, and one of 1 unit. The final distribution is not entirely independent of the initial conditions, but tends to become independent as  $N$  grows large. (Except for the few largest firms, the distribution is very insensitive to the initial conditions even for a relatively small  $N$ .)

#### IV. *A Distribution Generated by the Model*

As a first example of a distribution generated by this process, we consider a sample of 247 firms whose aggregate size,  $N$ , is 1,000 asset units, or an average of about 4 units per firm.<sup>4</sup>

The weights were reduced at a rate of 5 per cent per time period [ $\beta=.95$ ]. Table 1 shows the frequency distribution of firms by size. The largest firm is 56 units. At the other end of the scale, there are 110 firms of one unit each. As is characteristic of these models, the distribution is highly skewed and the cumulative distribution is approximately linear on a double log scale (Figure 1). Thus, the introduction of the weights did not change the general character of the equilibrium distribution.

<sup>3</sup> For reasons of speed, the actual computation is a little different. Instead of recomputing each of the weights each time period, we simply increase the *new* weights geometrically with time. Thus the asset unit allocated during period  $t$  is assigned a permanent weight of  $\gamma^t$  where  $\gamma=1/\beta$ . Clearly, this leads to exactly the same probability of each firm being allocated the new asset unit, for all weights, under the modified scheme, are multiplied by the same factor,  $\gamma^t$ .

<sup>4</sup> In generating this particular sample,  $\alpha$  was not strictly constant but was set at  $n(t)/t$ . (The usual initial conditions, described in the previous section, were imposed to start the process off.) In generating the other samples reported here,  $\alpha$  was fixed at a constant value. This change in assumption did not noticeably change the qualitative characteristics of the sample.

The final weights, after 1,000 time periods, were very far from proportional to the sizes of the individual firms. A mere 16 firms, out of the total of 247, accounted for almost all the potential for future growth. Each of these 16 firms had 1 per cent or more of the total of weights. The remaining 231 firms had, *in the aggregate*, only about 1 per cent of the total weight. Thus, at  $t=1,000$ , there was about one chance in four that the next increment would go to a new firm, since  $\alpha=.247$ ; if it did not, there were 99 chances in 100 that it would go to one of the 16 firms with non-negligible weights. These sixteen firms ranged over the whole size distribution: one was of size 39, one of size 12, one of size 7, one of size 5, two of size 4, four of size 3, and three each of sizes 2 and 1. In fact, these sixteen "growing" firms formed a skew distribution of almost the same shape as the entire distribution of 247 firms.

We see that Gibrat's law was very far from being satisfied for the individual firms. However, the growth potential was widely distributed over the size strata. In Table 1 we also show (column 4) the aggregate

TABLE 1—DISTRIBUTION OF FIRMS BY SIZE IN A SIMULATED "INDUSTRY" OF 247 FIRMS

Firm Size (1)	Number of Firms (2)	Weight $w_j$ (3)	Cumulative Weight $\Sigma w_j$ (4)	Cumulative Per cent of Total Assets (5)
56	1	—*	—	.056
54	1	—	—	.110
52	1	—	—	.162
39	1	.271	.271	.201
28	3	.002	.273	.285
23	1	—	.273	.308
22	1	—	.273	.330
21	1	—	.273	.351
18	1	—	.273	.369
15	1	—	.273	.384
13	1	—	.273	.397
12	2	.102	.375	.421
11	5	.005	.380	.476
10	1	—	.380	.486
9	1	—	.380	.495
8	2	—	.380	.511
7	7	.025	.405	.560
6	7	—	.405	.602
5	10	.025	.430	.652
4	18	.132	.562	.724
3	24	.207	.769	.796
2	47	.181	.950	.890
1	110	.050	1.000	1.000
Total	247	1.000		

\* Less than .001

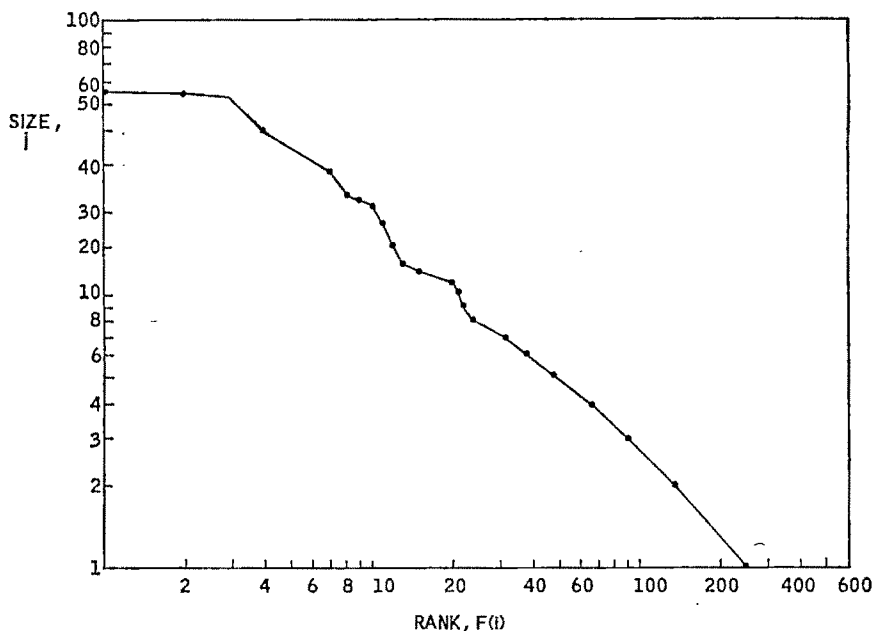


FIGURE 1. RANK-SIZE DISTRIBUTION OF SIMULATED "INDUSTRY" OF 247 FIRMS

of weights for all firms above a given size, and (column 5) the aggregate size of all firms above a given size. Thus, firms of size 28 and larger account for 27.3 per cent of the total weight, but 28.5 per cent of the total "assets." Their growth potential, in the aggregate, was therefore only slightly less than that postulated by Gibrat's law. The corresponding figures for firms of size 10 and larger are 38.0 per cent and 48.6 per cent, respectively; for firms of size 5, 43.0 per cent and 65.2 per cent, respectively; and for firms of size 3, 76.9 per cent and 79.6 per cent, respectively. Middle-sized firms, then, show less than average growth potential, firms of size 2 to 4, greater than average potential, firms of size 1, less than average.

#### V. *The History of a Simulated Industry or Economy*

To get a better understanding of the growth patterns produced by the model, we made additional runs, recording the sizes of individual firms at successive time intervals. Data are presented in Table 2 from a run of 1,000 time units, with  $\alpha$ , the probability of assigning the next increment to a new firm, set at .2; and  $\beta$ , the rate of discounting past growth, set at .95.

The table shows the sizes, at each 100 time intervals, of the 20 firms (out of a total of 209) that reached a final size of more than ten units. A number of interesting observations can be read from this table. Let

us call the 20 firms whose histories are recorded in the table the "large" firms. First, older firms had not much greater chance of becoming large than younger firms. Eleven of the large firms entered the system in the first half of the time interval, and nine in the second half—an insignificant difference. There was some underrepresentation, among the large firms, of the very youngest; only three firms among the youngest 60—i.e., the firms numbered 150 to 209—had reached the large size.

If we consider the eight largest firms, those of size 20 and over, there is a little more relation between size and age. The very largest firm was the first to enter the system, and three other very large firms were among the first 50 to enter. On the other hand, the second and third largest firms are relative newcomers, having appeared during the last 40 per cent and 30 per cent of the system's history, i.e., after times 600 and 700, respectively.

We see, further, that firms which grow large experience most of their growth during the first 200 or 300 time periods after they enter

TABLE 2—GROWTH PATTERN OF 20 LARGEST FIRMS IN A SIMULATED "INDUSTRY" OF 209 FIRMS

Firm Number (in order of entry)	Period Ending at Time:									
	100	200	300	400	500	600	700	800	900	1000
1	39	61	78	98	107	123	126	126	126	126
3	19	26	26	26	26	26	26	26	26	26
14	7	11	27	27	27	27	27	27	27	27
50			4	13	28	29	29	29	29	29
58			1	9	16	16	16	16	16	16
75				6	11	11	11	11	11	11
78				6	11	11	11	11	11	11
81				1	10	11	11	11	11	11
97					3	20	24	24	24	24
99					1	13	13	17	18	18
101					1	14	15	15	15	15
111						4	12	15	15	15
118							8	20	20	20
119							10	11	11	11
125							11	15	15	15
131							6	14	17	17
134							9	25	34	55
151								5	20	34
176									7	13
184									5	11
Total Number of Firms	24	43	61	81	103	118	141	166	189	209

TABLE 3—GROWTH AND GROWTH POTENTIAL OF LARGEST FIRM IN A SIMULATED "INDUSTRY"

Period Ending	Firm Size	Weight— $w_i$
100	39	.273
200	61	.306
300	78	.223
400	98	.143
500	107	.118
600	123	.257
700	126	.007
800	126	.000*
900	126	.000*
1000	126	.000*

\* Less than .001.

the system, then reach a plateau. The very large size of Firm 1 is associated with an abnormally long period of rapid growth (See Table 3 and Figure 2). The firm grew at a virtually constant percentage rate until its growth stopped. Table 3 shows how, during its period of growth, its weight (probability of being chosen for the next increment) remained in the range .1 to .3. When the weight dropped, virtually to zero, during the seventh period, the growth stopped abruptly.

We do not wish to defend in detail the realism of the assumptions of the model or of the histories it produced. The time discount rate,  $\beta$ , was almost certainly too high—underestimating the advantage of established firms—as compared with most real-life situations. What is interesting, however, is that a stochastic process that admits this very strong effect of recent growth in the determinants of future growth still produces the familiar kinds of skewed equilibrium distributions. Clearly, Gibrat's law does not have to be assumed in any of its strong forms to produce this result.

#### VI. *Effects of Parameters on the Distribution*

A substantial number of additional simulation runs, varying the parameters  $\alpha$  and  $\beta$ , provide information about the dependence of the equilibrium distribution upon these parameters. All of the cumulative distributions have a strong resemblance to that depicted in Figure 1. They are almost linear on a log scale. As  $\alpha$  increases, the rank-order distributions (the distributions of  $F(i)$ , see Figure 1) acquire somewhat smaller initial slopes,<sup>6</sup> and become slightly more convex to the origin. As  $\beta$  decreases, the distributions again become considerably more con-

<sup>6</sup> By initial slope we mean the angle of the curve to the horizontal axis at the bottom of the graph whose ordinate is  $\log i$  and whose abscissa is  $\log F(i)$ .

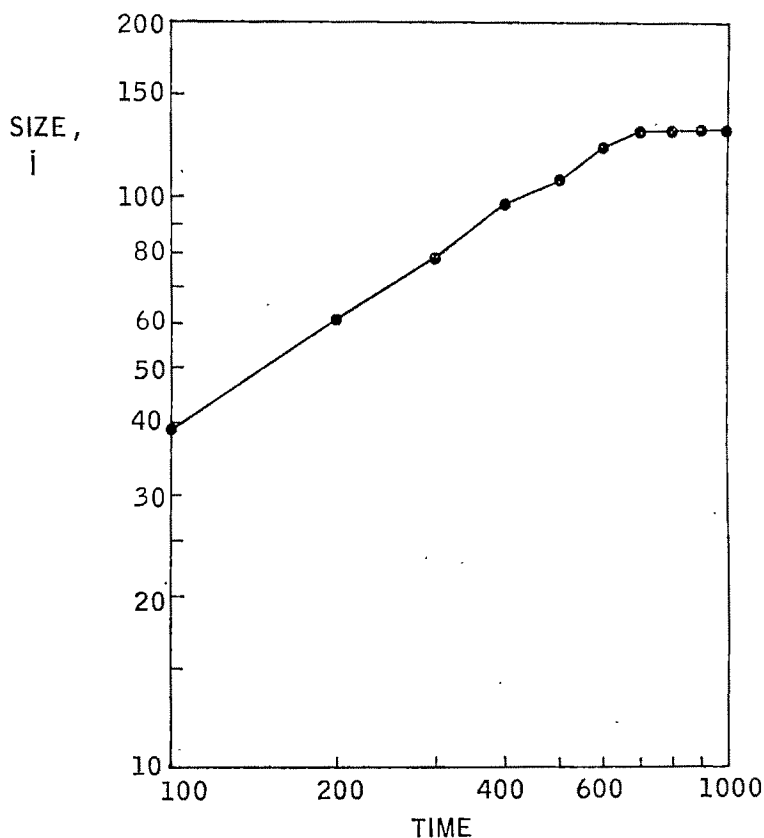


FIGURE 2. GROWTH OF LARGEST FIRM IN A SIMULATED INDUSTRY

vex to the origin, but acquire slightly *larger* initial slopes. The reciprocals of the slopes correspond to the parameter  $-(\rho+1)$  in the equations (1) and (2). For the case where  $\beta=1$ , it can be shown that  $\rho=1/(1-\alpha)$ . The parameter  $-(\rho+1)$  can be estimated, in the neighborhood of  $i=2$ ,

TABLE 4—ESTIMATES OF  $-(\rho+1)$ , FOR VARIOUS VALUES OF  $\alpha$  AND  $\beta$   
 $[-(\rho+1)=d \log F(i)/d \log i]$

$\alpha$	$\beta$			
	.95	.99	.999	1
.1	.65	.91	.86	.99
.2	.81	.9	1	1.2
.4	1.15	1.2	1.2	1.4

by the equation:

$$(5) \quad -(\rho + 1)_2 = \frac{\log F(1) - \log F(2)}{\log 2}.$$

The values of  $-(\rho + 1)_2$  for various combinations of  $\alpha$  and  $\beta$  are given in Table 4. They range from .65, for  $\alpha = .1$ ,  $\beta = .95$ , to 1.4 for  $\alpha = .4$ ,  $\beta = 1$ .

### VII. Conclusion

In this paper we have described a new stochastic process, and have shown, by numerical simulation, that it generates equilibrium distributions that closely resemble the Yule distribution. In this process the expected growth rates of individual firms are assumed proportional to weights, where the weights are the time-discounted sums of previous increments in size. Thus the process incorporates a significant modification of the law of proportionate effect that allows substantial differences in the expected growth rates of individual firms.

The behavior of the new model is consistent with the empirical observation that the growth of firms, deviating from the predictions of a simple one-period Markovian process, exhibits strong serial correlation in the size changes of individual firms. We can now confidently predict that many other processes, incorporating comparably weak forms of the law of proportionate effects, will lead to highly similar equilibrium distribution. Thus our analysis greatly increases the plausibility of a stochastic explanation for the observed distributions of firm sizes.

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# COMMUNICATIONS

## The Unimportance of the Embodied Question

In a recent article in this journal, Moses Abramovitz noted that Robert M. Solow and I obtain different estimates of the effect on economic growth of alternative investment rates because of different appraisals of the importance of embodiment of new knowledge in capital goods. Abramovitz suggested "an issue of first-rate importance is, therefore, posed" [1, p. 773]. Recent literature seems to imply that the issue can be resolved by discovering what fraction of developments that can potentially raise productivity must be embodied in capital. I wish to suggest here that the whole embodiment question is of little importance for policy in the United States; that the issue is not usefully formulated in terms of the fraction of progress that is embodied; and that the right question can't be answered beyond a showing that it is unimportant.

The argument that the question was of no great consequence that was given in my *Sources of Economic Growth* may be stated as follows [2, pp. 234-37, 254-55]. The embodiment effect operates through the age distribution of the gross capital stock of business. This distribution can be summarized accurately enough by average age, provided weights of different types of capital goods (particularly structures and equipment) are held constant. Suppose then the growth rate of output per man is 1.6 per cent a year, of which .6 per cent is due to advances in knowledge (my estimates for the 1929-57 period). Suppose we can change the average age by one year from 1960 to 1970.<sup>1</sup> In so far as productivity-changing developments are embodied, we will then be working in 1970 with the technology that we would be using in 1971 if average age did not change. If nothing is embodied, the change in average age of capital will not affect output. If all advances of knowledge but not other productivity-affecting developments are embodied, 1970 output will be .6 per cent higher than if nothing is embodied and the 1960-70 annual growth rate .06 percentage points higher. If *everything* affecting output per man favorably or unfavorably is embodied, 1970 output will be 1.6 per cent higher as a result of the reduced age of capital and the annual growth rate .16 percentage points higher. If half of either is embodied, the amounts will be half as large. After 1970 the growth rate will be the same as otherwise unless the average age is again changed. Thus the most extreme assumptions yield annual growth rates, calculated over a ten-year period, that differ by .16 percentage points for each year that the average age of capital changes; thereafter, for as long as the new average age is maintained, the levels of national income differ by a constant 1.6 per cent.

The force of the calculations depends on the fact that average age is a

<sup>1</sup> If average age (and the rate at which knowledge advances) does not change between dates or assumed situations, the calculated growth rate is the same whether advances are or are not embodied in capital.

variable that does not and can not vary much under normal conditions, so that one year is a big change. This is especially the case with equipment, for which the age level is short and observed variation small. Commerce Department calculations [5, p. 17] based on Bulletin F lives give an average age for equipment of 7.2 years in 1961 and a range of 1.5 years during the period since 1929. An alternate calculation based on assumed lives 20 per cent shorter gives an average age of 5.6 years and range of 1.2 years. Ranges even as large as these were possible only because of the extreme variation in gross capital formation due to the depression of the 'thirties and war.

Equipment, quite reasonably, is usually stressed, but some attention is given to structures. Their age is longer and the range greater. If structures and equipment are weighted by value of gross stock (surely the maximum weight that can be assigned to structures), based on an average of 1929 and 1961 weights in 1954 prices, extreme ranges of 2.6 and 3.6 years are given by the alternative estimates. Both extreme ranges include 1945. From 1929 to 1961 the change was only .3 years by one estimate and 1.0 years by the other.<sup>2</sup>

Such data illustrate how insensitive is the age of equipment, and even of plant and equipment, to big swings in the rate of gross investment. Edmund Phelps has also shown that average age is independent of the equilibrium (or average) level of capital formation and tends to return to an equilibrium point, so that sustained changes are hard to obtain [6]. This is borne out by the actual data.

The purpose of the present note is to argue that, though such calculations and considerations themselves assign no large importance to the embodiment question unless rates of technological progress well outside the bounds of experience are assumed, they overstate its significance by what I should judge to be an enormous amount. To speak of technological progress embodied in capital is simply to refer to changes in the quality of capital goods. The index of quality change in capital goods is the quotient of indexes of the quantity of capital goods when capital of different vintages is equated (1) by ability to contribute to production, and (2) by production cost at a common date.<sup>3</sup> Ordinary observation indicates that the amount of quality change in any time period must vary enormously among different types of equipment or structures, ranging from zero for many types of capital that aren't changed to huge amounts for a few types. The profitability of replacing any capital good (or complex of capital goods) before it is physically worn out depends directly upon the extent to which its quality falls short of the quality of its potential replacement. If an industry invests \$50 million, profit maximization will lead it to replace those types of capital in which quality improvement (hence obsolescence) is greatest. An extra \$10 million will go to capital in

<sup>2</sup> These average lives are computed from stock estimates derived by the perpetual inventory method. Lives were probably extended during the war, so the divergence between 1945 and more normal periods is probably understated.

<sup>3</sup> That is, quality change is the quotient of indexes of capital computed by methods 3 and 1 in the language of an earlier article [3], where the meaning of these indexes is specified more fully.

which quality improvement is smaller, and so on. Moreover, a particular type of capital good acquired for replacement will be applied first to uses where the gains from replacement are greatest. Also, the mix of capital goods, and consequently the average quality change, varies greatly among industries; hence so does the average profitability of replacement of capital. Industries in which average quality of new capital goods changes most will, other things equal, have the highest replacement rate. Any reasonable amount of gross investment in the economy will already provide for taking up those investment opportunities arising because quality change in capital goods is large: i.e., within firms and industries, large relative to all capital used; and, as among industries, large relative to the average industry.

It follows that if we suppose \$50 billion of investment in the economy will embody 25 per cent more technological progress than \$40 billion—an assumption which would be consistent with calculations made earlier based on average lives—we shall have a radical overestimate. Within ranges of gross investment worth talking about—that is, above some reasonable minimum, certainly above the point of no net capital formation—I should think the additional amount of technological progress that will be incorporated into production at a higher investment rate rather than a lower rate must be very small relative to the amount incorporated at the lower rate. We can legitimately take only a small fraction of the .16 previously computed as the difference between the 10-year growth rates if everything or nothing is embodied (given a one-year change in average age) because that calculation assumed quality change on incremental investment to be the same as on average gross investment. All the calculations I have seen on embodiment effects assume the same rate of quality improvement for all capital goods, hence are great overestimates.<sup>4</sup>

The same reasoning disputes the importance of the idea that a high rate of gross capital formation is necessary in order to get radically improved equipment into use quickly so that, with experience in its use, further technological advance will be accelerated. Any attention to rates of return will tell us that introduction of capital goods representing radical improvement over those previously available will be made at even a low rate of gross capital formation. There may be cases of radically improved capital goods whose merit is not obvious to potential buyers, so that attachment of a high risk premium makes their introduction marginal, but the point hardly deserves much weight.

I have explained why the embodiment question appears to me unimportant in the United States at present. The same reasoning shows it would not be helpful to know the fraction of “technological progress” that is, or must be, embodied. We are dealing not with a two-way classification but with a curve of rates of return. Some innovations (the purely unembodied) require no gross investment, and their introduction, if computed against capital cost (other costs may be involved), yields an infinite rate of return. Others (I suspect a great many) require some trifling readjustment of existing capital or insignificant capital expenditure to achieve a substantial continuing gain; these imply a near-infinite rate of return on capital if we compare the effects

<sup>4</sup> In addition to Solow [7] and myself, I may refer specifically to W. A. Eltis [4] and Phelps [6].

of making or not making the capital expenditure, but will be made routinely with scarcely a thought to capital cost or total capital budget. In a two-way classification these are embodied, but the fact is of no significance. We then move down to those changes requiring appreciable investment, which must be expected to yield rates of return ranging from very high down toward zero.

If one seriously wished to measure the embodiment effects of having (say) \$50 billion instead of \$40 billion of gross investment a year, he would need (1) to specify the composition of the extra \$10 billion in terms of types of capital goods; and (2) to measure the quality improvement in these capital goods. The first is difficult, the second, I have argued [3, pp. 229-34] and am convinced, impossible.

Calculations that assume (1) that quality improvement in capital is necessary for all productivity increase and (2) that quality improvement in (say) the first \$40 billion of gross investment is no larger, per dollar, than in the next \$10 billion, yield results that are, I believe, enormously too large. They are useful only because they yield such small numbers that we can deduce that the true figures must be insignificant under normal circumstances in the United States.

There is an important reservation to universal application of the conclusion that it makes little difference to analysis whether or not one takes account of embodiment effects. It need not hold in countries where (or times when) capital goods are used much longer than in the present-day United States (hence have more room for variation) or their age distribution is greatly distorted and where (or when) tendencies toward equalizing rates of return are (or recently have been) extremely weak because of lack of competition or because of controls on investment. These conditions existed to a degree in the United States in the immediate postwar years, their effects on short-term productivity changes perhaps being important until around 1950. Because specific investment controls during the war and wartime demand patterns prevented the introduction of new capital in the industries serving postwar markets, at war's end there presumably was a significant backlog of high-yielding investment opportunities deriving from improvement in capital goods. This was reflected in a catching-up of productivity, particularly from 1948 to 1950. Opportunities deriving from previous improvement in capital goods are therefore significant in examination of the early postwar period. A one-year change in the true average age of equipment had a different effect than in normal times; and the change recorded by the available capital stock estimates probably understates the change in average age that actually took place.<sup>5</sup>

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<sup>5</sup> Note, however, that if average age is understated in the immediate postwar period, the size of the gross stock is also understated.

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### The Stability of Neutralism: A Geometrical Note

Suppose two powerful, industrialized countries, called Usonia and Russonia, compete by means of capital exports and other forms of "aid" for influence in various underdeveloped countries, typified by Thirldonia. We are interested in exploring whether anything can be said about the resulting relations between total aid, the proportions of the total supplied by each of the powers, the political position of Thirldonia, and particularly about the stability of that position. Usonia and Russonia, even though antagonistic over a wide range of issues, may come to perceive a community of interest in such stability, or at least in the avoidance of utter instability, for sudden large-scale changes in alignment on the part of Thirldonia could increase the risk of war between the two powers to a point not desired by either.

Aid from each of the two powers is likely to be influenced by the political alignment of Thirldonia as expressed, for example, by the proportion of votes it casts in the United Nations General Assembly in line with Usonia's or Russonia's wishes. Aid received from Usonia may be assumed to be the larger, the more closely aligned Thirldonia is with Usonia, with a similar aid-giving behavior holding for Russonia. For the time being, we shall suppose that complete alignment with Usonia yields the same flow of aid as does complete alignment with Russonia.

Figures 1a, 1b, and 1c portray some of the principal resulting possibilities when political alignment is shown on the horizontal axis and the amount of aid received on the vertical axis. In Figure 1a aid from Usonia declines linearly and aid from Russonia increases in the same fashion, as Thirldonia takes up political positions farther removed from Usonia and closer to Russonia; in Figure 1b aid from both decreases little as Thirldonia moves toward neutralist middle ground; while in Figure 1c aid is cut to the bone as soon as Thirldonia ceases to be a faithful camp-follower. The total aid flow available to Thirldonia results from adding up the two individual aid flows from Usonia and Russonia and is shown as the heavy line or curve in the figures: It is invariant with respect to political alignment in 1a, and exhibits either a maximum or a minimum for the neutralist position depending on whether the

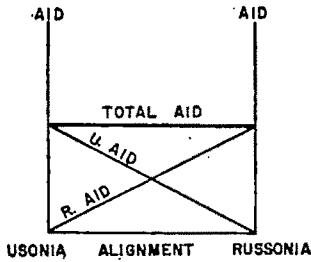


FIGURE 1a

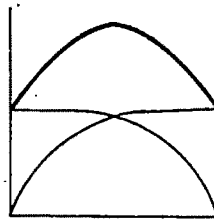


FIGURE 1b (NPN)

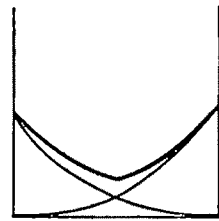


FIGURE 1c (PN)

TYPES OF SYMMETRICAL AID-GIVING BEHAVIOR BY USONIA AND RUSSONIA

aid-giving behavior of the powers corresponds to the one portrayed in 1b or in 1c.

We have just derived the transformation curve or opportunity-set available to Thirdonia if aid-from-Usonia and aid-from-Russonia are considered as two commodities which can be "produced" in differing combinations, depending on the country's position between the two powers. The three possibilities just discussed are shown along traditional lines in Figure 2 where one of the two commodities is measured along the horizontal and the other along the vertical axis. Aid-giving behavior of Figure 1b which does Not Penalize Neutralism (NPN) is now seen to result in the traditionally shaped transformation curve, while 1c-type insistence on substantial identification on the part of the aid-givers results in the opposite shape (labeled PN for Penalization of Neutralism).

Consider next the preference functions of Thirdonia for aid from Usonia and Russonia. Again we may distinguish between three principal possibilities shown in Figure 3. First, Thirdonia may be exclusively interested in maximiz-

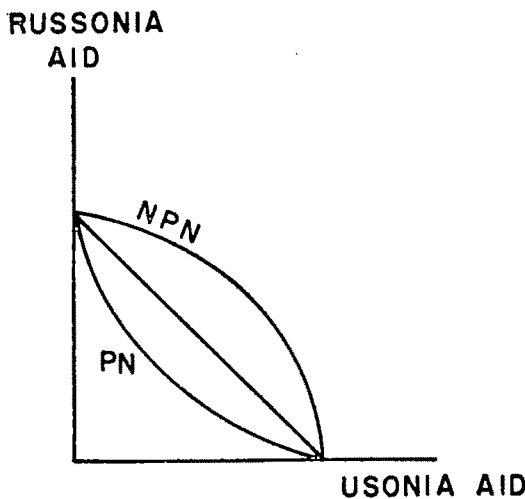


FIGURE 2. THIRDONIA'S OPPORTUNITY-SETS

ing aid: in this case its indifference map will consist of straight lines such as *MM* (for Mercenary Maximization of aid) forming a  $45^\circ$  angle with the axes along which the two aid flows are measured. The next possibility is that a country puts a high value on independence as well as on aid and feels that, for any given total, its independence is the greater, the more the ratio of Usonia aid to Russonia aid approaches unity. Such a preference would result in the traditionally shaped indifference curve, labeled *AI* (for Aid plus Independence).

A third conceivable preference pattern exists. Thirdonia could prefer complete alignment with one of the two superpowers to a middle position, either

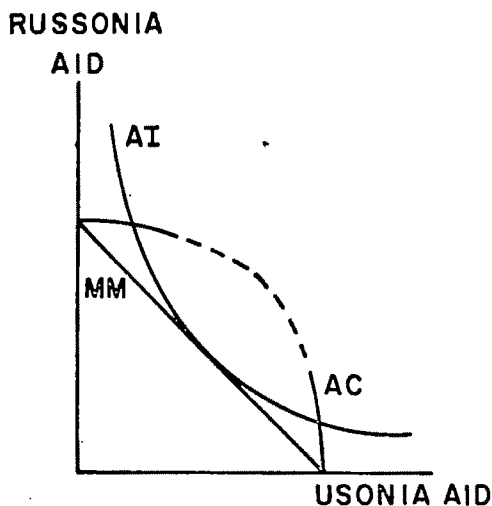


FIGURE 3. THIRDONIA'S INDIFFERENCE FUNCTIONS

because it has a liking for a strong, ideological stance or because it does not wish to be suspected of playing Usonia off against Russonia for the sake of material advantage or for some other reason. Given any fixed amount of aid, the country prefers getting it all from one source rather than through any combination of the two aid-givers. The resulting shape of the preference pattern is shown in the curve labeled *AC* (for Aid plus Commitment); the middle portion of this curve is dotted to indicate that, at any point of time, a country exhibiting this preference pattern is likely to find itself on one or the other end of the curve and is likely to visualize only immediately adjacent positions.

We can now join our "transformation" and "indifference" curves (Figures 2 and 3), and one important result follows immediately. The traditional point of stable equilibrium is achieved by joining the *NPN* curve of Figure 2 with the *AI* curve of Figure 3. In other words, if Thirdonia's preference pattern is such that it values both aid and independence, a stable point is best achieved through aid-giving behavior on the part of Usonia and Russonia that does not

penalize neutralism. Should aid be reduced strongly as soon as Thirldonia ceases to be wholly aligned with one or the other aid-giving powers (curve *PN* of Figure 2), then a potentially unstable situation results from the *AI*-type of preference. The highest *AI* curve could touch the *PN* curve at two widely distant points, between which the aid-receiving country would be indifferent. Even if there is only one point of tangency, small real or imagined shifts in the position of the two similarly shaped curves could produce a drastic shift of the optimal point, i.e., a sudden switch in Thirldonia's position in relation to the two superpowers. Hence, if an underdeveloped country is known to have the Aid-plus-Independence type of preference and if both Usonia and Russonia, in the interest of reducing the risk of war, are intent on preventing sudden shifts in the world power balance through shifts of positions on the part of less developed countries, then they ought to adopt the aid-giving behavior of Figure 1b, which does not penalize neutralism.

Looking at some of the other conceivable combinations of the transformation curves of Figure 2 and of the indifference curves of Figure 3, we find that Mercenary Maximization (*MM*) of aid is compatible with stability only if neutralism is not penalized (*NPN* curve of Figure 1). Small real or imagined shifts in the supply curve of aid of a linear or *PN* type could produce a complete flip-flop on the part of a country that is only interested in squeezing the maximum aid out of the two competing powers.

What happens if a country puts a positive value on ideological commitment to one of the aid-giving powers and hence has a preference function of the *AC* type? In this case, the more stable situation appears to result from the transformation curve following the *PN* rather than the *NPN* type. In other words, if Thirldonia likes commitment, the most stable situation is created through the aid-giving countries responding to and reinforcing this attitude by penalizing decisively any straying from the commitment path. It is possible to speak of stability in this situation only if Thirldonia, which is by definition aligned with one of the aid-giving powers at the outset, does not visualize a switch all the way to the other camp (its preference curve is defined at any one time only for one or the other end of the *AC* curve of Figure 3).

At this point in our reasoning we perceive that, in contrast to the usual assumption of economic theory, the shapes of the transformation and indifference curves in our model are *not* independent: clearly, the *NPN* and *AI* curves, on the one hand, and the *PN* and *AC* curves, on the other, "belong" together and generate each other. A Thirldonia with the *AI*-type preference will see its behavior rewarded and its correctness confirmed by finding that its opportunity-set is in fact *NPN*; and the realization on the part of Usonia and Russonia that Thirldonia is *not* "playing off one against the other," but is genuinely interested in keeping at an equal distance from both, may induce them to adopt the kind of aid-giving behavior that does not penalize neutralism. On the other hand, the *AC*-type of preference on the part of Thirldonia will generate expectations of unbending loyalty on the part of the country to which it professes to be committed; should these expectations be disappointed ever so little, aid is likely to be reduced drastically, in accordance with the *PN* pattern.

Unstable situations exist nevertheless and can now be viewed as a result of transitional departures from the two basic stable situations, *NPN-AI* and *PN-AC*. Suppose we start out with preference system *AC* and opportunity-set *PN*. An unstable situation can then arise because Thirdonia shifts its preference pattern to *AI* while Usonia and Russonia hold fast to *PN* instead of switching to *NPN*. A potentially unstable situation results, in other words, if a previously committed country shifts to a neutralist or independent position and its previous mentor cannot reconcile itself to the loss of its dominant influence and attempts to coerce the country back to the fold. The possible, and indeed likely, result is that the country will move even farther away.

Another case of instability would occur in the following situation: Suppose

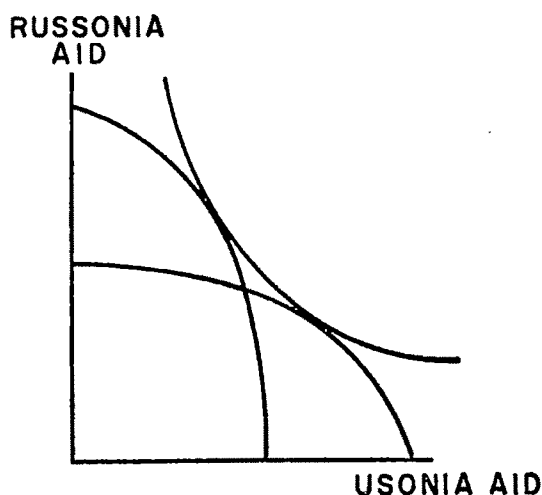


FIGURE 4. TWO EQUALLY NEUTRALIST THIRDONIAS  
WITH DIFFERENT OPPORTUNITY-SETS

Thirdonia has the neutralist *AI*-type of preference, but the aid-giving countries have adopted the *NPN* aid-giving behavior simply as a means of cajoling Thirdonia into becoming a satellite; after a while they may then tire of waiting for this hoped-for event and try a more coercive policy of the *PN* type—Thirdonia's choice would be unpredictable.

The model could no doubt be complicated considerably in various ways, for example, by introducing time more explicitly into the analysis. Suppose that the longer Thirdonia continues as Usonia's or Russonia's satellite, the more it will tend to be taken for granted with a consequent decline in aid received—such aid-giving behavior could easily be shown to give rise to instability.

Another realistic complication of the model may be briefly discussed. Suppose that the maximum aid flow that can be obtained by Thirdonia I from Usonia is larger than that which Russonia can be expected to provide, while the opposite holds for Thirdonia II. This means that the opportunity-set

facing these two countries is no longer symmetrical with respect to the coordinates. Figure 4 assumes that the relevant opportunity-sets are of the *NPN* type and that both Thirdonias have the same neutralist *AI*-type preference pattern. It is clear then that the two countries will select different foreign policy positions and would receive their aid in different proportions from the two superpowers even though they have basically the same neutralist attitude. For this reason, it is impossible to detect (to "read off") the extent or sincerity of their neutralism merely from the proportion of total aid received from one of the two contenders for influence (or from the proportion of votes cast in the United Nations General Assembly in line with the wishes of either Usonia or Russonia).

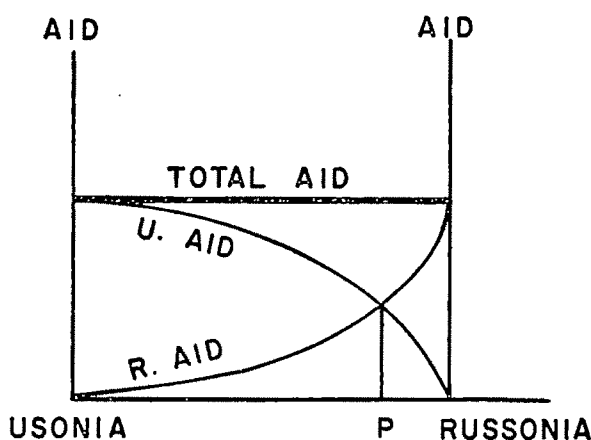


FIGURE 5. ASYMMETRICAL AID-GIVING BEHAVIOR

Thus far we have assumed (see Figures 1a, 1b, and 1c) that the aid-giving behavior of Usonia and Russonia is symmetrical. Naturally this need not be—indeed is not likely to be—the case. An interesting combination results if Usonia exhibits the *NPN* behavior of Figure 1b and Russonia the *PN* behavior of Figure 1c. The outcome could well be, as shown in Figure 5, that total aid will once again be invariant with respect to Thirdonia's political alignment. If Thirdonia is neutralist and therefore has the *AI*-type preference function, it will now select the political position that will equalize aid from both camps and will therefore tend to take up the political position *P* on Figure 5, rather close to Russonia. This situation will be exasperating to Usonia whose contribution to Thirdonia's development equals that of Russonia. As a result, Usonia will now be tempted to imitate Russonia and withdraw to the *PN*-type of aid-giving behavior. This sequence would lead to the unstable *AI-PN* combination we have already discussed. Suppose, however, that instead of Usonia adopting Russonia's behavior, Russonia for some reason goes over to Usonia's *NPN*-type behavior; we would now be back to the stable *NPN-AI* combination, and Thirdonia would assume a truly neutralist position, equidistant from both blocs. The irony would be, in this case,

that Russonia's more generous aid-giving policy would have led Thirdonia to move politically away from Russonia.

Another step toward the real world is taken if we allow for multilateral aid agencies or for aid-giving countries other than Usonia and Russonia. Taking the most worrisome unstable case where a neutralist country faces aid-giving behavior that penalizes neutralism (the *AI-PN* combination), it appears that the function of multilateral aid or of aid from countries other than the two superpowers may well be to restore stability by stepping up aid from these sources as Thirdonia moves to neutralist ground. In this way the total aid available to Thirdonia from all three sources of aid could approximate the *NPN* rather than the *PN* shape and stability would be restored. It is conceivable that Usonia and Russonia, realizing the domestic policy constraints under which they operate, find it difficult in their bilateral relations with Thirdonia to adopt anything but the *PN*-type of aid-giving behavior; it would then make good sense for them to foster deliberately multilateral aid or aid from other industrial countries in such a way as to compensate for their inability to make their own bilateral aid-giving behavior conform to the requirements of stability.

Finally, it might be tempting to analyze how the behavior of each of our three principal actors is shaped by expectations about that of the other two. But before the resulting complications are explored, it was perhaps worth while to show that a simple static model can account satisfactorily for a variety of real-life situations and can even yield some suggestions for policy.

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\*The author, professor of international economic relations at Columbia University, is indebted to Jacob Mincer for a number of helpful comments.

### Cost Functions and Progress Functions: An Integration

The relationship between unit production costs and the level of output per production period has come to be depicted in economic analysis by U-shaped or L-shaped functions, indicating that, as the level of output increases, unit costs fall over a certain range and then either rise or remain constant. In engineering studies, the relationship between unit production costs and the level of output has come to be depicted by hyperbolic "learning curves" that show unit costs declining as accumulated total output increases over time. The apparent incongruence of these two conceptions has stimulated at least one major econometric study [5] and two serious attempts at a theoretical integration [1 and 6] [4]. The purpose of this note is to present a straightforward algebraic and graphic integration of static cost functions and dynamic progress functions and an illustration of the empirical relevance and interpretative usefulness of these analytical constructions.

#### I. The Cost-Output Relation

The static cost function of economic theory does not require elaborate exposition; the standard reference is the well-known article of Viner [8], supplemented by the more recent contribution of Wolfson [9]. A few fundamental features of the static cost concept of special relevance for the present

analysis may be briefly summarized:

(1) The static cost curve is a picture of alternatives; each level of cost and output is an alternative to every other level.

(2) Static short- and long-run cost functions are not sequentially related in time, as they would be if, for example, the firm could or must move automatically from one function to another.

The progress functions<sup>1</sup> used in engineering cost studies relate the level of unit production costs to the accumulated level of output over the entire sequence of time periods during which production takes place. Typically, a hyperbolic function is assumed or estimated and graphed as a straight line on log-log paper. As generally used, progress functions take no account of the time rate of production per period; thus, it is assumed either (a) that the time rate is constant or (b) that unit cost levels are unaffected by the time rate.

Progress functions may be computed both in terms of average unit accumulated costs (total accumulated costs divided by total accumulated output) and average unit costs per production run or time period. The cost concept used makes a difference in the position of the progress function but not in its algebraic slope. If the cumulative unit cost function is written:<sup>2</sup>

$$c = aV^b,$$

the period unit cost function is

$$c_t = a(1 + b)V^b.$$

## II. A Simple Model

Our suggested integration of the static cost function and the progress function involves three variables: the level of period production costs, either total ( $C_t$ ) or average ( $c_t$ ); the level of output per production period,  $q_t$ ; and the accumulated level of total output,  $V$ .<sup>4</sup>

The general pattern of anticipated relationships between period production costs, period output, and accumulated output is illustrated in Figures 1 and 2.

<sup>1</sup>The most comprehensive general treatment of progress functions or learning curves is that of Asher [2]. The outstanding article in the engineering literature is Conway and Schultz [3]. For an evaluation of applications of the concept and a comprehensive bibliography see [7].

<sup>2</sup>For derivation see [5b, p. 137].

<sup>3</sup>Unhappily, the word "slope" has come to be used in the engineering literature to refer not to the algebraic slope of the progress function, the exponent  $b$  in the log-log transformation, but to the ratio between unit production costs at any particular level of accumulated output and unit production costs at a level of accumulated output one-half as great. For example, if a doubling of output results in a 20 per cent decline in unit production costs, the slope of the progress function is said to be 80 per cent. Slopes of 70-90 per cent are typical for the progress functions reportedly discovered and used in engineering cost studies. Evidently, for a given hyperbolic function, there is a direct correlation between the value of  $b$  and the slope in this sense—if  $b = -0.322$ , the slope is 80 per cent, etc. Hirsch has suggested the term "progress ratio" as a substitute for the present use of the term "slope" in the engineering literature, and "progress elasticity" for the value of the exponent  $b$  [5b, p. 138].

<sup>4</sup>Contrast should be noted with the Cooper-Charnes analysis [4] in which *time* is the second explanatory variable and the Alchian-Hirschleifer analysis [1, 6] which uses a capital value measure of cost.

In Figure 1 total period production costs are shown composed of fixed costs per production period, invariant with respect to both current and accumulated output, and variable costs that rise at a gradually increasing rate as the volume of output per period increases. The cost surface slopes downward as  $V$  increases, so long as the cost-reducing progress in production technique continues.

In Figure 2 the relationship is pictured in terms of unit costs. Here the familiar U-shaped curve is depicted for familiar reasons: the "spreading" of fixed costs and the gradually rising rate of variable costs. This cost surface

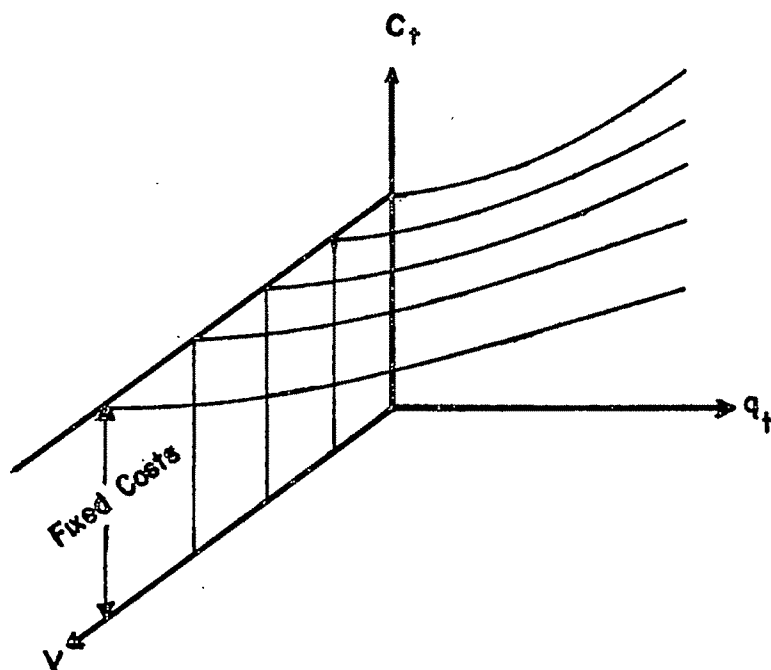


FIGURE 1

is also downward sloping with respect to  $V$ . In addition, the surface may flatten and the minimum-cost level of period output may shift as  $V$  increases. These effects would depend, of course, on the underlying technology and the particular form that the cost progress takes in individual circumstances.<sup>5</sup>

The plot of accumulated output levels and unit cost observations forming the basis for the usual "learning curve" may now be understood as a series of points on a cost surface like that in Figure 2. The same may be said of the schedules of unit costs and period outputs examined by economists in search of evidence of economies of scale. Whether the usual simplifications of either profession constitute *oversimplification* in particular circumstances is a

<sup>5</sup> The illustration reflects the accumulation of output over discrete production periods. Thus at each level of  $V$  the function is defined for all values of  $q_t$ . If we assume continuous production and instantaneous accumulation of output, the function is undefined for all values of  $q_t$  greater than  $V$ .

matter for empirical investigation; the general model of the cost-output relation should include both significant variables.

The cost-output relation depicted here is conceived in terms of a series of short-run production periods. It is explicitly *not* the traditional long-run cost curve of economic theory. The concept of the long run refers fundamentally not to the passage of calendar time but to the length of the planning horizon with respect to the variability of all factors of production. It is not necessary to proceed in time from one short-run cost curve to another in order to attain any particular position on the long-run curve. It is, however, necessary to

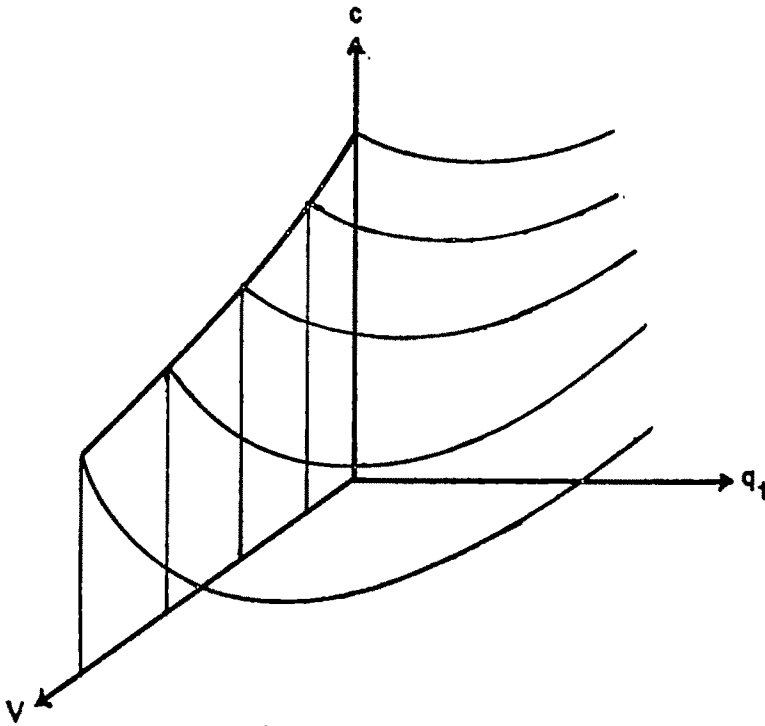


FIGURE 2

proceed in time through one short-run production period after another in order to obtain an accumulated level of output, and thus to experience the reduction in unit cost levels that results from accumulated production experience. Indeed, it may be that the cost-output surface is not fully determinate unless the particular time sequence of period outputs is specified.

### III. *An Illustration*<sup>6</sup>

The cost-output relation suggested here has been tested by application to several collections of production and cost data for manufacturing activities. Only one of these applications will be reported in detail.

<sup>6</sup>Data analyzed here were obtained by E. C. Keachle in the course of research reported in [7].

A total of 22 observations were obtained for total unit production cost, unit labor cost, and volume of output per production run for the production and assembly of five different pieces of radar equipment. The production technology was similar in all cases, and the five sets of data were reduced to a single series of cost-output observations for purposes of analysis. Output, unit cost, and unit labor cost on the initial production run for each piece of equipment were designated 100, and all subsequent cost and output values were converted to an index on this basis. From examination of the data, it was determined that the rising phase of the short-run cost curve would not be observed with sufficient frequency to permit estimation.

The variables were defined as follows:

$X_1$  = unit labor cost

$X_1'$  = unit total cost

$X_2$  = lot size (i.e., the volume of output per production period)

$X_3$  = cumulative quantity.

Because we do not know the exact shapes of the functions, for each of the cost variables three regressions were computed:

- (1) unit cost as a function of the logarithms of lot size and cumulative output;
- (2) unit cost as a function of the inverse of lot size and cumulative output;
- (3) the logarithms of unit cost as a function of the logarithms of lot size and cumulative quantity.

Five significant regression equations were obtained. In one regression (equation 1a) the lot size variable proved to be insignificant.

The five equations, with standard errors and levels of significance of the regression coefficients and coefficients of multiple determination, are shown in Table 1. All of the regression coefficients are significant at the 5 per cent level or better, and all of the correlations obtained are high. As may be observed from a direct comparison of the regression coefficients (because the variables are measured in the same units) and from a comparison of the Beta coefficients shown in Table 2, cumulative output is in all instances a more powerful explanatory variable than lot size. The increase in the Beta coefficients for lot size in regressions 2b and 3b over the corresponding coefficients in regressions 2a and 3a indicates that the relative importance of lot size as an explanatory variable is greater for unit total cost than for unit labor cost; the relative importance of cumulative quantity is correspondingly less. This result indicates that the decline in unit costs attending the accumulation of output over time is well described as a "learning" phenomenon; the accumulation of output experience is a more important explanation of changes in *labor* costs than of changes in total costs.

These results both confirm and extend those obtained by Hirsch. His analysis was cast entirely in terms of unit labor cost, and he found neither lot size nor size of plant to be a significant variable [5a, p. 145]. Similar results have been obtained by us from the analysis of other data and in one of the tests computed here. However, as our results show, there are instances in which the volume of output per production period exerts a significant and in-

TABLE 1—RESULTS OF REGRESSION ANALYSIS\*

(1b)	$X_1' = 207.61 - 9.43 \log_e X_2 - 14.79 \log_e X_3$ (4.07) (3.01) (.05) (.001)	$R^2 = .88$
(2a)	$X_1 = 27.23 - 1489.47X_2^{-1} - 5922.05X_3^{-1}$ (527.06) (694.15) (.02) (.001)	$R^2 = .90$
(2b)	$X_1' = 41.99 - 1491.57X_2^{-1} - 4420.79X_3^{-1}$ (479.53) (631.55) (.01) (.001)	$R^2 = .88$
(3a)	$\log_e X_1 = 7.01350 - .181532 \log_e X_2 - .344730 \log_e X_3$ (.08184) (.06050) (.05) (.001)	$R^2 = .90$
(3b)	$\log_e X_1' = 6.29088 - .169311 \log_e X_2 - .202341 \log_e X_3$ (.05682) (.04200) (.01) (.001)	$R^2 = .90$

\* Both variables significant at the 5 per cent level or better in all equations. Results for regression (1a) excluded because variable  $X_3$  failed to meet required significance levels.

dependent effect upon unit costs beyond the effects due to increasing cumulative output.

#### IV. Implications

The inclusion of both the cumulative level of output and the level of output per production period as significant variables in the explanation of the level of unit production costs has important implications for both teaching and research. Judging from current textbooks and casual observation, the "learning" phenomenon receives almost no mention in the standard treatments of production costs in economics or industrial engineering courses. This neglect of the principle of "progress in technique" in the classroom is reflected in the neglect of the differing ages and production histories, as well as sizes, of plants and firms in empirical studies of scale economies. Although accumulated production know-how has long been accepted as an advantage of established firms over new entrants, the importance of production history, in addition to or instead of capacity or output level, as an explanation for cost

TABLE 2—BETA COEFFICIENTS

Equation Number and Dependent Variable	Lot Size	Cumulative Quantity
1b—Unit total cost	-.31	-.67
2a—Unit labor cost	.26	.77
2b—Unit total cost	.32	.71
3a—Unit labor cost	-.28	-.71
3b—Unit total cost	-.38	-.61

differences has rarely been specifically investigated. As a result, some of the evidence that has been taken to reflect economies of scale may in fact reflect the accumulation of experience by long-established large firms and plants. If so, then available estimates of minimum optimal scale of production may be *high*; long-established, large firms and plants may have low unit costs because of their past output experience, not because of their present size.

The importance of "progress functions" as described here for the explanation of cost behavior within the plant and firm also carries implications for macroeconomic analysis. Hirsch [5b, p. 155] has pointed out the relevance of "learning" considerations in the forecasting of cost and labor requirements on industry-wide and economy-wide bases. In addition, progress in technique due to "learning" may need to be considered as a third variable, along with autonomous technological progress and research and development activity, in explaining changes in the aggregate production function and differences between rates of economic growth, both over time and between countries. The widely held view that the pattern of long-term economic change in the United States is more largely due to the increasing productivity of the workforce than to increasing capital per worker may be, in part, a macroeconomic reflection of the "learning" phenomenon. The existence and form of such relationships, if any, will require careful investigation. As a beginning, the inclusion of accumulated production experience along with "scale" and "production technology" as determinants of the efficiency of the fundamental production unit suggests the possibility of these macroeconomic effects and also suggests a reassuring symmetry between the analysis of production in the small and in the large.

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### Capital Gains and the "Life Cycle" Hypothesis of Saving

In the March issue of this *Review* Professors Albert Ando and Franco Modigliani presented empirical estimates of aggregate consumption functions which included net worth as an explanatory variable [1]. They used Goldsmith's estimates of national wealth [3] [4] to test some of the implications of the Modigliani and Brumberg consumption model [6] [7].

Similar empirical results were obtained (from net worth data in [4] [5]) in my study [2], which was independent of, but is parallel to, that of Ando and Modigliani. There is one basic conceptual difference, however, between the two studies, but apparently it does not lead to very different empirical results. In the Ando and Modigliani study, net worth for the period is based upon the *average* price during the year; therefore, their measure of net worth for the period *includes* the impact of net worth changes due to price level movements during the period, i.e., includes the gains for the period. My study examines net worth based upon the price at the *beginning* of the year ( $A_{t-1}$ ) and includes capital gains on this net worth ( $G_t$ ) as an explicit exogenous variable in the consumption function. There are other minor differences in definitions and in the time covered by the data, but the basic difference lies in the inclusion in the consumption function of the capital gains variable and the estimation of its impact upon consumption. (It should be noted that in the Ando and Modigliani model it is not necessary to make explicit reference to these capital gains since their model implies that such gains will affect present and future consumption insofar as they change the value of net worth.)<sup>1</sup>

Given this conceptual difference, I fitted a consumption function of the following form:

$$C_t = B_1 + B_2 Y_t + B_3 A_{t-1} + B_4 G_t.$$

This function was fitted both in money and real values; Tables 1 and 2

<sup>1</sup>At one point there is some confusion when they seem to imply that changes in net worth result only from the current period's saving; they state that  $A_t = A_{t-1} + S_t$  (where  $A_{t-1}$  is the initial net worth of the period;  $A_t$  is the ending net worth of the period; and  $S_t$  is saving during the period; cf. [1, p. 69, fn. 26]); however, either their definition of saving includes changes in the value of net worth due to price level movements or else they may be assuming that real changes in the purchasing power of net worth do not exist (an assumption of homogeneous price movements). This point was not clear. In my model, however, this difficulty is avoided by defining the period's saving in more conventional terms as the difference between disposable income and consumption; therefore, the change in net worth from the beginning of the period to the end would be the period's saving *plus* the period's capital gains or losses ( $G_t$ ); i.e.,  $A_t = A_{t-1} + S_t + G_t$ .

TABLE 1—REGRESSION RESULTS: MONEY VALUES\*

13 Observations

$$C_t = B_1 + B_2 Y_t + B_3 A_{t-1} + B_4 G_t + B_5 T_t$$

Regress Format	Coef.  $B_1$ Constant	$B_2$ Income	$B_3$ Initial Net Worth	$B_4$ Capital Gains	$B_5$ Time	$R^2$
A	60.39 (37.6) 1.61	.4835 (.1449) 3.337	.064 (.023) 2.844			.994
B	71.97 (34.8) 2.07	.3816 (.1437) 2.656	.0796 (.0223) 3.574	.0308 (.0172) 1.786		.995
C	88.44 (360.69) .245	.3845 (.1662) 2.314	.0810 (.0385) 2.100	.0314 (.0232) 1.353	.5849 (12.75) .0459	.995
D		.6639 (.0976) 6.802	.0396 (.0171) 2.236			.992
E		.6112 (.1045) 5.848	.0481 (.0186) 2.583	.0241 (.0194) 1.243		.993
F		.3720 (.1482) 2.510	.0732 (.0203) 3.597	.0279 (.0170) 1.636	2.525 (1.235) 2.047	.995

Note: Upper figure is estimated coefficient. Middle figure is standard deviation. Lower figure is  $t$  statistic.

\* The basic regressions were also computed with data weighted (divided) by initial net worth; however, the empirical results were about the same as those presented in this table and are consequently not reproduced here.

present the statistical results.<sup>2</sup> The data are shown in Table 3; they are approximately the same as those of Ando and Modigliani with some definitional exceptions. All observations are on a per capita basis (i.e., all data are deflated by population). The variable time ( $T_t$ ) is included in the regressions to test whether initial net worth is significant only as a proxy for secular growth. Ordinary least-squares estimation was used; some regressions omit the constant term ( $B_1$ ) in order to have the same form as Ando and Modigliani's consumption function.

The estimated coefficients of income and initial net worth are different from zero as in the Ando and Modigliani study. My estimates differ slightly from theirs, probably because of the different definitions of variables<sup>3</sup> and because of the different time periods used.

<sup>2</sup> In the original study, saving was used as the dependent variable. Tables 1 and 2 are based upon these results but with consumption as the dependent variable.

<sup>3</sup> For a complete discussion of the definitions of the variables used in this study see

TABLE 2—REGRESSION RESULTS: REAL VALUES<sup>a</sup>

13 Observations

$$C_t = B_1 + B_2 Y_t + B_3 A_{t-1} + B_4 G_t + B_5 T_t$$

Regress Format \ Coef.	$B_1$ Constant	$B_2$ Income	$B_3$ Initial Net Worth	$B_4$ Capital Gains	$B_5$ Time	$R^2$
A	111.52 (76.34) 1.461	.5307 (.1346) 3.943	.0500 (.0184) 2.722			.993
B	113.78 (76.20) 1.493	.5187 (.1347) 3.851	.0516 (.0184) 2.804	.0154 (.0150) 1.025		.994
C	210.93 (105.53) 1.999	.5350 (.1311) 4.081	.0925 (.0364) 2.538	.0358 (.0215) 1.664	-8.643 (6.713) -1.288	.995
D		.6794 (.0925) 7.345	.0368 (.0168) 2.189			.992
E		.6708 (.0935) 7.174	.0381 (.0169) 2.243	.0147 (.0159) .927		.992
F		.6548 (.1350) 4.850	.0349 (.0258) 1.351	.0125 (.0209) .598	.950 (5.435) .1749	.992

Note: Upper figure is estimated coefficient. Middle figure is standard deviation. Lower figure is  $t$  statistic.

<sup>a</sup> The basic regressions were also computed with data weighted (divided) by initial net worth; however, the empirical results were about the same as those presented in this table and are consequently not reproduced here.

The estimated coefficient of the capital gain variable is positive, but the hypothesis that such gains have no effect upon consumption ( $B_4 = 0$ ) cannot be rejected. However, one implication of the theoretical model is that unanticipated gains have a "one-shot" effect to be spread over the remaining life of the individual. They become part of net worth in future periods and therefore affect consumption in the same way as initial net worth, as Modigliani and Ando imply. This would mean that the coefficient  $B_4$  should equal  $B_3$  (unless there is a period of adjustment; this possibility is discussed below). One can test the hypothesis that the two coefficients are equal by rewriting the consumption function as  $C_t = B_1 + B_2 Y_t + B_6(A_{t-1} + G_t) + B_7 G_t$  and

[2, Part B of the Appendix]. Apparently there are differences between the definitions used here and in the Ando and Modigliani study [1]. For example, their definition of income is labor income only, while mine is the more conventional disposable income. The complete sources and definitions of their data are also to be published shortly—see the references given in [1].

testing the hypothesis that  $B_7$  equals zero. This equation was estimated in the original study [2]. The coefficient  $B_7$  is slightly negative but not significantly different from zero.<sup>4</sup> Regression  $B$  implies that the coefficient of capital gains is not significantly different from zero; the test of  $B_7$  implies that the coefficient of capital gains does not differ significantly from that of initial net worth. The test is not of sufficient power to make the distinction between the two hypotheses.<sup>5</sup>

TABLE 3—BASIC DATA—CURRENT VALUES\*

	Saving <sup>b</sup>	Consumption	Disposable Income	Initial Net Worth	Capital <sup>c</sup> Gains	Real Capital Gains
1946	\$19854	\$127235	\$147089	\$ 728310	\$ 54296	\$-23062
1947	13311	135695	149006	802460	74189	70059
1948	19504	145757	165261	889960	31426	60257
1949	17187	147297	164484	940990	-5087	-82603
1950	25442	154992	180434	953090	85028	40555
1951	25601	170801	196402	1063560	62079	57137
1952	24771	181179	205950	1151240	20929	11255
1953	27547	191393	218940	1196940	723	9209
1954	25350	197958	223308	1225210	67730	69676
1955	29411	209268	238679	1318290	79329	39808
1956	31049	222854	253903	1427030	56381	8505
1957	30330	236464	266794	1514460	10910	-12296
1958	27269	246247	273516	1555700	126324	105415

Note: Figures are in millions.

\* For a detailed source of data, see [2, Part B of the Appendix].

<sup>b</sup> Includes consumer durables net of replacement cost depreciation.

<sup>c</sup> These gains were computed by taking the change in initial net worth for the year and subtracting saving for the year from this difference.

The results in real terms (Table 2—deflation was by the consumer price index of 1958 base) are about the same as those of Table 1. The only basic difference lies in the definition of real capital gains which are not simply money gains deflated by a price index.<sup>6</sup> (See Table 3 for the values of the

<sup>4</sup> If one assumes that the capital gains are received evenly throughout the period rather than entirely at the beginning of the period, one could plausibly assume that the coefficient on capital gains should be one-half that on wealth to reflect the fact that the household adjusts to the average level of net worth, i.e.,  $B_4 = \frac{1}{2}B_5$ . This possibility was also considered and the hypothesis that  $B_5 - 2B_4 = 0$  could not be rejected.

<sup>5</sup> Also, there are very few degrees of freedom in these tests (although more observations were used in the original tests [2] with essentially the same results), and such estimating techniques do present statistical difficulties as were pointed out in the Ando and Modigliani study.

<sup>6</sup> Money capital gains were computed by subtracting money saving of the period from the change in the money values of initial and ending net worth.

$$G_t = A_t - A_{t-1} - S_t.$$

Real capital gains were computed by subtracting real saving of the period from the change in real purchasing power of ending net worth and of initial net worth. The consumer price index was the deflator.

$$G_t = \frac{A_t}{P_{t+1}} - \frac{A_{t-1}}{P_t} - \frac{S_t}{P_t}.$$

For a detailed discussion of this variable, see [2, Part B of the Appendix].

real capital gains.) Again the capital-gain coefficient cannot be distinguished from that of net worth or from zero. When the variable "time" is included in the function, initial net worth is still a significant variable, implying that net worth is not merely a proxy for some growth item.

I would, therefore, conclude that the omission of capital gains as an explicit variable in the Ando-Modigliani study does not greatly affect the empirical results; in fact the results would tend to justify their treatment of such gains as a part of wealth. Capital gains appear to encourage more consumption in the current period. They will certainly do so in future periods when such gains are part of future net worth, which has a significant effect upon consumption. However, even with the inclusion of this variable, the statistical results of both studies are about the same for the variables of income and initial net worth.

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### The "Life Cycle" Hypothesis of Saving: A Correction

In the March, 1963 issue of this *Review*, we presented an aggregate theory of consumer behavior based on considerations of households' life cycle of income and of consumption needs, as well as some empirical tests.<sup>1</sup> At that time, we were unaware of the existence of revised data on net worth of consumers prepared by Goldsmith and Lipsey.<sup>2</sup> We also had some numerical errors in adjusting the old, now obsolete, data by Goldsmith to fit our framework. We have prepared new estimates of the basic parameters, using the revised data by Goldsmith and Lipsey, and they are reported in Table A.

<sup>1</sup> Albert Ando and Franco Modigliani, "The 'Life Cycle' Hypothesis of Saving: Aggregate Implications and Tests," *Am. Econ. Rev.*, March 1963, 53, 55-84.

<sup>2</sup> The new data were kindly provided for us by Dr. Lipsey of the National Bureau of Economic Research. They have since been published in Raymond W. Goldsmith, Robert T. Lipsey, and Morris Mandelson, *Studies in the National Balance Sheet of the United States*, Princeton, 1963.

TABLE A\*  
ESTIMATES OF THE COEFFICIENTS OF THE CONSUMPTION FUNCTION

Rows	Hypothesis Tested	Mode of Regression <sup>a</sup>	Coefficients and Their Standard Errors of Estimates <sup>b</sup>						$\alpha_1 + \alpha_3$	Standard Deviation of Dependent Variable	Standard Error of Estimate	R <sup>2</sup>	Durbin-Watson Statistic
			Constant	Y ( $\alpha_1$ )	XY ( $\alpha_{12}$ )	Y(L/E) ( $\alpha_2$ )	A ( $\alpha_3$ )	XA ( $\alpha_{32}$ )					
(1)	I	A	5.33 (1.46)	.767 (.047)	—	—	.047 (.010)	—	.767	88.289	2.352	.999	1.29
(2)	II	A	4.69 (1.51)	.633 (.112)	—	.163 (.124)	.040 (.012)	—	.796	88.289	2.314	.999	1.17
(3)	I	A	—	.640 (.039)	—	—	.077 (.008)	—	.640	88.289	2.860	.999	.89
(4)	I	A	—	.787 (.086)	—	—	.058 (.013)	-.010 (.005)	.787	88.289	2.756	.999	1.17
(5)	II	A	—	.430 (.108)	—	.287 (.139)	.058 (.012)	—	.717	88.289	2.690	.999	.85
(6)	I	B	—	.550 (.116)	—	—	.079 (.021)	—	.550	8.292	2.335	.921	2.00
(7)	I	B	—	.577 (.170)	-.030 (.138)	—	.079 (.022)	—	.577 <sup>d</sup>	8.292	2.385	.918	2.03
(8)	I	B	—	.550 (.119)	—	—	.082 (.031)	-.004 (.025)	.550	8.292	2.385	.918	2.02
(9)	II	B	—	.444 (.124)	—	.274 (.147)	.051 (.025)	—	.718	8.292	2.215	.929	1.82
(10)	II	B	—	.411 (.127)	—	.353 (.163)	.068 (.029)	-.028 (.026)	.764	8.292	2.211	.929	1.96
(11)	I	C	—	.634 (.020)	—	—	.080 (.003)	—	.634	.092	.018	.962	.99
(12)	I	C	—	.644 (.056)	-.004 (.018)	—	.078 (.008)	—	.644 <sup>d</sup>	.092	.019	.958	1.02
(13)	I	C	—	.654 (.054)	—	—	.077 (.008)	-.001 (.004)	.654	.092	.019	.958	1.04
(14)	II	C	—	.639 (.081)	-.003 (.019)	.007 (.070)	.077 (.009)	—	.646 <sup>d</sup>	.092	.019	.958	1.02
(15)	II	C	—	.649 (.079)	—	.006 (.070)	.076 (.009)	-.001 (.004)	.655	.092	.019	.958	1.04

Table A is identical in construction to our original Table 2. Comparison of the two tables indicates that our revised results are not appreciably different from the original ones, though they appear to be somewhat more satisfactory in a number of respects as noted below:

1. There is no longer any evidence of a significant downward shift in the coefficient of labor income from the prewar to the postwar period. In every instance the estimated shift is of negligible magnitude and is but a small fraction of its standard error. In the case of the coefficient of net worth, where a shift could be due to differences in the method of estimating net worth, the hypothesis of a downward movement cannot be rejected outright but the evidence is much less convincing than before.

2. In the case of hypothesis I, the various methods of estimation yield values of the parameters which are both appreciably closer to each other and more in line with a priori expectations. In particular, the constrained regression (3), the first difference method (6), and the ratio estimates (11), which are supposed to bracket the true value, yield an estimate of  $\alpha_1$  in the immediate neighborhood of .6 with a range of .09 (instead of around .5 with a range of .12), and an estimate of  $\alpha_3$  ranging between .072 and .08 (instead of between .072 and .105).

3. The revised data provide somewhat stronger support for hypothesis II than did the original ones, as the coefficient of  $Y(L/E)$  is in every instance both numerically larger and statistically more significant (though the outcome is reversed for the coefficient of  $L/E$  in the ratio estimates). We also note that the estimate of  $\alpha_1 + \alpha_2$  remains in the neighborhood of .7 but the range of the estimates from the above-mentioned three methods is reduced from .10 to .07. Finally the coefficient of net worth moves closer to the estimates obtained for hypothesis I and to a priori expectations, averaging now just over .06 with a range of .016, instead of .05 with a range of .03. Thus the new estimates of the parameters for both hypotheses I and II seem to be considerably more reliable, a conclusion reinforced by the substantial improvement in the Watson Durbin Statistic.

On the whole, in the light of these results, we see no reason to change any of the conclusions we stated in our original paper.<sup>3</sup>

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<sup>3</sup> We should like however to take this opportunity to clarify one passage in our paper which might give rise to misunderstandings. In footnote 30, for the purpose of examining certain long-run implications of the life cycle hypothesis, we made use of the equation:

$$S_t = A_t - A_{t-1}.$$

In general, of course, saving,  $S$ , which we define as current income minus current consumption, need not coincide with the change in the value of assets, on account of capital gains (or losses). In writing the above equation we abstracted from capital gains, which we feel is justified for purposes of long-run analysis, although we should have made this assumption explicit.

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### Public Policy Problems of the Domestic Crude Oil Industry: Comment

The recent article by Paul Davidson in this *Review* [4], while illuminating certain aspects of crude oil policy, is seriously defective in one vital area. A failure to perceive clearly the relationship between ultimately fixed supplies and Ricardian rent payments completely vitiates his rather striking conclusion concerning the effect of depletion allowances on the allocation of resources to exploration.

He points out that the supply of oil-bearing land in the United States is fixed and that nothing man can do will change it. This simple observation leads him to the conclusion that, given perfect competition, certainty, etc., the major (or total) effect of depletion allowances is to increase rents accruing to owners of subsurface mineral rights. Depletion allowances lead to either no or very little more exploration (and development?) than would otherwise be carried on.<sup>1</sup>

The conclusion is strikingly at variance with that reached by the most authoritative academic students of the industry, DeChazeau and Kahn, who feel that depletion allowances are part of a "powerful inducement to look for oil and gas" and that they "undoubtedly have the effect of enhancing the flow of capital into crude oil exploration and production and hence of holding the price of oil lower than it otherwise might be" [5, p. 221]. Harberger, who is cited as partial authority for their statement, feels that depletion allowances probably lead to too much exploration and development [6, p. 447]. A long list of what Davidson calls "apologetic 'scholarly' support" could be cited in favor of the conclusion.

Numbers, however, do not make a case and it is necessary to examine carefully the arguments advanced. The critical assumption underlying Davidson's conclusion is that the supply of new oil land is perfectly (or nearly perfectly) inelastic. On what does he base this assumption? First, his "own acquaintance with the crude oil industry" leads him to disagree with Harberger's conclusion that there is a substantial degree of elasticity in its supply [4, pp. 102-3, n. 20]. Secondly Harberger's statement that the amount of oil underground is a fixed physical quantity apparently adds to his conviction. Thirdly, his view is apparently confirmed by the feeling that:

The landowner of any undeveloped oil property [Davidson really means the owner of the subsurface mineral rights] exhibits a perfectly inelastic supply function for his mineral rights—there is no alternative use. Thus, the lease bonus and royalty payments are, as McKie and McDonald intimate, Ricardian rent payments [4, p. 104].<sup>2</sup>

<sup>1</sup> Compare these statements: "... our analysis has indicated that, in the long run, percentage depletion *does not significantly encourage* the allocation of additional real resources to exploring presently nonproductive oil lands" and "A reduction in the depletion allowance *will not reduce* the incentives to explore undeveloped oil lands" [4, p. 107, italics added].

<sup>2</sup> He seems also to have been misled by the purely coincidental fact that the "value" of the depletion allowance is roughly equal to the typical producing royalty of  $\frac{1}{8}$ . See [4, p. 104, n. 21 and p. 106, n. 23]. While it is not true that the owner of mineral rights can do nothing but lease them (he can drill them himself, or keep them in the hope that their

Assuming homogeneity, Davidson's argument then follows nicely. If all land is homogeneous, with only one use, it is a free good up to the point where it is all being used, and thereafter rent payments accrue to holders of claims to land. Below the fixed limit a subsidy to the purely competitive land-using industry will increase land use; beyond the limit it will simply increase rents accruing to claim holders. If any rent is currently being paid, the land must be currently in perfectly inelastic supply.

The difficulty lies in the fact that "oil land" (by which Davidson seems to mean all land which either does or may overlie crude oil pools [4, p. 102]) is in no sense homogeneous. Some 80 per cent of the area of the United States and continental shelf is potentially oil bearing, but no two acres have exactly the same potential. Given the level of geological knowledge it is possible to rank very crudely and tentatively the potentially oil-bearing acres into a potential profitability continuum. The attractiveness of an area will depend, *inter alia*, on the expectation of finding oil there, the likely depth of the oil, if found, and the anticipated size of fields. Given the price of oil, areas can be roughly ranked according to the expected profitability of surveying and drilling in them. The ranking may be turned upside down by putting the question in the form "What is the expected cost of finding an extra barrel of reserves in each of the different areas?" All the potential oil areas can thus be ranked in a continuum according to, *inter alia*, the number of feet that will probably have to be drilled in order to find one extra barrel of oil.<sup>3</sup>

While the above is greatly simplified, it nevertheless constitutes a reasonable approximation of the situation. Great uncertainty prevails and "rankings" are subject to considerable change as surveying and nearby drilling make more knowledge available. Absolute certainty cannot be achieved except by drilling on the spot.

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value will rise), this error does not substantially affect the argument. If rights owners had no alternative but to lease their rights, the rent would necessarily equal the depletion allowance only in the case where it would otherwise be zero. The prospective  $\frac{1}{8}$  is, as Davidson points out, not the only incentive a rights owner has for leasing—bonuses and per acre rentals are usual and sometimes substantial in amounts. Claim owners are also permitted to apply percentage depletion to their royalty payments.

The coincidental nature of the current equivalence of the two figures is made even more obvious by the fact that the "value" of the depletion allowance is much higher now than it was in 1939, but the basic 12.5 per cent producing royalty has remained unchanged. With the tax rate on corporate incomes above \$25,000 set at 19 per cent in 1939, the "value" of the depletion allowance was then a little over 5 per cent.

<sup>3</sup> This is not to imply that the number of feet drilled per barrel found is the only relevant cost variable. It is the largest single item and accounts for about 70 per cent of the nonlease cost of finding new fields [7, p. 551]. The existence of other cost items that do not vary in the same manner from area to area may change the relative area rankings, not the principle. It should be noted that there are three phases involved in the "production" of new crude oil reserves: geological and geophysical activity, exploratory drilling, and development drilling. For simplicity, we might consider a new field to be "found" when a successful oil well first comes into being. The evaluative ranking procedure outlined above is relevant to all three phases, although only very rarely will a new field not appear to be worth development or at least further exploratory drilling. Leases are generally obtained after some geological and perhaps geophysical evaluation of the prospects, and always before exploratory drilling takes place.

It is clear that the appropriate model is that of the rent of nonhomogeneous land, provided that the land-using industry can be categorized fairly as purely competitive. While oil production activities are generally cartelized by state prorationing and protected by federal import quotas, the industry does appear to be substantially competitive in its exploration and leasing activities,<sup>4</sup> and the model therefore applies. Given the level of knowledge and the price of oil, the industry will rent and explore those acres that are within the extensive margin of exploration.<sup>5</sup> The purely competitive phase of the industry will earn only a normal return, plus a risk premium. All possessors of mineral rights within the extensive margin (largely the same companies in their role as oil producers) will receive rent. Those on and beyond the margin will receive none. If the expected return rises or a depletion allowance is granted, the extensive margin will move outward, more exploratory work will be done, and additional proven reserves will in all probability be added to the nation's stock. Rents on the intramarginal land will rise, and those who can appropriate them will receive them. The relative increases in total rents and in new reserves discovered will depend on the elasticity of supply of new reserves; this elasticity of supply will be zero only when every drop of oil which can be found has been found.

The strict version of Davidson's conclusion—that a subsidy will not increase exploration at all—is demonstrably erroneous. But the box remains empty; if the supply curve of new reserves is virtually vertical in the current range, then his policy conclusion remains substantially undamaged. The available evidence does not permit derivation of a full-scale statistical cost curve of a convincing nature, but certain data are suggestive.

1. *Undiscovered Reserves and the Supply Curve.* Netschert [9] presents the results of 10 estimates of the total "ultimate" crude oil reserves of the United States, all made between 1948 and 1960.<sup>6</sup> These estimates range from 140 billion to 2,000 billion barrels; the median estimate is 200 billion. Since past production plus known reserves as of December 31, 1960 amounted to approximately 96 billion barrels, it would appear that about half of the crude oil which could be produced with current technology and costs remains to be found [3, p. 47].<sup>7</sup>

<sup>4</sup> See [7, p. 571]. These government activities affect the value of newly discovered reserves, and therefore the amount of exploration, but do not alter the competitiveness of the activities involved in finding the new reserves.

<sup>5</sup> In 1960 about 19 per cent of the area of the United States was under lease [3, p. 59]. It should be emphasized that the acquisition of more information or technological change can shift areas from one side of the margin to the other—and even back again.

<sup>6</sup> Netschert distinguishes between the "resource base" and "ultimate reserves." The "resource base" includes every drop of oil that existed when Drake sank the first well in 1858. "Ultimate reserves," in this context, include only the portion of the resource base that was or is recoverable with current technology and costs. "Ultimate reserves," then, are the sum of all oil produced to date, the American Petroleum Institute estimate of "proven reserves," and the amount of undiscovered oil that could be made available with current costs and technology. It does not include shale oil, which is apparently currently close to the margin [9, pp. 295-300, 397].

<sup>7</sup> It should be noted that production plus proven reserves have already exceeded some previous estimates of ultimate reserves. It is entirely possible that the 200 billion figure will also prove to be too low.

It is impossible to specify properly the percentage of the potentially oil-bearing land which has been explored, for the term is relative not absolute. An industry committee report, however, estimated in 1952 that production was current on only about one per cent of the 80 per cent of all U.S. territory that is potentially oil and gas bearing [8, pp. 45-46]. It is further estimated that, at the then current rate of exploratory<sup>8</sup> drilling, it would take 50 years to drill exploratory wells to an average density of one per three square miles [8, p. 56]. Since it has been not infrequently necessary to drill to greater density in order to find the oil pools which existed, even that figure would in no sense represent full exploration. That the supply curve of new reserves is not vertical is hardly open to question; new reserves are being added each year.

These reserves, when found, could presumably be extracted at roughly the current cost level [9, p. 348], but this does not imply that they can be found at the current finding cost level. There is some historical evidence to indicate that the static supply curve of new reserves may have a substantial upward slope. Data for the period 1944-60 show virtually no change in the percentage of new-field wildcats that find oil or gas [1, p. 714].<sup>9</sup> New field wildcats are, however, gradually increasing in depth [1, p. 709],<sup>10</sup> and there appears to have been some tendency for the average size of fields found to diminish from 1946 to 1954.<sup>11</sup> Even without taking into account the increased volume of geological and geophysical work, which is largely responsible for the stable percentage of wildcats that find oil or gas, it would appear that it has been getting harder, in real terms, to find an extra barrel of reserves. The static real supply curve probably has had a substantial upward slope, but its elasticity was probably considerably greater than zero, for cumulative discovered reserves nearly doubled in the period 1944-60.<sup>12</sup>

2. *Statics, Dynamics, and Policy.* In the absence of a vertical (or virtually vertical) static supply curve, Davidson's argument fails to apply. Increasing

<sup>8</sup> "Exploratory" wells are customarily divided into two subclasses, those on structures which are not yet producing, and those on already producing structures. The former are referred to as "new field wildcat" exploratory wells [1, p. 703]. The statement above is apparently based on the broader concept. Elsewhere, I have used "exploratory" to mean "new field wildcat," and "exploration" to refer to the search for new fields. Fields are made up of one or more pools.

<sup>9</sup> The average for the period is 11.08 per cent.

<sup>10</sup> The increase is fairly regular from an average of 4,203 feet in 1944-46 to 4,956 feet in 1958-60. Cost per foot increases with depth, so constant dollar costs rose more rapidly than the footage figure alone would indicate [3, p. 21].

<sup>11</sup> The annual review of exploratory drilling in the *Bulletin* of the American Association of Petroleum Geologists gives estimates of the number of fields found in each of several size classes on an annual basis. Evaluation of the size of fields is exceptionally difficult until a few years of development drilling have provided fuller data on their size and contents. The statement above is based on evaluation of the number of fields in each of the size classes six years after the discovery year. The percentage of new oil fields which after six years are either abandoned or estimated to contain less than 1 million barrels of crude oil has risen from 73.3 per cent in 1946-48 to 79.6 per cent in 1952-54. Calculated from [1, 1952-60, June issues].

<sup>12</sup> Cumulative discovered reserves rose from 50 billion barrels in 1944 to 79 billion in 1954 and reached 96 billion in 1960 [2, p. 62] [3, p. 46].

the subsidy will induce further exploration, and more rapid additions to proven reserves. The extra amount of exploration and reserves would appear to depend on the actual slope of the cost curve discussed above, but this is not so. The search for oil has witnessed a substantial amount of technological change, and is likely to witness more. Since the impact of this technological change has been to push the supply curve to the right, the fact is that, even if the static curve were to be fully delineated, it would not provide a sound basis for policy formation. If a change in the subsidy rate affects the flow of innovations in the industry, as would seem likely on a priori grounds, then the static curve loses almost all its relevance for policy formation—any subsidy policy will change the static curve on which the policy is based. If changes in the subsidy rate had no effect on technological change in the industry, the static curve would shift in response to exogenously determined technological changes. In this case the ratio of the increment in total subsidy payments to the increment in reserves caused thereby could not be determined by simply examining the static supply curve, and the social cost of the policy could not be determined. In both cases the moral is clear: policy cannot properly be formulated in the absence of a knowledge of dynamic as well as static elements.<sup>13</sup>

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<sup>13</sup> In one of Davidson's cases (absolutely fixed supply) the stricture does not apply. Here the impact of the subsidy on exploration is always zero, unless it affects the flow of innovations and somehow moves the vertical curve to the right.

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## Public Policy Problems of the Domestic Crude Oil Industry: Comment

In the March, 1963 issue of this *Review*, Paul Davidson developed a model of the domestic crude oil industry and attempted to evaluate certain policy matters in the light of his model [2]. Three of his more important conclusions relate to conservation, the price of crude oil, and the relationship between percentage depletion and exploration. The paper as a whole is an interesting and worth-while endeavor to apply economic analysis to such policy problems. Unfortunately, these issues are extremely complex, and in my view there are a number of points at which the analysis suffers from the failure to consider certain relevant facts and from imperfections in the mode of analysis. This comment proposes to take issue with Davidson's major policy conclusions<sup>1</sup> and to suggest that a closer look at some of the facts may raise questions about the validity of his conclusions.

### I. *Conservation*

Davidson argues that compulsory field unitization would eliminate the undesirable effects of the rule of capture and would insure that lower-cost wells were operated more intensively. I do not propose to take issue with Davidson on the purely economic desirability of unitization. Indeed, under ideal conditions (small number of parties concerned, perfect knowledge of reservoir characteristics, absence of prominent structural advantages for particular operators, absence of mistrust or obstructionism, no need for compulsory implementation by state agency) unitization would be an ideal solution. But in the institutional context of the crude oil market, unitization is probably not a desirable policy recommendation. It is not strictly pertinent to criticize a pure economic analysis of present policies merely by arguing that for political reasons it will be difficult to change them. But if an economist elects additionally to make policy recommendations ("Regulation to Assert the Public Interest"), it is appropriate to suggest that the institutional difficulties of implementing a policy that is optimal when analyzed in an institutional vacuum may make an imperfect situation still more imperfect. In this section it is argued that (1) the failure of state conservation commission regulation to achieve an optimal distribution of production among wells is due more to political than to economic circumstances; (2) greater political difficulties are inherent in the implementation of unitization than in existing conservation regulations; (3) if instituted by state authorities, unitization would very likely be accompanied by prorationing to protect marginal wells and fields; (4) even with unitization, the spasmodic nature of discoveries and long-run increasing costs may still argue for the preservation of some marginal wells under certain circumstances; (5) Davidson tends to exaggerate the degree of kinship of the old rule of capture to current state conservation regulation and to overlook

<sup>1</sup> In the original version of this comment, I analyzed Davidson's depletion argument along the line presented in Duncan Campbell's accompanying comment on Davidson's paper. To avoid duplication, I need merely indicate my complete agreement with Campbell's analysis of this point.

the evolutionary tendency of such regulation toward greater economic efficiency and a closer approach to the standard of performance theoretically possible under enlightened unitization.

In the institutional context of the crude oil market, state commissions now play (and will no doubt continue to play) an important role. Efficient low-cost wells are now producing at relatively low levels precisely because a judgment has been reached by public officials that it is in the public interest to prolong the life of stripper wells. It is this judgment that must be reversed, not merely the method of regulation, if low-cost wells are to be granted a larger share of the nation's production. Otherwise, compulsory field unitization does not necessarily imply that low-cost wells will produce at the maximum profit rate. Given the actual market context, both market demand prorationing and compulsory unitization would probably come to exist side by side, if for no other reason than that compulsory unitization cannot be achieved without implementation by an agency of the state. Public officials could very well persist in their present judgment that the public interest requires production of more oil from marginal fields than would otherwise be the case. Thus, efficient low-cost fields would be cut back to make room for higher-cost marginal fields (and possibly even for marginal wells in the supramarginal fields).

Elected public officials are likely to persist in their current attitudes in view of an inherent bias in the legislative arrangements of a democratic nation in favor of small businesses, which are vastly more numerous than large ones in many industries.<sup>2</sup> The possible social benefits of such a dispensation are not at issue in the present context; it is the economic implications which are pertinent. The larger integrated producers typically produce a greater portion of their crude oil from supramarginal prorated wells than from marginal exempt wells; for the small independent producer, the reverse is true. Hence elected officials have a certain incentive to favor marginal wells when granting allowables. In the absence of such political pressure, a more rational allocation of production among wells could be achieved. But under unitization, the regulatory commission would be subjected to even greater political pressure since compulsory unitization entails much more comprehensive control, is regarded with suspicion, and is generally unpopular among producers. Individual small producers oppose unitization out of the fear that they will be unable to assert their interests in particular fields where one or more majors have large holdings. The majors are uneasy about unitization in view of the proven ability of small operators and royalty owners to organize to influence regulation and press for special legislation. The resulting pattern of regulation under unitization might be more strongly stamped with the imprint of political expediency than is the present system.

There is little doubt that the protection of stripper wells has been carried too far. But whether under prorationing or unitization, there may be valid eco-

<sup>2</sup>It is scarcely necessary to give examples, beyond a reference to the "fair trade" laws, the Robinson-Patman Act, and the laws regulating the various transport media, particularly with regard to entry policy.

nomic reasons for preserving some production from currently submarginal wells for certain periods of time. Over the long run, the petroleum-supply picture is characterized by great uncertainty in the exploration phase, with a very strong purely random influence on the level of annual discoveries. Consequently there is a chronic imbalance between current additions to supply and current levels of demand. Instead of being able to find new reserves precisely when they are needed, there is a recurrent (but not periodic) bunching of major discoveries, with intervals of sparse discoveries in between. Since the long-run supply curve seems to be characterized by increasing costs [6, Parts I and III] [4, Parts II and III], it is in many cases more costly to allow stripper wells to be abandoned (with the resulting loss of physically readily producible reserves) when supply temporarily exceeds demand at price levels necessary to maintain them in production, than it would be temporarily to curtail the output of supramarginal wells. If such marginal wells are abandoned, and if supply later declines and prices increase, it may cost more to drill entirely new wells at higher costs to replace reserves lost through prior abandonment.

To repeat: this is not to say that all prorationing practices currently in use can be justified on grounds of long-run economic efficiency. It is possible to pay too much to keep marginal oil in the supply picture, but it is also possible at times to pay too little. In 1931, as Davidson cites, the price of oil in the East Texas field fell to 10¢ per barrel. No one believed that the long-run supply curve for crude oil was at 10¢ per barrel. As a matter of public policy it was felt wise to protect marginal producers who were producing at a cost of over 10¢ per barrel. Regulation subsequently established a higher price, but submarginal wells which were not profitable even at the higher price still had to drop out. Then, with the onset of World War II, it was necessary to subsidize production, and it appeared fortunate that the abandonment of marginal wells of the 1930's had been prevented. This was not merely a wartime phenomenon, however; as late as 1948, all wells in the country were producing at approximately their maximum efficient rates of production as defined by the National Petroleum Council, in order to meet demand, even at increased prices.

Furthermore, Davidson tends to overstate the case against current conservation methods by failing to allow for the extent to which the latter have been rationalized over the years.<sup>8</sup> Hence, although Davidson concedes that prorationing diminishes the effects of the rule of capture, he fails to make clear

<sup>8</sup> Progress has taken the forms of (1) more precise measurements of MER, (2) provisions for pooling agreements, (3) arrangements (usually voluntary) for unitization, (4) a shift away from prorationing formulas emphasizing per-well allowables (which stimulate over-drilling) toward formulas emphasizing per-acre allowables, and (5) successively requiring wider and wider well-spacing. In recent years, wider well-spacing has been the general rule, particularly in the two most important oil-producing states, Texas and Louisiana. In 1962 Texas increased its minimum well-spacing regulation from one well in 20 acres to one well in 40 acres, and Louisiana increased its minimum from a former range of 18 to 40 acres per well to a requirement of 80 acres per well. See *Oil and Gas Journal*, March 12, 1962, p. 64; Oct. 8, 1962, p. 70.

the extent to which the present system is superior to the unrestricted rule of capture. It is important to understand how greatly the rule of capture has been modified by the adoption of pooling agreements, field unitization, and minimum well-spacing regulations in producing states. The major cost of the old rule was in the host of unnecessary wells it caused to be drilled; well-spacing rules reduce this waste directly. In fact, no less than 26 states now have compulsory pooling statutes which require leaseholders to pool their acreage to form drilling or prorationing units meeting certain minimum standards. Of the major producing states, only California, Texas, and Kansas do not have such statutes, and bills to provide for compulsory pooling have been recently introduced in the legislatures of Texas and Kansas. There is still room for improvement, but it is improper to ignore the substantial progress that has been made. It is likely that the eventual competition of shale oil will force further rationalization of the conservation system.

That compulsory unitization is either a necessary or a sufficient condition for the achievement of efficient oil reservoir production may therefore be questioned. Under the present state conservation statutes, virtually the same results which Davidson advocates could be achieved by modifying spacing rules.<sup>4</sup> Obtaining such modifications is once again a matter of influencing the attitudes of public officials. Unless their attitudes are altered, changing the form of regulation may have no effect on its substance. But the political difficulty (and the economic cost attendant thereto) of modifying spacing rules is apparently less formidable than that of instituting compulsory unitization, as is witnessed by the cases of Texas and Louisiana, referred to above.

## II. *Crude Oil Prices*

Davidson argues further that, even if unitization could be achieved, large integrated refiners would not be willing to pay a price for crude oil which would reflect accurately the demands of consumers on grounds that a firm with a net *domestic* self-sufficiency ratio greater than 77 per cent benefits from an increase in U.S. crude oil prices relative to product prices [2, p. 99]. Some questions may, however, be raised in regard to Davidson's statistics and conclusions. He states that in 1956 there were eight firms that would clearly benefit from a rise in the price of crude without an increase in product prices, and he adds that it is these firms which "... are typically the posted price leaders in many of the major producing fields" [2, p. 99]. Davidson is referring to data on page 224 of DeChazeau and Kahn [3], but he has incorrectly used the data on the ratio of company world-wide production to world-wide refinery runs (column 6) instead of the ratio of domestic production to domestic refinery runs (column 3). It is not only contrary to Davidson's own explicit contention to use world-wide ratios, but it is also economically inappropriate since import controls (at first voluntary and then mandatory) have largely insulated the domestic price of crude oil from repercussions from

<sup>4</sup>Spacing rules may be modified directly by decreeing minimum well-spacing or indirectly by penalizing closer spacing through reduced allowables. A flexible use of well-spacing rules can eliminate overdrilling and achieve most (although probably not all) of the benefits of unitization.

changes in the foreign price structure for crude oil. Actually, in 1956 there were only four such firms with a domestic production ratio in excess of 77 per cent of domestic refinery runs. The nation's largest refiners and its principal purchasers of crude oil all had a self-sufficiency ratio below 77 per cent. The firms that might be considered the "typical posted price leaders" (Cities Service, Gulf, Phillips, Shell, Sinclair, Socony-Mobil, Standard Oil of California, Standard Oil of Indiana, Standard Oil of New Jersey, and Texaco) had domestic self-sufficiency ratios ranging from 28 to 60 per cent. Only four of the 30 largest companies (Continental Oil, Ohio Oil, Skelly, and Sunray Mid-Continent) had net domestic production in excess of 77 per cent of their domestic crude runs, and these four firms together accounted for less than 6 per cent of the nation's net production of crude oil. It is thus factually incorrect to argue that these four firms (which are among the smaller of the group) had "... a good deal of oligopsony power ..." and were "... the posted price leaders in many of the major producing fields." By 1961, the number of such firms had declined from four to only three: Continental, Ohio Oil, and Skelly<sup>6</sup> [1, p. T-49].

Upon reflection it would seem that the greatest power to influence prices is possessed by those who have no great incentive to raise prices. The very fact that a firm is 77 per cent (or more) integrated means that it purchases relatively little crude and hence its contribution to the total effective demand for crude oil which is bought and sold prior to refining is relatively minor. Firms with lesser degrees of self-sufficiency have something to lose by higher prices, but they must buy more crude, and their greater effective demand would tend to increase prices in spite of their own interests in lower prices. These latter firms are in fact the chief buyers in many important fields, as can be verified by reference to *Platt's Oilgram News Service*, where such companies are listed daily as "reference crude buyers," and their posted prices are reported. These companies include Gulf, Texaco, Humble, Shell, Sinclair, Standard Oil of California, Standard Oil of Indiana, Cities Service, Phillips, and Socony-Mobil. Davidson's analysis also assumes that these companies can set the prices of refined products in complete disregard of the role of independent refiners and marketers, a growing segment of the market. Davidson's conclusion [2, p. 99] that "... in regional markets where there is a good deal of oligopsony power, the oligopsonists may have a vested interest in a high crude price [relative to product prices]" is simply not supported by the evidence.

Davidson realizes that a firm with a lower self-sufficiency ratio will benefit from crude price increases if product prices may also be increased. But his conclusion that "increased earnings due to product price increases are quickly

<sup>6</sup> By 1961 the separation of world and domestic crude oil markets had been made even more distinct by virtue of the institution of mandatory import controls in 1958. (Prior to 1958 the importing companies, under pressure from domestic producers, had individually exercised voluntary limitation of imports.) Mandatory import controls tended to favor the smaller refiners relative to the larger, in that allowable imports relative to refinery runs range from 12 per cent for smaller refiners down to about 8 per cent for larger refiners. Even allowing for permissible imports, none of the major integrated companies attain a 77 per cent self-sufficiency ratio.

passed down the integrated chain to the tax-sheltered production segment . . ." [2, p. 99] is open to serious question. As McKie has shown [5], entry is particularly free in crude oil production. Given such free entry, in the long run it is not possible to increase unit profit margins in crude oil production by raising prices. Any such price increases will lead to new entry and a restoration of equilibrium profit margins through the price reductions consequent upon increases in supply relative to demand.

It is also incorrect to argue that the major integrated producer-refiners have a great incentive to raise crude oil prices and increase total profits because of the inelasticity of demand. While it is true that the total demand curve for crude oil is markedly inelastic, the demand curve as seen by the typical major integrated company is apt to be much less inelastic. This comes about because the major integrated company is typically producing the bulk of its oil from supramarginal prorated wells while the small independent more often produces most of his oil from stripper wells exempt from prorationing. Since total demand is not perfectly inelastic, an increase in price will cut output, but all of the decrease in output would fall upon production from prorated wells, hence making demand much more elastic for such wells. That the quantitative increase in elasticity of demand is appreciable is evident from the fact that about 40 per cent of all production in Texas is exempt from prorationing. Therefore, depending upon the actual elasticity of demand and the majors' share in total and in prorated output, the major integrated firms might do themselves a disservice, and in any event would confer a relative benefit upon the small independent producers at their own expense, if they increased crude oil prices and thus raised the prices received by all crude producers, absorbing almost all of the impact of reduction in volume sold while leaving the sales of their smaller competitors virtually unchanged.

Davidson also argues that because of crude oil "trading" there is a built-in rigidity in crude prices since each refiner wants to prevent a decline in the price of the crude he "controls" [2, p. 100]. This argument is faulty in that it considers only one side of the market. Crude oil prices are generally determined on a field-by-field basis. The company which is a net purchaser on balance in a field will be interested in lower prices while the firm which is a net seller will desire higher prices. Since there must be a buyer for each seller, it would be more reasonable to suppose that the two opposing forces are more or less in balance, and that the secondary market for crude oil is essentially neutral in its effect on prices, except insofar as it reduces costs by eliminating cross-hauling and by channeling specific grades of crude oil to those refineries which can best utilize them.

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\*The author, assistant professor of economics at Rice University, has benefited greatly from discussions with several economists in the petroleum industry.

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### Public Policy Problems of the Domestic Crude Oil Industry: A Reply

In this reply, most of the space will be devoted to a discussion of Campbell's comment, for it indicates the necessity to elaborate some points of my article, and it requires further discussion about the rate of exploration. Steele's comment, on the other hand, is primarily based on what he believes to be the relevant political, rather than economic, considerations. His argument lacks analytical rigor and is, in fact, devoid of analytical content. (Moreover, his attempt to demonstrate "imperfections in the mode of analysis [of my paper]" suffers from logical errors.) Given the space limitations, therefore, I will give only brief consideration to some of the major deficiencies in Steele's argument at the end.

#### I. Reply to Campbell

Campbell has raised two issues: (1) the elasticity of supply of oil lands and its relation to the extensive margin, and (2) the effect of uncertainty on contractual rent payments.<sup>1</sup> Space limitations in my article allowed me to mention these issues only obliquely and I am grateful for the opportunity now to elaborate on them.

Campbell has set up a straw man only dimly related to my model. He proceeds to "completely vitiate" the "strict version [straw man] of Davidson's conclusion—that a subsidy will not increase exploration at all." This is not an accurate interpretation of my position, for I have stated that "percentage depletion does not *significantly* encourage the allocation of additional resources to exploring presently nonproductive oil lands . . . [it] is *primarily* an ad valorem subsidy to mineral rights owners" [3, p. 107, italics added; also see pp. 102]. In other words, the present depletion policy is an inefficacious means for encouraging the allocation of additional resources into the exploration of oil lands.<sup>2</sup>

<sup>1</sup> Campbell, however, misses the significance of the latter point, that is, that with lease contracts and uncertainty, actual rent payments to landowners (which are based on *ex ante* rents) need not equal *ex post* rents.

<sup>2</sup> Inefficacious in the sense that most of the subsidy dollar goes to windfall rents and very little remains to encourage producers to explore.

Having demonstrated that the straw man's (but not my) conclusion is wrong, Campbell implies that the opposite inference must be true, i.e., that the depletion allowance will significantly encourage exploration. To buttress this vulnerable position, Campbell introduces three items: (1) a passage from "the most authoritative academic students of the industry," who, it turns out, are merely citing works of others; (2) a visceral belief, without justification, that the depletion allowance "affects the flow of innovations in the industry"; and (3) some statistics on oil reserves which are meant to be "suggestive" of the elasticity of oil lands.

None of these items analytically advances Campbell's position. His appeal to authority as well as his visceral beliefs are extraneous. Thus, all that remains to be disposed of are the statistics on oil reserves.

Before turning to an analysis of this issue, it will be useful to present a brief list of the areas of agreement between Campbell and myself, followed by a list of areas of disagreement. The areas of agreement are:

1. Campbell apparently agrees with my analysis of the problem of conservation [3, pp. 85-101], including the analysis of crude oil prime costs such as operating costs, user costs, and royalties. Furthermore, he appears to accept the notion that, although royalties are an important component of the short-run supply function, they are, in the long run, price determined.

2. He apparently accepts the analysis on the "Crude Oil Buyer" which demonstrates that, in the short run, percentage depletion gives many integrated refiners a vested interest in higher crude oil prices and as a result tends to distort short-run production decisions from the social optimum [3, pp. 98-101]. Thus, Campbell must agree that, given the integrated structure of the domestic industry, percentage depletion is an undesirable public policy from a short-run viewpoint.

3. Campbell explicitly states his agreement that mineral rights owners normally receive economic rents.

The areas where Campbell and I disagree are the following:

1. He states that "the critical assumption underlying Davidson's conclusion is that the supply of new oil land is perfectly (or nearly perfectly) inelastic . . . [and] assuming homogeneity, Davidson's argument then follows nicely." But I never assumed homogeneity! Campbell apparently leaped to this erroneous conclusion by missing the relevance of my statement that "the landowner of any undeveloped oil property exhibits a perfectly inelastic supply function for his mineral rights—there is no alternative use" [3, p. 104]. In my discussion of the incidence of a reduction in the depletion allowance, the reference to the individual landowner's inelasticity of supply merely indicates that any payment to the landowner is an economic rent which can be taxed away without any resulting allocative effects.

Oil lands are obviously analogous to the Ricardian case of agricultural lands of differing fertilities, and I shall show below that the policy conclusion of my article is completely consistent with this fact.<sup>8</sup>

<sup>8</sup> The similarities between the differential fertilities case and the homogeneous land case have been so ubiquitously developed in the literature that I felt little compulsion to elaborate on this point. Campbell has indicated that I should have.

2. In his second footnote, Campbell implies that I believe that the royalty rate must always equal the after-tax value of the depletion allowance on corporate income. This is quite surprising since in the same footnote Campbell states "... as Davidson points out, [royalties are] not the only incentive a rights owner has for leasing—bonuses and per acre rentals are usual and sometimes substantial in amount." Since economic rent payments to landowners can take several concomitant forms, while the depletion allowance is not the sole source of economic rents, it should not be surprising to find that the royalty rate does not necessarily exactly equal the economic rent arising from percentage depletion. Campbell's apparent failure to comprehend the role of uncertainty in the determination of contractual commitments to pay economic rents leads him to place too much emphasis on my arithmetic illustration which explicitly assumed perfect foreknowledge [3, p. 104, n. 21].

#### A. *Uncertainty and Economic Rents*<sup>4</sup>

Uncertainty about the future is the critical part of most investment and production decisions. Normally we abstract from uncertainty by postulating a certainty equivalent.<sup>5</sup> As I have already indicated, if the future income stream of each oil-bearing property is known with certainty, then the lease bonus would exactly equal the discounted value of the future economic rents on the property, and all rents would go to the landowner. There would be no need for royalties [3, p. 104].

Often, when purchasing a capital asset, the estimated future income stream is known with such a high degree of probability that the buyer and seller can agree on a fixed purchase sum which each feels approximates the *ex post* economic rent. This is not, however, true of oil leases. Given the large dispersion around the expected mean value of an oil property (the probability of an extreme result such as a dry hole or a gusher is significant), both the producer and the mineral rights owner recognize that the probability of the *ex ante* rent even approximating the *ex post* economic rent is extremely small. Consequently, a royalty payment based on a percentage of the gross value of crude oil as it is produced allows the producer and the landowner to share in the inherent risks. The way the parties contractually agree to split the expected economic rent into a fixed sum (the lease bonus) and a variable sum (the royalty) depends, in part, upon the expected risks for different classes of oil-bearing properties. As one expert in petroleum engineering described the process:

If the land is remote . . . and the presence of oil beneath the tract is uncertain, the owner may have to be content with a small royalty . . .

<sup>4</sup>For an excellent discussion of this whole question see [8, Ch. 9].

<sup>5</sup>For example:

An entrepreneur, who has reached a practical decision to his scale of production, does not, of course, entertain a single undoubting expectation of what the sale-proceeds of a given output will be, but several hypothetical expectations held with varying degrees of probability and definiteness. By his expectations of proceeds I mean, therefore, that expectation of proceeds which, if it were held with certainty, would lead to the same behavior as does the bundle of vague and more various possibilities which actually make up his state of expectation when he reaches his decision [4, p. 24 n. 3].

[and] without any cash consideration or bonus. If the land is near productive acreage and the structural conditions seem favorable, the owner may demand an initial payment or bonus in addition to a substantial royalty. Some owners prefer to have a large initial bonus and a small percentage . . . while others will be willing to share the risks, and receive a smaller bonus, or none at all, in return for a larger royalty. Generally, the lessee prefers to offer a higher royalty in lieu of a large bonus, as this arrangement reduces the preliminary outlay that is sacrificed in the case the property becomes nonproductive [9, p. 169].

Accordingly, in a world of uncertainty, there is no guarantee that the royalty rate must equal the after-tax value of the depletion allowance.<sup>6</sup>

Since once oil is discovered, the lease contract normally remains in force as long as oil can profitably be produced, if the landowner has (from hindsight) made a bad deal, the producer will end up with some of the *ex post* economic rent. On the other hand, if the producer's expectation proves to be too optimistic, then in the short run the landowner might receive more than the *ex post* rent. Since, in the latter case, the producer will earn less than a normal return on his investment, it will not pay him to develop the property under the existing contract. Thus, statistical data on actual rent payments to mineral rights owners (such as the data introduced in the next section) are likely to *underestimate* the *ex post* economic rent on producing properties, because only that property on which, under existing contracts, the landowner receives less than or, at most, an amount equal to the *ex post* economic rent will be developed and remain in the statistical series.

The key point is that, given uncertainty, the depletion allowance merely raises both the landowners' and producers' estimates of the *ex ante* rent. There is no reason to believe that this subsidy will systematically increase the amount of the *ex post* rent that the producers receive as compared to the amount the producers might receive in the absence of the depletion allowance, and therefore it should not significantly encourage additional exploration of oil lands.<sup>7</sup>

<sup>6</sup> The actual contractual agreement on the mode of making rent payments to the landowner depends on a number of factors, including expectations about the future income stream, the subjective estimates of the risks involved, the risk-aversion functions of the landowner and the producer, and the ability of the producer to obtain financing. The popularity of the  $\frac{3}{8}$ th royalty might be partly explained as an institutional rule-of-thumb, for experience has shown that a  $\frac{3}{8}$ th royalty takes about half of the revenue that remains after paying all operating expenses "in the average case" [9, p. 170].

<sup>7</sup> If the producers had better information about prospective yields than landowners, and if there were collusion among producers in their bidding for leases, then the producers could systematically keep some part of the *ex ante* rent for themselves by making favorable contracts. If this collusion exists (and both Campbell and I agree that it probably does not exist in the domestic market), a reduction in the depletion allowance would tend to bring about a reduction in collusive bids. The producers could still obtain a share of the *ex ante* rent, while passing the entire burden of the tax back to the landowner. Of course, to the extent that producers were obtaining a share of the rent, then this portion of the producers' income could be taxed away without altering allocative decisions.

### B. *The Inelasticity of Supply and the Extensive Margin*

Since oil lands are not homogeneous in their oil-bearing potential, Campbell is ultimately driven back to examine the question I originally raised—that of the elasticity of supply of oil lands, for “the more inelastic the supply of mineral lands, the greater will be the landowners’ windfalls, while the additional exploratory activity will be minor” [3, p. 102]. If these windfalls are large, then (1) the depletion allowance is primarily a subsidy to mineral rights owners, and consequently (2) the present depletion policy is an exceedingly expensive (for the taxpayers) way to encourage additional exploration, and (3) my initial conclusion about the efficacy of the present depletion allowance remains undamaged.

Campbell presents some statistics on oil reserves which he claims “are suggestive”; but these figures actually do not have any direct relationship to the relevant elasticity. For example, Netschert’s estimate on “ultimate reserves” only indicates an external constraint to the supply schedule and states nothing about the elasticity of supply.<sup>8</sup> Furthermore, the fact that cumulative proved reserves “nearly doubled in the period 1944-60” has no relevancy to the elasticity of supply of oil lands, and although Campbell argues that it is “suggestive,” he does not show any connection between proved reserves and the relevant elasticity.

The development of proved reserves is really a “shelf-inventory” problem and has no direct relationship to exploration of presently nonproductive oil lands. In a forthcoming essay, M. A. Adelman has an excellent and thorough discussion of this shelf-inventory point [1]. For my purposes here, it is sufficient to indicate that Adelman is able to set up analytically a classificatory scheme in which discovered, but yet underdeveloped, oil fields are considered to be raw material inventory, while reservoirs under development are work-in-process inventory, and fully developed fields (i.e., proved reserves) are merely shelf-inventory.

The level of shelf-inventory is, of course, primarily a function of expected sales, and since sales have continued to increase since 1944, it is not surprising to find “cumulative [proved] reserves nearly doubled during the period.” Statistics on the development of shelf-inventory do little to clarify the question of the elasticity of supply of nonhomogeneous oil lands.

Fortunately, however, there are other data which enable us to judge the elasticity of supply. If the supply of nonhomogeneous oil lands is very elastic, then the rent on intramarginal oil lands will be quite small, while if the supply is quite inelastic, then intramarginal lands will yield large *ex post* economic rents. Bain has provided some estimates of actual rent payments received by owners of producing (intramarginal) oil properties in the state of California for the period 1929-40 [2, Vol. 2, p. 84]. Bain’s data show that total rent payments to landowners over this entire period exceeded the producers’ total income by approximately 18.5 per cent (or \$96 million). Even though this

<sup>8</sup> Campbell’s footnote 7 suggests that estimates of ultimate reserves should be taken with a grain of salt.

estimate is, as suggested above, biased downward, it is apparent that huge economic rents are earned on intramarginal lands. It follows therefore that the supply of nonhomogeneous oil lands is highly inelastic and that percentage depletion added substantially to the huge windfalls received by landowners.<sup>9</sup> Accordingly, for each dollar of subsidy granted, most went to intramarginal rents, and little was left to encourage the exploration of otherwise intramarginal no-rent land.

### C. *Exploration: Intramarginal vs. Inframarginal Land*

Bain's statistics provide strong support for my initial conclusion that the present depletion tax policy is an inefficient method of encouraging additional exploration. A reduction in the depletion allowances will not therefore significantly discourage exploration, although some marginal oil lands may once again become inframarginal.

As long as large areas of intramarginal oil lands remain to be explored, however, it seems unwise to encourage the exploration of any lands with inferior potential.<sup>10</sup> Moreover, we are already pursuing other public policies (prora-

<sup>9</sup> Interestingly, in another volume of his monumental study, Bain adopts an analytical argument similar to mine when he proposes an increase in the oil severance tax. Bain argues that the increased tax will have an impact on the large oil rents, including those obtained by the producer (for Bain believes that producers manage to obtain some *ex post* rent via favorable contracts). He cautions that "vigilance would have to be exercised to insure that its [the severance tax] burden is not shifted between producers and landholders through the revision of royalty terms on new leases" [2, Vol. 3, p. 60]. Bain expected a vigorous attacks on his proposal, but he argued that huge rents have occurred and the tax scheme he proposed "would recover only the unnecessary excess" [*Ibid.*, p. 60].

The fact that income received by landowners of oil properties involves huge economic rents is well recognized in oil-producing states where local tax collectors have normally assessed royalty oil at a substantially higher valuation than a barrel of producer's oil from the same well [9, p. 175].

<sup>10</sup> Campbell cites a National Petroleum Council estimate that it would take 50 years to explore all presently known potentially profitable oil-bearing lands. What determines the rate of exploration on this intramarginal land and why does so much intramarginal land remain to be explored? Many more analytical studies are needed before a definitive answer can be formulated to these questions. Some possible lines of inquiry, however, can be suggested.

Many factors contribute to the actual rate of exploration. For example, imperfections in the capital market make the financing of lease contracts and exploratory activity difficult. Also many acres of intramarginal land are already under lease and will be explored only when the lessee believes it is profitable to do so (which, in turn, depends on several factors including expected future prices and costs, i.e., the user cost of raw materials, the scarcity of geological teams and managerial ability, etc.).

From a conceptual point of view, Lerner's distinction between the marginal productivity (or efficiency) of capital and the marginal efficiency of investment [6, pp. 334 ff.] might usefully be employed to explain the rate of exploration of intramarginal land. Although Campbell is correct that "no two acres have exactly the same potential" (cannot the same statement be made for any factor of production?), there are sufficiently similar attributes so that violence is not done to reality if oil lands are divided into a relatively small number of quality classes, where the between-class variation is significantly larger than the in-class variability. We can then speak of a separate marginal efficiency of capital (in Lerner's sense) for each class of oil land. For those classes where the marginal efficiency of capital exceeds the rate of interest, investment in exploration will be going on. That class where

tioning and import quotas) which have resulted in higher crude prices and consequently have encouraged exploration of land which otherwise would have been inframarginal.<sup>11</sup> Isn't it the height of folly to continue a tax policy which attempts to push out the extensive margin even farther, while intramarginal lands remain to be explored?

Nevertheless, if it is believed that a pushing-out of the extensive margin is in the public interest, then a more efficient public policy would be to provide government support of geological research. The resulting technological improvements would move out the extensive margin (as Campbell recognizes in his footnote 5) and might at the same time reduce the rent on intramarginal lands.

## II. *Reply to Steele*

Although Steele concedes "the purely economic desirability of unitization," he nevertheless questions the "validity" [practicability?] of my conclusions on several grounds. Below I briefly discuss the most obvious deficiencies in Steele's argument.

1. Steele's defense of prorationing because of our inherent democratic bias in favor of small business is silly. Prorationing is normally defended by its advocates as primarily an instrument of conservation. Until now, no one has seriously attempted to argue that market prorationing is primarily a result of a conscious public desire to misallocate resources and redistribute income to the small crude oil producers. (Even if there is a public desire to improve the income position of the small crude producers, surely as economists we would prefer a system of lump sum taxes and subsidies that would achieve the desired distributive effects without incurring the economic wastes of prorationing.) The fact that the small producer tends to be unregulated and therefore gains the most from present prorationing arrangements is merely "a sop to practicality for it avoids the regimentation of a host of small enterprises" [3, p. 97].<sup>12</sup>

If, however, Steele wishes to defend prorationing on the basis of an "inherent bias," I might remind him that this nation also has an inherent

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the marginal efficiency of capital equals the rate of interest will be marginal land, and all other classes will be inframarginal. The rate of exploration per period will be determined by the marginal efficiency of investment for each class. The equilibrium rate of exploration per period is that rate which adds expected new reserves from each intramarginal class up to the point where the value of the marginal input in exploration equals the expected value of the marginal addition of the capital asset.

<sup>11</sup> The rent-creating aspects of these policies have been excellently developed by Adelman [1]. Besides raising crude prices, the equity provisions of market prorationing make it more desirable to drill a marginal well than to drill a flush well. In East Texas, for example, a well that profitably produces less than 20 barrels a day will have no prorationing constraints, whereas a well which might profitably produce several hundred barrels a day could be restricted to a quota as low as six barrels a day. Under these conditions, exploration as well as production decisions are likely to be distorted.

<sup>12</sup> Similarly, present (summer, 1963) congressional discussions about exempting Mrs. Murphy's boardinghouse from pending civil rights legislation is more a result of the administrative difficulties of policing such a law than a reflection of an inherent democratic bias to allow small producers to be biased.

democratic bias for free markets, which field unitization could provide, while market prorationing does not. (Steele is wrong when he indicates that prorationing must exist side-by-side with unitization. To accept the *status quo* of prorationing as an unalterable institution is to remove any economic interest in the problem.)

2. Under prorationing, the decision to prolong the life of stripper wells does not have any economic foundation, despite Steele's suggestion that, in the absence of prorationing, stripper wells which have physically producible reserves might be abandoned when "supply temporarily exceeds demand." The possibility of physical producibility should not be the guiding criterion for continuing the use of facilities. If supply should temporarily exceed demand, then the user cost of raw materials would be the proper economic criterion for evaluating whether or not a stripper well should be abandoned;<sup>12</sup> and therefore, given unitization, this user cost will enter into rational production decisions.

Of course, in a world of uncertainty, mistakes in the foreseeing of future events may occur. Even though user-cost calculations are subject to human fallibility in predicting the course of human events (as is, of course, any method of conservation), user-cost computations under a system of unitization would supply the correct approach for husbanding oil reserves over time—a claim which cannot be made for any prorationing scheme.

3. Steele never refutes my argument that prorationing schemes do not correctly allocate oil resources over time. Nevertheless, he claims that "the present prorationing system is superior to the unrestricted rule of capture." To substantiate this, Steele would have to demonstrate that the economic cost of restricting production from low-cost supramarginal wells while allowing high-cost wells to be operated intensively (and simultaneously encouraging the drilling of new high-cost marginal wells [see footnote 11]) is less than the economic costs involved under the rule of capture.

The wastes of either of these systems manifest themselves differently, and since the domestic crude producing industry has operated throughout much of its life without compulsory production controls, and only on rare occasions were the wastes of the rule of capture as visually spectacular as the East Texas experience, it is not immediately obvious that the present system is, as Steele implies, significantly superior.

Moreover, the fact that 26 states have adopted some compulsory pooling statutes suggests that some of the alleged superiority of the present system may be a result of compulsory unitization laws rather than prorationing.

4. Steele is wrong when he suggests that "virtually the same results which Davidson advocates could be achieved by modifying [well] spacing rules." (Does this statement imply that Steele really agrees with the policy implications of my analysis, despite his protestations?) Well-spacing rules do *not* provide any economic guideline to the optimum rate of extraction of crude from a reservoir, while, as my analysis has demonstrated, profit-maximization

<sup>12</sup> For an excellent discussion of this point see Keynes' analysis of the carrying costs of redundant stocks in extractive industries in [5, Ch. 29] and [4, pp. 70-71].

decisions under unitization will result in the optimum rate of extraction over time.

To the extent that well-spacing rules are effective, they merely limit the economic wastes of overdrilling. Under unitization, however, only the minimum number of wells necessary to drain a reservoir economically would be drilled, and, therefore, well-spacing rules would be unnecessary.<sup>14</sup>

5. Steele's suggestion that I had "incorrectly used" statistics on domestic self-sufficiency ratios is itself erroneous. My information was not taken from the secondary source suggested by Steele, but was obtained from the industry periodical *World Oil* (June 1957, p. 101), which showed that there were eight refiners who had domestic self-sufficiency ratios that were greater than 77 per cent.

Furthermore, Steele doth protest too much when he indicates that the world-wide self-sufficiency ratios are not relevant. To the extent that refiners use their foreign crude to supplement their domestic crude supply, the domestic self-sufficiency ratio overstates the refiner's dependence on market purchases of crude. Accordingly, many more refiners will have self-sufficiency ratios (including foreign production) which exceed 77 per cent and will, therefore, have a vested interest in a higher short-run price of crude (both domestically and abroad). For example, Manes has shown that in 1962 eleven major refiners producing more than half of the total domestic output of refined products had over-all self-sufficiency ratios that equalled or exceeded 77 per cent. Manes correctly concludes that "where more than 50 per cent of the industry on both the buying and selling side of the market for the raw material has a vested interest in higher prices it is not unreasonable to conclude that industry and price leadership has been exercised in this direction" [7, p. 106].

6. Although Steele agrees that firms with lower self-sufficiency ratios will also benefit from a crude price increase which occurs concomitantly with product price increases,<sup>15</sup> he questions my conclusion that, in the short run, "increased earnings . . . are quickly passed down the integrated chain to the tax-sheltered production segment" [3, p. 99]. Despite the price data cited in my article, Steele rejects my argument because, "in the long run, it is not possible to increase unit profit margins . . . by *raising* prices" since this will encourage entry and restore "equilibrium profit margins through . . . price *reductions*" (italics mine).

Steele's position is obviously wrong, for he has mixed short-run and long-run factors. To argue, as Steele does, that short-run prices can never rise because if they do, in the long run they will fall, is logically indefensible. Once Steele admits that short-run prices can rise, then he implicitly accepts my position.

At this point, little would be gained by continuing the discussion of the

<sup>14</sup> The recent scandals about slant-drilling in Texas indicate that the equity provisions of market prorationing may actually encourage wasteful (and illegal) drilling.

<sup>15</sup> In 1956, for example, only two out of the top 31 refiners had a domestic self-sufficiency ratio below 37.5 per cent.

many other errors remaining in Steele's paper. For example, (1) he demonstrates that the demand facing prorated wells is less *inelastic* than total demand, but he concludes that it is "much more elastic"; or (2) in discussing crude oil trading, he implicitly denies that, in any field, the major purchaser from that field may be an integrated refiner who is also a major producer in that field, and therefore the buyer will not have a desire for a lower field price. The abundance of faults in Steele's Comment makes a constructive discussion between us of these vital policy problems impossible.

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#### An Econometric Analysis of Population Growth: Comment

In a recent article in this *Review*, Professor Irma Adelman took issue with a possibility that I mentioned in the appendix to Chapter 10 of my *Economic Backwardness and Economic Growth*, namely that, if investment rises and consumption falls, *other things equal*, mortality rates may rise with per capita income [2, p. 173]. The language used in the paragraph in question indicates that this is a tentative exploration of a theoretical possibility and not the assertion of a universal generalization. Mrs. Adelman appears to interpret my statement as a universal generalization. However, her study does not refute the *possibility* that the relation may hold for some countries under some circumstances.

Mrs. Adelman's regressions give a negative partial correlation between the rate of growth of real per capita income and death rates. She alleges that this finding is "in direct conflict" with my theory. However, this is

not so. She set out to explain mortality rates of different age groups by (1) the percentage rate of growth of per capita income, (2) the percentage of labor force outside of agriculture, and (3) the number of physicians per 10,000 inhabitants. Neither consumption (per capita or otherwise) nor investment appears in her mortality regressions. Consequently, Mrs. Adelman has not tested the possibility that I mentioned.

She further states that the flaw in my "contention" is that "even though a higher rate of growth of per capita income is associated with a decrease in over-all consumption, it is also associated with an increase in the amount of consumer products accruing to the lower income groups. This decreases mortality among the laboring classes without a corresponding increase in the mortality rate of upper income groups" [1, p. 329]. I grant that this is a possibility to be placed alongside my own. The point is, though, that, despite her claim, Mrs. Adelman has tested neither one.

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#### Erratum

In the article by Lowell Gallaway, "Labor Mobility, Resource Allocation, and Structural Unemployment" (*American Economic Review*, September, 1963), the exponents were omitted in the denominator of equation (5) on page 703. Thus, equation (5) should read:

$$R = \frac{\sigma^2 - \delta_0^2}{\delta_1^2 - \delta_0^2}.$$

## BOOK REVIEWS

### General Economics; Methodology

*Disarmament and the Economy.* Edited by EMILE BENOIT AND KENNETH E. BOULDING. New York: Harper and Row, 1963. Pp. x, 310. \$5.75.

*The Economic Effects of Disarmament.* Toronto: Toronto University Press; London: The Economist Intelligence Unit, 1963. Pp. xiii, 224. \$5.00.

As though to testify to the importance of, if not the necessity for, competent and painstaking studies of the probable economic impact of complete disarmament, these two books—each representing the culmination of a two-year research program—were prepared virtually simultaneously on either side of the Atlantic Ocean and were published within a few months of each other early in 1963. The signing of a limited nuclear test ban by the United States and the USSR shortly after their publication raises hopes for a termination and reversal of the arms race and adds further emphasis to the urgent need for studies of this type.

Both studies view the possibility of complete disarmament as an opportunity rather than as a cause for alarm—an opportunity to reduce taxes and to undertake useful programs with the released resources. They recognize that economic problems and dangers would exist, to be sure; but, by identifying them, by planning well in advance to cope with them, and by convincing the public of the necessity of adopting “a forward policy for optimum adjustment,” it should be possible to cause these released resources to “contribute more effectively to the highest-priority needs of mankind,” according to Professor Benoit in the last sentence of *Disarmament and the Economy*.

Of the two books, *Disarmament and the Economy* will almost certainly have the greater appeal for an American audience. It is the most comprehensive study of this particular aspect of disarmament yet to appear.<sup>1</sup> Except for Benoit, who directed the project and authored three chapters, or about one-quarter of the book, each of its 15 chapters was written by a different author or pair of authors—recognized authorities in their fields, e.g., Seymour Melman on the cost of inspection, Wassily Leontiff and Marvin Hoffenberg on the application of input-output analysis, Daniel Suits on econometric analysis, and Richard Nelson on research and development. This makes for broad and interesting coverage. It also inevitably results in some repetition and lack of agreement on various points (although all start from a common and agreed-upon set of assumptions), and opens up lines of inquiry calling for further research. Almost any of the chapters could be expanded into a book of its own. This may leave the reader slightly up in the air, but it is also bound to be stimulating and provocative. It would be misleading to suggest that pat answers could be provided for the complex problems raised.

<sup>1</sup>For example, the monumental *Arms Control, Disarmament, and National Security*, edited by Donald G. Brennan and published in 1961, devoted only one 11-page chapter of its 475 pages to “Economic Implications of Arms Control,” by Kenneth Boulding.

It is Emile Benoit who gives continuity to the book: in Chapter 2 he defines and describes the disarmament model which is basic to the entire study—one of “general and complete,” multilateral disarmament, “broadly consistent with the U.S. outline . . . presented at Geneva in 1962.” He translates this politico-military model into an economic model. In Chapter 12 he outlines various “high-priority” alternative uses which total altogether twice the estimated resources that would be released by the postulated disarmament. The problem as he sees it is not so much one of finding important alternative uses of released resources as it is one of adopting the proper monetary and fiscal policies “to endow the chosen programs with enough purchasing power to make them effectively able to bid for and absorb the released resources.” This in turn is his subject for the final chapter of the book, in which he examines seven alternative fiscal programs for adjusting to the assumed \$32 billion defense cut and selects one of them as the preferred solution: the “Balanced Stabilization Offsets Program” with tax cuts about equal to new government expenditure programs.

The other chapters of *Disarmament and the Economy* add flesh to this skeleton. Particular attention is given to presenting various economic “tools” for analyzing the impact of disarmament on production, on research and development, and on employment. These for the most part take the form of models of one type or another: an interindustry model, an econometric model, and a set of five models—(1) tax-cut, (2) debt-cut, (3) federal spending offset, (4) combination, and (5) a “judgment” model.

The British work, *The Economic Effects of Disarmament*, differs from the U.S. study in a number of respects apart from its concern with a quite different and much smaller economy. More than half of the British book (all of Part I) is devoted to a description of “the current level of defense expenditure in the United Kingdom” as a necessary preamble to a discussion of what might happen when this level of expenditure is sharply reduced by disarmament. Part II (some 93 pages) deals with “The Transfer of Resources from Defense Expenditure.” It starts with a brief review of disarmament experience following World War II, which is found to be virtually irrelevant because of changed conditions. Four main uses to which resources released by disarmament might be put are discussed, but without any recommendation as to an “optimum” or preferred distribution. The four types of expenditures include: private consumption, investment, and government current expenditure (a) at home, and (b) abroad. The principal conclusion is that “the need to maintain expenditure is of paramount importance; revenue considerations are of a secondary nature.” There is considerably less emphasis on methodology and model building in the U.K. study than in the U.S. study and more emphasis on the presentation of statistical material, both factual and hypothetical. Both studies assumed that “it would undoubtedly be the policy of the government of the day to keep . . . industrial dislocation and unemployment . . . to a minimum” under a program of complete disarmament.

A unique feature of the British study is its finding that “there appears to be some advantage in keeping the transition period as short as possible”—

two years or less (in contrast to the twelve-year transition felt to be necessary by the Americans). Advantages of the short transition (consistently favored by the Soviets) are found to be: (1) minimize uncertainty, (2) greater psychological impact, (3) readier public acceptance while memory of the agreement is fresh, and (4) minimize wasteful use of resources.

In the U.K. study considerable attention is given to the way in which individual firms are affected by defense expenditure. Surprisingly, in order to obtain data it was necessary to send out a special questionnaire "to a selection of companies in the industries known to carry out the major part of defense work"—aircraft, armaments, electronics, shipbuilding, etc. Nor was it known which companies actually were engaged in such work—"no official information is available on this subject." Permission to send out the questionnaires was obtained from the Ministry of Defense. Of 455 companies contacted, 251 replied and 142 reported that they had defense work. The data collected have the merit of being quite up to date, mostly based on the final quarter of 1961. However, the questionnaire was very brief and the number of respondents small, particularly for use in industry-by-industry analysis (aircraft industry: 8 respondents; electronics: 44; shipbuilding: 15). In a number of instances further details as to what might be expected under disarmament were obtained by means of interviews. All of this points to a need for improved data both quantitatively and qualitatively.

In spite of shortcomings, it is encouraging to find two major studies published in 1963 devoted to analyzing the economic effects of complete disarmament and to discussing alternative ways of avoiding sharply deflationary effects. Both studies are interesting and clearly presented and should be a stimulus to much-needed further thought and study in this area.

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*Growth, Employment & the Price Level—Intermediate Macroeconomic Measurement, Theory & Policy.* By F. V. WALKER. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. ix, 342 \$6.95.

*Macro-Economics—The Measurement, Analysis, and Control of Aggregate Economic Activity.* By T. F. DERNBURG AND D. M. McDUGALL. New York: McGraw-Hill Book Co., 1963. Pp. x, 310. \$6.95.

The name of Keynes is anathema in some circles. Despite diligent efforts to distinguish the analytical techniques from the deficit-spending policies, confusion and suspicion remain as to just what is encompassed by "Keynesian Economics." As a result, courses bearing the name of Keynes are very scarce and one publisher (not here represented) even boasts that his elementary economics book contains no mention of Keynes.

All is not lost. There is available a very competent economist called "Macro." This economist, Macro, is most versatile. He not only encompasses everything that Keynes taught, might have taught and would have denied teaching but also supplants fields that used to take an entire semester or year to teach, such as business cycles, fiscal policy, and monetary policy.

These separate fields are also, of necessity, introduced in elementary courses in economics. There, however, the aim is to teach humility. The coverage (sometimes in even briefer compass) in the upper division courses in macro-economics purports to be adequate to evaluate live policy and perhaps even turn out real, genuine policy-makers. These "macro" courses need textbooks (we assume that was the sequence), and some very able economists have produced some very able volumes to fill the need.

The two books reviewed here are among the ablest. They take us through the problems of income estimation, the theory of income determination, the theory of economic growth, the causes of business fluctuations, and the use of fiscal, monetary, and other economic policy to deal with cyclical and growth problems. They are up-to-date in their analysis and present an excellent review of the available material.

F. V. Walker provides an analytical basis for considering the problem of maintaining a rate of economic growth adequate to employ the labor force while controlling inflationary tendencies. He presents a unified theory of growth and income determination. Attractive features of the book are: (1) a critical evaluation of national income estimates; (2) a simplified but highly informative discussion of sector analysis and flow-of-funds accounts; (3) a succinct historical summary of trends and cycles in the U.S. economy; (4) a review of the currently important question of the effects of built-in stabilizers and destabilizers; (5) an incisive analysis of price-index techniques; (6) a good circular flow analysis in terms of "net leakage" and "net pumpage"; (7) summaries at the end of each chapter and bibliographies at the end of each major topic; and (8) a very readable style.

Policy recommendations are interspersed throughout the book and there is a brief concluding chapter also dealing with fiscal, monetary, and other government policies. The brevity of the treatment of important policy matters is actually a blessing in some respects: no reader would be deluded into thinking that he has been given a substitute for comprehensive courses in monetary and fiscal analysis. The limited space accorded any specific policy, however, raises the question whether more harm than good results from such treatment. In referring to the possible wider use of sales taxes, for instance, Walker says, "The drawbacks are that such taxes abridge effective consumer choice and may impede incentives to earn income" (p. 327). A fuller evaluation would involve a comparison between income and sales taxes on these and other grounds, which is after all the significant current issue.

Walker makes the statement, "Economic growth is an increase in the nation's output, which occurs both because the ability to produce rises and because this added ability is employed" (p. 2). Some economists also emphasize definitions which run in terms of output per capita (e.g., Stigler in the American Bankers Association *Symposium on Economic Growth*) and of productive efficiency rather than the aggregate output (e.g., *Federal Reserve Bulletin*, August, 1963). Walker makes an important point when he says, "It makes no sense to say or imply that the theory is right and the facts are wrong" (p. 23). One is reminded of the definition of a tragedy as a theory

killed by a fact. The statement, "Merely standing pat is a policy" (p. 191), is a useful reminder to those who think they are doing nothing by doing nothing.

The clear and simple style of the book is well illustrated in the section on Harrod's theory of economic growth. Before the reader is aware of it, he has been taken through the complex analysis and has obtained an understanding of the major concepts without having first been required to master a catalog of technical terms. On this subject and on statistical developments of the last decade the book makes a useful expository contribution. The book is intensive and thorough without attempting to be encyclopaedic.

T. F. Dernburg and D. M. McDougall present a particularly good merger of cycle and growth analysis. There is more reliance on mathematics than in the Walker book but the more frightening parts are relegated to footnotes and appendices in this edition. The major changes from the first edition are a new chapter on the relation between static and dynamic analysis; a new chapter on "demand-pull" and "cost-push" inflation and structural unemployment; and substantial revision and up-dating of several other chapters. The chapter entitled "Financing Government Expenditures," which surprisingly deals only with public debt, contains a thorough discussion of debt burden and debt management.

The book reminds us appropriately that the gross national product should not be confused with welfare: "To appreciate this point, one has but to consider that in Elysium, where there is no economic activity, gross national product is zero" (p. 45). Under existing statutes, regulations, and interpretations, moreover, the income tax would also be zero. (The income tax form contains no line on which we have to declare our psychic income—as yet.) As Elysium becomes crowded, however, the capital gains tax will undoubtedly make its appearance.

Dernburg and McDougall point out that as a result of statistical studies using American data, "many economists no longer feel that an income-leveling policy will significantly help to raise total consumption" (p. 77). We may add that a logical consequence of this may be that an income-disleveling policy would also not help significantly to reduce total consumption. This has important implications for a greater reliance on sales taxation in the federal tax system.

They state, "Not to be in favor of economic growth is as heinous a crime in our society as is hostility to dogs and little children. But is it desirable to force-feed economic growth, as many are now urging?" (p. 217). This query may be used to provoke lively discussion in the classroom. In an incentive-based economy, the provision of incentives can hardly be called "force-feeding." And as long as there is a large volume of human resources going to waste through unemployment, the provision of such incentives is amply justified. We should not tolerate unemployment merely because we fear the consequences of full employment.

The impact that "credit card living" has had or should have on the analysis of the demand for money is called to mind by the authors' tale of the

salesman planning a business trip from Chicago to New Orleans. Discussing the precautionary motive for holding cash, they tell us that the salesman "will require a certain amount of cash to pay for travel expenses. However, if he is prudent, he will take along more money than the average amount he usually requires to make transactions during such a trip" (p. 105). That was yesterday. The prudent salesman nowadays simply takes along a pocketful of credit cards in total disregard of the impact of his behavior on traditional (i.e., Keynesian!) monetary and interest theory.

If a textbook should present conflicting points of view without attempting to convince the reader of the validity of one and the invalidity of the other, there are several instances in which this book qualifies. After reading the following paragraph, one is in doubt whether or not to become upset by mild inflation:

We are not among those who feel it necessary to cry havoc each time the price level rises by a small amount. Neither are we comfortable about the "relax and enjoy it" attitude of some economists. There is always the danger that mild inflation may gradually snowball into hyperinflation. Because a steady, though minute, rise in the price level may create such expectations that people will behave in a way that makes further increases inevitable, it would be folly to take a flippant attitude toward mild inflation (p. 222).

One is also left up in the air when, after discussing expansion of investment-goods production via forced savings, the authors say, "But whereas the banks are the villains of the piece to Hayek, to Schumpeter they are the indispensable instruments of capitalist progress since it is they who make resources available to the innovating entrepreneur. Apparently, it all depends on the point of view" (p. 243). With conclusions like this, students are likely to go through life not knowing whether bankers are good guys or bad guys!

There is no fence-sitting in the concluding chapter, however, in which some very strong assertions are made about cost-push inflation and structural unemployment. The discussion there is controversial, courageous, and stimulating. The same may be said of the analysis of the relation between the level of activity and budget deficits in the chapter on fiscal policy. It provides a basis for understanding the futility of trying to balance the budget simply by balancing the budget.

Both books rely on the so-called "balanced budget theorem" to determine the effects of a balanced budget. Dernburg and McDougall reach the conclusion that a balanced budget is expansionary, and that the amount of the expansionary effect is measured by the size of the budget (pp. 87-88). Walker's discussion of equal increase in government purchases and taxes is technically correct in the limited model used (pp. 191-94), but the natural tendency of a reader to assume that the author is talking about effects of a budget that includes all government expenditures (not just purchases) and all government revenues (not just taxes) is not forestalled. The author states, "An equal increase in purchases and net taxes is not neutral but has an expansive effect, even though the government budget remains balanced" (p. 194; and

see p. 215). Walker does recognize one limiting case where, "Fiscal policy cannot increase aggregate demand and output, because each dollar borrowed or taxed by government reduces private spending in the same measure. (This is the one case where the balanced budget multiplier effect does not work)" (p. 269).

The authors of these books fail to warn the reader adequately that the "balanced budget theorem" neglects liquidity effects. In a tight capital market the restrictive impact of the taxes might partly or wholly offset the expansive impact of the purchases; and in a loose capital market, if total budgetary expenditures (including appropriations to lending agencies) are balanced by revenues, there could even be a net restrictive impact of the balanced budget as a whole. Thus a balanced budget might be expansive, restrictive or neutral in its effect on GNP, not solely expansive. The authors should not be pilloried, however, for repeating a widely propagated fallacy.

Both books can be recommended highly as textbooks in courses on macroeconomics. Either can also be used as serious reading by economists of an earlier generation who received their training before the trend to growth.

HAROLD M. SOMERS

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*The Theory of Consumer Demand: A Critical Appraisal.* By GEOFFREY P. E. CLARKSON. Englewood Cliffs, N.J.: Prentice-Hall, 1963. Pp. 152. Text, \$4.95.

Geoffrey Clarkson has written a tightly reasoned methodological polemic critical of traditional microeconomic theory. It is a book well worth reading because it neatly states the logical empiricist criticism of analytical economics. It is polemical because Clarkson accepts with little qualification the now highly controversial notion that logical theory presents the paradigm of science. Once having accepted this logical empiricist position, that all *scientific* knowledge can be built from simple atomic propositions about observables extended by deductive rules of inference, Clarkson can declare that the postulates of modern demand theory have little empirical foundation and that "theories of economics have failed to meet the standards governing empirical science."

Clarkson has ingeniously applied principles of inference well known to modern philosophers and logicians to the theory of demand. A large portion of this concise book is devoted to a lucid exposition of these familiar principles. The author makes the usual distinction between deductive rules built from axioms that have no empirical content and empirical matters to which they are applied. Scientific knowledge, then, is a system of deduced hypotheses containing mainly observational terms and capable of refutation by empirical test.

Traditional demand theory, according to Clarkson, cannot fulfill this condition of refutation. Demand theory is founded in the theory of utility, a concept incapable of being formulated so that it can be subjected to empirical test. Expressed formally in "indifference analysis" or even in the "theory

of revealed preference," the utility function cannot be identified empirically. Why is it void of empirical content? Because, according to Clarkson, *any* behavior by a choosing consumer can express his "utility function" and cannot be refuted by observables. True, utility could be made empirical if real definitions and operational interpretations were given to the basic postulates of utility (i.e. that a consumer knows his preferences, that he is consistent over any designated time, that his preferences are transitive). But in practice this is not achievable because we can never know how maximal is any particular selection or what relationship holds between a chosen set, the state of information, and the universe of possible choice. Since demand theory is derived from utility theory, it too has no empirical content, nor can it readily be made empirical unless it were possible to treat "the world as though one thing varied while everything else remained constant." Demand theory, Clarkson would argue, must have content, since it is a "theory about the behavior of consumers in the market place . . . [explaining] . . . the process by which consumers make choices among alternative commodities available to them at any one point in time." But the traditional theory merely serves as a set of theoretical concepts, which, lacking the support of empirical test, provide only "an uninterpreted theoretical structure incapable of producing explanations and predictions of economic events" and yielding mainly after-the-fact rationalizations of why an event has occurred. The economist's methodology of constructing theory from empirically untested axioms has made a large body of microeconomic theory incapable of refutation by empirical test and inapplicable to the explanation and prediction of consumer behavior.

Wherein lies the remedy? According to Clarkson, it consists in the establishment of general laws of demand based upon empirically confirmed premises. These postulates would be derived from the "direct observation of human decision-making behavior," and linked to empirical evidence provided by psychology and other behavioral sciences. Traditional theory of demand must be reduced to a more microscopic set of data and postulates, a theory of individual decision-making behavior. This theory must be founded in concrete observation, in established empirical laws, in recent researches in behavioral theory, in the simulation of individual decision-making. In short, what Clarkson proposes is that the theory of human problem solving should provide the foundations for a theory of demand and, by implication, for all of microeconomic theory.

But Clarkson's preference for a science based on only descriptively rich empirical statements is not shared by everyone. The simple classifications in his analysis may even prove misleading. The "utility function" may be neither vacuous nor verified; there are mixed entities in science with some empirical content and logical form. Some postulates determine scope, and are not to be analyzed by the calculus of propositions. Nor can one agree that the theories of utility and of demand are merely concerned with the description of consumer behavior; they are part of the total machinery of economic theory which generates propositions about welfare, trade, growth, *et al.*

The concepts of a science are highly complex and frequently not amenable to the simple judgments of "empirical" or "vacuous." Might not the reduction of economics to the direct observation of human behavior result in the creation of an infinite series of particularized descriptions rather than the general theory Clarkson hopes direct observation would introduce?

Clarkson's book has the unusual virtue of presenting a strong and well-reasoned argument for a limited point of view. In economics, he has certainly raised the level of the logical empiricist demand for strong verification principles and descriptively accurate postulates. Clarkson has written a book which ought to be read by everyone interested in the methodology of economics. It is not that his ideas are particularly original, but he has presented the empiricist position in the modern mode and in a way that is both clear and stimulating.

SHERMAN KRUPP

*Lehigh University*

**Price and Allocation Theory; Income and Employment Theory;  
Related Empirical Studies; History of Economic Thought**

*A Behavioral Theory of the Firm.* By RICHARD M. CYERT AND JAMES G. MARCH, with contributions by G. P. E. Clarkson, K. J. Cohen, W. R. Dill, E. A. Feigenbaum, E. Grunberg, C. G. Moore, P. O. Soelberg, W. H. Starbuck and O. E. Williamson. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. xii, 332. \$8.25.

This book delivers a major blow to that battered but hitherto unshaken intellectual construct, the theory of the profit-maximizing firm. Its importance derives from the fact that it presents a well-elaborated alternative theory that stands up well under the tests of both systematic and casual empiricism, rather than from any novelty in the criticisms it levels against orthodoxy. Indeed, the interpretation of the book as a challenge to orthodoxy is mainly the reviewer's rather than the authors'. The latter are, if anything, overly willing to accept the alibi that the received theory was never intended to be a theory of the how or the why of business decision-making, but only of the behavior of markets. The legitimacy of this alibi has been a matter of some dispute, but the authors forestall any corresponding disagreement about their own objectives. They are interested in developing a positive theory of the economic behavior of the individual business firm. If success in that endeavor contributes to success in predicting more macroscopic phenomena, or if it advances the normative theory of the firm or of the whole economy, that will be an instance of external economies.

The most distinctive feature of the authors' research strategy is their fundamental assumption that the positive theory of the individual firm must be *process oriented*, i.e., the theory must be a representation of the essentials of the process by which firms make decisions. Organization theory in the tradition of Barnard and Simon (and March) provides the set of concepts and some of the hypotheses that structure their view of the decision process. The elaboration of those concepts and hypotheses in the specific context of busi-

ness decision-making seems to render them a good deal more concrete (and perhaps more interesting to most economists) than in some earlier formulations.

The main elements in this view of decision processes in the firm may be summarized as follows. First, the firm is a coalition in which goal conflicts among the participants are never permanently resolved. The effective goals of the firm in any given situation are governed by the bargaining process which fixes the composition and general terms of the coalition, by the control procedures of the firm, and by the effects of experience on the aspirations and attention focus of the participants. The bargaining process at a given time is, of course, severely constrained by the precedents and policy commitments established at earlier times. Second, firm behavior is goal-seeking, not in the sense that an attempt is made to maximize some over-all utility function for the organization, but in the sense that an attempt is made to find behavior that will satisfy a number of independent aspiration level constraints. That is, *satisficing* behavior in a number of utility dimensions replaces maximizing behavior in a single utility dimension. Third, *search activity* is triggered by the appearance of a particular policy problem (failure to achieve goals), and its direction is much influenced by the symptoms and organizational locus of the problem. Search is generally limited to the "neighborhood of the current alternative," i.e., policy change in the firm is incremental. In the absence of a problem, there will be little or no search for alternatives to existing policies. Fourth, the firm reacts to uncertainty by employing *standardized decision rules* that emphasize reaction to short-run feedback rather than pursuit of long-run strategies, and by adhering to "good business practice" (a sort of implicit collusion with competitors in the interests of uncertainty avoidance). Lastly, decision rules, aspiration levels, and the rules of the search process itself are subject to adaptive change through time: Search leads to modifications in decision rules; aspiration levels adjust upward or downward according as search is rewarding or not and achievement exceeds or falls short of aspiration; search tends to take the forms and directions which have yielded success in the past.

A wide range of evidence is presented in support of this view of decision-making in the firm. Four case studies of business decisions (Chapter 4) amply document the limitations of the conventional theory of the firm as a theory of decision processes within the firm, and at the same time lend support to the hypotheses advanced by the authors. The studies illustrate, for example, the prevalence of feasibility tests of proposed actions rather than optimality tests, and the fact that agreement on firm policy among the subunits in the firm may be the result of disagreement both about goals and about the consequences of the policy.

The most impressive evidence of predictive power is undoubtedly that contained in two computer simulation studies of particular business decision problems. The first of these is a model of pricing decisions in a department store, developed by the authors and C. G. Moore (Chapter 7). The flow chart for the "mark-down pricing" portion of this computer model contains

boxes with injunctions like "Reduce price by one-half, carry down to nearest 0.85," but none of the boxes contains any reference to anything resembling the concept of elasticity of demand. This mark-down pricing program predicted, to the penny, the mark-down prices of 140 out of 159 items in a random sample of mark-down decisions made by the particular department investigated. The second computer model is a simulation of trust investment behavior developed by G. P. E. Clarkson. The data employed were derived from observation of the behavior of a particular investment trust officer in a particular bank. Again, there is little resemblance between the decision-process model and anything that would ordinarily be thought of as maximizing behavior. For example, portfolio diversification is accomplished according to the rule of thumb that all accounts must participate in at least five industries, and participation in stocks is usually limited to one per industry. The model was tested by using it to predict the trust officer's choices for four accounts not used in developing the model. The model and the trust officer agreed on the number of stocks for each account; they agreed on the choice of companies in 24 out of 29 cases, and on the size of the participation in 22 out of the 24. In short, trust officers appear to be threatened by automation.

These models provide striking support for the view that business decisions are dominated by rules of thumb and standard operating procedures—at least in the short run. They also indicate that, with the help of an electronic computer, a process-oriented theory of firm behavior can be made to yield specific and highly accurate quantitative predictions—at least if the decision process investigated is sufficiently simple and the data required for estimation of the parameters of the decision rules are available. However, the qualifications appended to the foregoing statements are important ones if the theoretical framework constructed in the early chapters is indicative of the aspirations held for the behavioral theory of the firm. The fact is that only a small portion of the theory developed in the early chapters is subjected to test by these computer simulations, except in the limited sense that the theory predicts that fairly simple rule-of-thumb decision procedures will govern behavior in the short run. For example, the performance of the models does not provide any evidence that it is useful to view the firm as a coalition. Neither do these models throw any light on the sorts of search processes that might be triggered by an important failure to achieve goals for the firm as a whole. As for the adequacy of computer simulation as the chief research tool of the behavioral theory, the evidence in the book is as suggestive of the limitations of computer techniques as of their promise. While the department store and trust-investment models are clear successes, the attempts to simulate the behavior of large oligopolistic firms (Chapters 5 and 8) appear to be in grave danger of getting bogged down in the problems of estimating a large number of parameters and devising appropriate statistical tests of simulation models when the agreement between predicted and observed behavior is not overwhelmingly persuasive.

Although the emphasis on process-oriented models with computer simu-

lation as the implementing tool is the dominant one in the book, a much different approach to the behavioral theory of the firm is also represented. In Chapter 9, O. E. Williamson proposes a model of "rational managerial behavior" that, in terms of method, is much closer to orthodox theory than it is to the decision-process analysis in the rest of the book. His procedure is to introduce a managerial utility function to replace profit as the maximand in the theory. The link to the concepts employed elsewhere in the book is provided by the choice of arguments for the utility function. These are (1) spending on "staff," including managerial salaries; (2) the difference between actual and reported profits; and (3) "discretionary spending for investment," equal to reported profits less taxes less the minimum profits required to satisfy the demands of other members of the coalition (e.g., dividend demands of stockholders). Comparative statical analysis of the maximum position yields various interesting predictions; for example, an increase in the tax rate on profits will increase output and "staff" and decrease the fraction of profits reported. The predictions of the model approach those of traditional theory when the environment of the firm is sufficiently severe.

The enormous gap in technique between Williamson's work and the computer simulations suggests the existence of a correspondingly large range of as yet uninvestigated possibilities for elaborating and testing the behavioral theory of the firm. Hopefully, the continuing search for testable implications of the theory will not be confined too strictly to the "neighborhood of the current alternatives." Also, it is to be hoped that someone will eventually accept the challenge of attempting to provide a better definition of the relationship between the behavioral theory and traditional theory than is provided by the assertion that the two theories are concerned with different problems. It is, after all, a simple matter to construct hypothetical situations in which some simple version of the behavioral theory yields predictions about *market* phenomena that differ from those of the usual perfect information version of the traditional theory. (In fact, Williamson's model provides examples.) On the other hand, the consistency of the behavioral theory with the more persuasive portion of the *empirical evidence* for the traditional theory has yet to be determined. Investigation of the relationship between the two theories will probably involve closer attention to the circumstances that determine when the profit goal is evoked and when profit aspirations adjust upward, as well as to the ways in which competition may force an approach to profit maximization by firms whose decision processes are governed in the short run by crude rule-of-thumb decision rules. If the reconciliation can be effected, the resulting theory will have to be regarded as the general theory of firm behavior and the assumption of profit maximization will have to be regarded as an approximation that is useful in a limited class of problems.

For the past several years, the research on the theory of the firm conducted at Carnegie Institute of Technology has been distinctive for the boldness of its departure from the accepted modes of economic thought on the subject. Cyert and March and their collaborators have consolidated much of

that research, and the result will be impressive even to those economists who attended carefully to the fragmentary reports as they came in. Those who have not heard the distant rumblings of the "behavioral revolution" will be surprised at the momentum it has achieved. The final verdict cannot be predicted, but this book should at least convince most economists that the revolutionaries bear watching.

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*Capital and Rates of Return in Manufacturing Industries.* By GEORGE J. STIGLER. Princeton: Princeton University Press for the National Bureau of Economic Research, 1963. Pp. xiii, 229. \$5.00.

This volume has two major purposes. The first is to describe and present comprehensive new annual estimates of capital and rates of return in 3-digit manufacturing industries from 1938 to 1957. The second is to analyze these and other data to determine what insights the economist can gain from them. Since this reviewer is not competent to pass detailed judgment on a data-collection task of this sort, attention will be concentrated on the three analytical essays that form the heart of the text. My remarks concerning the data will mainly pertain to matters of presentation.

The salient characteristic of George Stigler's capital measure is its comprehensiveness. In addition to the items of fixed capital usually included in studies of firms' investment policies, he includes all items of working capital except investment in other firms. He presents some persuasive arguments for his comprehensive measure, although it is not clear why investments in other firms should be excluded from a measure that includes holdings of cash and government securities. An adequate justification for compiling the new series is that the measures compiled by Creamer, *et al.*<sup>1</sup> are not available on an annual basis. However, the two measures are not the same in years for which both are available. It would have been helpful if Stigler had given the reader a brief reconciliation of the two measures.

The three analytical essays are very stimulating indeed. Their hallmark is the ingeniousness rather than the mathematical or statistical sophistication of the procedures used.

Chapter 3 examines the classical proposition that competition tends to equalize rates of return in different industries. There is a careful discussion of the theoretical relationship between competition and rates of return and of the factors that govern the strength of the tendency. That the strength of the tendency is considerable is suggested by a number of observations on the data, including the observation that the relative dispersion of investment rates is much greater than the relative dispersion of rates of return. Stigler observes almost no differences between average rates of return in concen-

<sup>1</sup> Creamer, Dobrovolsky, and Borenstein, *Capital in Manufacturing and Mining: Its Formation and Financing*, Princeton University Press for National Bureau of Economic Research, 1960.

trated and unconcentrated industries, but substantially greater persistence through time in the rates of return in the former than in the latter group.

Chapter 4 analyzes the determinants of the rate of investment. Using data from the postwar part of his sample, Stigler concludes that changes in sales receipts are much more important than profit rates in explaining investment in book values. When deflated values are used, the roles of the two explanatory variables are interchanged, since the high correlation between book values of investment and changes in receipts is largely accounted for by the similar effect of price changes on each.

The final chapter of the book is concerned with substitution between capital and labor. The usual way to estimate the elasticity of substitution is to compare differences in the capital-labor (or output-labor) ratio at different points in space with corresponding differences in wage rates (assuming, as Stigler also does, that capital costs are the same for each observation). Stigler's procedure is to make the comparison between large and small firms rather than between different points in space. He observes that earnings per worker are usually higher in large than in small firms, and his estimates yield much higher elasticities of substitution (averaging about four) than do other estimates. But why are earnings higher in large firms? Presumably, either large firms employ higher quality labor or they have some monopsony power. The former is excluded by assumption (in other similar studies as well as in Stigler's) and, if true, would invalidate the estimates. But the latter also invalidates the estimates. If large firms have some monopsony power, it means that their factor proportions are in response to a higher marginal labor cost than is indicated by the wage rate. Therefore, the large excess of their capital-labor ratios over that for small firms is partly accounted for by the understatement of their marginal labor costs. If this line of reasoning is correct, it means that Stigler's estimates of the elasticity of substitution are too high.

This is a stimulating book and contains many suggestions and hints for further work. Some of them can be followed up by others using the data that Stigler has provided for us.

EDWIN S. MILLS

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*The Evolution of Economic Thought.* By W. E. KUHN. Cincinnati: South-Western Publishing Co., 1963. Pp. x, 451.

In the last four years at least five histories of economic ideas (Blaug, Leachman, Oser, Whittaker, and Taylor) have preceded W. E. Kuhn's entry. Nevertheless, Kuhn has succeeded in differentiating his product substantially from those of his most recent competitors. For one thing his organization is designed to parallel that of the standard introductory texts. Almost the entire first half of the volume is devoted to the development of value and distribution doctrine. A single chapter on macroeconomics, here regarded almost exclusively as the contents of Keynes' *General Theory*, follows. The final 200 pages rapidly survey monetary and banking theory, international trade, business cycle theory, public finance, and the "isms." Then, too, Kuhn segregates

biographical and historical information in a series of pounds at the end of each chapter. Close to a quarter of the text is given over to these appendices and to the questions and reading lists also appended to each chapter. Finally, Kuhn distinguishes himself from his rivals in the degree of his partisanship—to his credit, openly avowed. In Kuhn's opinion scientific economics is essentially value and distribution theory. Among value and distribution theorists, "the Austrians added more to the body of pure economic theory than any other 'school'." Accordingly the text spends 36 pages on Menger, von Wieser, and Böhm-Bawerk. The corollary of this intellectual judgment is the author's depreciation of macroeconomics which leads to the peculiar treatment of Henry Hazlitt as a major critic of Keynes, and the comparative neglect of such respected Keynesian opponents as Robertson, Viner, and Haberler.

Is the result of this organization and these preferences a usable text? On the whole the first 200 pages are good, subject to some major reservations which will be noted. Chapters 3-7 which trace value and distribution from Cournot through Robinson and Chamberlin contain a closely reasoned account of obvious benefit to serious students. But this section is seriously weakened by the opening two chapters which cover the English classical school. This is one of the places where the author's special slant creates a special difficulty. By interpreting Smith and Ricardo primarily as theorists of value and distribution, Kuhn completely misses the dynamic core of the Ricardian theory. He emphasizes this lapse by referring to Malthusian population doctrine only in his biographical appendix. Hence he loses the chance to display to his readers the model of economic change which in Ricardo rests so substantially on Malthusian population assumptions. It will be hard indeed for students to avoid the conclusion that the classicists were uninterested in economic change and economic growth.

Kuhn's handling of Marxist economics is equally questionable. Marx receives some five and one-half pages of attention in the very last chapter devoted to the "isms," separated by some hundreds of pages from the English classical economists from whom he borrowed. Kuhn ignores Marx's interesting business anticipations and models. The historical school is brushed aside with three pages. Veblen earns only biographical mention and Commons little more than that. The "associationist" socialists and the radical Ricardians are nowhere to be found.

Most dubious of all is Kuhn's handling of Keynes. Although in fairness it must be granted that Kuhn does intelligently summarize some aspects of the *General Theory*, the chapter on Keynes suffers from serious faults. The comments on page 207 can scarcely fail to leave students with the impression that if Keynes had lived he would now be opposing fiscal policy and the welfare state. It is a revelation of Kuhn's state of mind that he refers to the *General Theory* as "Keynes' treatise on value theory" (p. 208). Worse still, Kuhn says nothing of Keynes' antecedents in Swedish and Robertsonian theory. His account of the controversies which followed the publication of the *General Theory* is brief to the point of distortion. Although Kuhn disclaims any "attempt . . . to arrive at a final evaluation of Keynes' contribution to the body

of macroeconomics and his inspiration of varied econometric research," he consigns Keynes to a minor place in the history of his subject by implication, omission; and emphasis.

The chapters on specialized topics which round out the volume too frequently deteriorate into a catalogue of names and contributions to permit easy use. A random check of the index suggests the omission of a large number of names and references.

In sum, despite some quite good sections and a vigorous point of view, Kuhn ought to be used as a text only with very considerable caution.

ROBERT LEKACHMAN

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*Antología del pensamiento económico-social.* Edited by JESÚS SILVA HERZOG. Mexico City: Fondo de Cultura Económica, 1963. Pp. 606. 70 pesos.

The publishers announce that this volume, which covers economic and social thought from Jean Bodin to Louis Blanc, is to be the first in a set of three. Whether or not that ambitious project is ever completed, the present book fills a large gap in Spanish textbooks. The principal works in economics published after 1850 are all available in Spanish; it was always the earlier period that gave trouble. Because of the structure of Latin American schools of economics, this anthology will rather quickly be adopted by most of them for use in their standard required course in the history of economic doctrines. It will, therefore, acquire an importance which no similar anthology published in the United States could achieve.

The work is clearly identified as a textbook, with no particular pretensions to scholarship. Several of the translations are even at one remove: those of Serra, von Hornick, von Justi, and Galiani, all of which are Spanish translations of the English translations found in Monroe's *Early Economic Thought*. Ever since the establishment of the Library of Congress' microfilm service, that sort of thing has not really been necessary. Also, the text is marred by endless typographical errors: Adam Smith's famous pension is quoted at £800; the U.S. Constitutional Convention is said to have met in 1887; the date of Babeuf's birth is given as 1706; and the like. These and hundreds of similar errors will only annoy economists but they may well mislead students.

As a textbook, an anthology of this kind has long been needed. Up to now the collections available in Spanish have either been monumental and relatively inaccessible, like Sempere y Guarinos' *Biblioteca española económico-política*, or sketchy and out of print, like Silva Herzog's own 1939 anthology. Now, building on his forty years of teaching the history of economic thought in various Mexican universities and on his earlier publications, the editor has put together a comprehensive and useful anthology. All of the standard names are present, grouped into mercantilists, transitional economists, physiocrats, classical economists, and social critics and Utopian socialists. But a number of surprises await the reader: selections from the works of Juan de Mariana, Juan Botero, Jerónimo de Uztáriz, Bernardo Ward, the Conde de Campomanes, Gaspar Melchor de Jovellanos, Álvaro Flórez

Estrada, Morelly, and Babeuf. The availability of these selections should add a note of color to future seminars.

Some of the selections made from standard authors seem a bit odd. The inclusion of such writers as Locke and Rousseau is well within the editor's self-imposed limit, but certain other extracts are dubious. For example, only the first third of Turgot's *Réflexions* is reproduced, the third in which he merely recapitulates the system of Cantillon and Quesnay before settling down to present his own contributions. The specimen from Campomanes is a letter to the Conde de Lerena in which he castigates the internal revenue in an unexceptional fashion, rather than, for example, the *Discurso sobre el fomento de la industria popular*, which so aroused McCulloch's admiration. One of the selections from Quesnay is, inevitably, the *Tableau économique*, but only the verbal exposition. The *tableau* itself is omitted, a fact which will very likely make the whole exposition unintelligible to students. And the essay chosen to represent Hume is the pedestrian one on commerce rather than the more analytical ones on interest, balance of trade, or money. But these points should not obscure the fact that the collection as a whole is first-rate: all of the necessary names are included; and, in the main, the selections are representative.

Each selection is preceded by a biographical and critical note. Instead of the brief presentations of dates and facts which are typical of United States anthologies, Silva Herzog presents the reader with extensive comments and criticisms, going on for as much as six pages at a time. The opinions set forth are such as to demonstrate the editor's *bonae fides* as a sound Latin American economist. They may not have the same effect north of the Bravo, as a few quotations might show:

It is frequently said that there never were any economists in Spain. That opinion is the result of a lack of knowledge on the part of persons who hold that opinion. All of the sciences flourished in Spain from the end of the Middle Ages and the science of Economics from the beginning of the modern period. What happened is that, since the time that Spain stopped being a first-class political and military power, she did not have and has not had the necessary might to spread the ideas of her thinkers throughout the world, as France, England, and lately the United States have had (p. 210).

I think that nowadays we know full well that the accumulation of wealth—especially great wealth—is never due to personal merit and even less to personal effort. It is due to the productive use of the energy of many persons for the benefit of one person, to the exploitation of man by man, to unbridled speculation, or to a variety of obscure, turbid, and unmentionable practices (p. 274).

Every person of even moderate education knows very well that the rich countries have exploited and are exploiting the poor countries. They know very well that the rich countries fix the prices of the raw materials which they buy from poor, underdeveloped countries, at the same time that they fix the prices of the manufactured goods which they sell. If that were not the case, then the rich countries would not be as rich as they are, nor the poor countries as poor (p. 458).

These opinions will probably pass unnoticed north of the border, where hardly anyone bothers to read editors' notes. They will be considered unexceptional south of the Bravo, where the students will study them carefully. Similarly, Silva Herzog's preliminary section, a 70-page summary of economic history from classical antiquity to the nineteenth century, will be ignored as supererogatory in the north, but read with interest to the south. The book will provide a useful addition to libraries of economics and will serve as a required text in most schools of economics in Latin America. All in all, it stands as the first substantial anthology of the history of economic thought available in Spanish.

I. H. OTTO

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### **Economic History; Economic Development; National Economies**

*Italy—A Study in Economic Development.* By VERA LUTZ. New York: Oxford University Press for Royal Institute of International Affairs, 1962. Pp. ix, 342. \$7.20.

In recent years, the Italian economy has begun to attract the keen interest of a growing number of outside specialists, well beyond that nation's comparative economic importance as a producer and trader. There is good reason for this.

From its inception as a national economy, Italy has developed in a curiously uneven and distorted fashion. After World War II, this gained recognition as a strong case of economic dualism, with burdensome consequences that remain large today. The postwar years were also marred by massive overt unemployment and even worse underemployment. Yet this was the very time when the great boom burst forth, giving Italy one of the highest growth rates in the world. Between 1948 and 1961, this impressive upsurge went on under quite orthodox policies toward money, convertibility, and foreign trade—in which a high savings rate, rather than a high spending multiplier, was deliberately sought. Moreover, these policies were coupled to a massive program of incentive planning for the depressed South, intended to evoke autonomous growth there by creating a variety of external economies through subsidies and public investment.

Obviously, the Italian story has needed telling, but even more, careful technical analysis. From her sustained study of the case for some 15 years, more recently as a research expert for one of the big government banks, Dr. Vera Lutz has made a substantial contribution in both respects, one that brings together and amplifies her valuable pioneering papers in this field. Considering the frustrations and sheer bafflement implicit to any outsider's use of the Italian materials, or to understanding the maze that is the Italian economy, the skill with which she cuts through to generally the right conclusions clearly puts the profession in her debt. The result is a work of much value alike to students of development, international economics, labor economics, monetary policy, and other fields as well. One could hardly find a

better demonstration of the value of undertaking factual studies after an initial mastery of economic theory itself.

The central theme of the book is that Italy was a dual economy throughout 80 years of industrial development, and remains one today—a “half-and-half system” whose dichotomies are not geographic alone, but run through particular industries and localities and even entire sectors. The North-South bifurcation is but the most obvious expression of the phenomenon. These divisions are found in techniques of production, forms of enterprise and employment, and, above all, in comparative productivities and incomes. Their explanation is complex. In oversimplified terms, it centers upon a secular shortage of capital to carry through transformation to a unified system, and a collection of misguided policies toward tariffs, wages, labor mobility, taxation, and public expenditure. The major consequence has been that the total income produced by the system for years has fallen well short of potential. No less important, particularly in the postwar years, it has enforced chronically heavy unemployment and underemployment. As Dr. Lutz herself puts it, a “common market” for labor never really emerged in Italy, and only now is beginning to appear.

The first part of her book poses her view of the Italian problem, presented through a model of a dual growth process in which the labor market plays the strategic role. Next she considers the North-South split: its nature, origins, remedies in course, and issues for policy. Taking note of the failure of the per capita income gap to close despite all efforts since 1950, she concludes that accelerated emigration north, rather than forced-draft industrialization in the South, would achieve “approximate parity” in much shorter time, and with less impairment of over-all income growth. This does not mean abandonment of the South, as its Neo-Mercantilist spokesmen have been claiming in parliamentary debates, but a shift of emphasis. In my judgment, her diagnosis is basically right, and it took a good deal of courage to voice it.

Part III then moves into a variety of special topics—agriculture, wages, the labor market, taxation, state enterprise, foreign trade, and monetary policy. All are tied together by reference to the common theme of dualism. One will find here much valuable statistical and descriptive material, all relevant to the central analysis.

In view of the distressing reversal of Italy's economic fortunes since late 1961, it is regrettable that the book mainly terminates with 1959, the more so because this was the first year in which registered unemployment finally began to show significant declines. Also disappointing is a certain lack of firmness regarding current issues, and a failure to explore more fully some highly important topics—for examples, the monetary reform of 1947, the outcome of Einaudi's monetary policy, the true extent of overt unemployment since 1952, and the main choices for policy that have been evident from the start of the 'sixties. But I do not wish to cavil. This is still the best study of the Italian economy ever to appear in English.

GEORGE H. HILDEBRAND

*Cornell University*

*The Dilemma of Mexico's Development: The Roles of the Private and Public Sectors.* By RAYMOND VERNON. Cambridge: Harvard University Press, 1963. Pp. xi, 226. \$4.95.

In this book Professor Vernon offers a perceptive, provocative prognosis of Mexico's development problems, particularly of the problem of maintaining the economic momentum which Mexico has achieved during the period since 1940. The prognosis is based on close historical study, and, indeed, one of the most remarkable attributes of the book is the clear, succinct manner in which Vernon has been able to weave together the various threads comprising the fabric of Mexican economic history. He largely eschews statistical and other conventional techniques of the economic historian and focuses instead upon interrelationships between economic and political forces.

In the first pages of the book Vernon leads his readers to expect a treatise dealing with the relative roles of the public and private sectors in the process of Mexican economic development. However, he very soon directs attention to the public sector-private sector confrontation in the political arena. This, in turn, leads to an examination of the ailments of the Mexican body politic and it is upon the results of this examination that Vernon bases his central thesis. In Vernon's view "the dilemma of Mexican development" is essentially of a political nature.

While Vernon does not undertake a detailed analysis of the strictly economic forces underlying contemporary Mexican economic difficulties, he does have a good deal to say about the nature of these difficulties. He indicates that there exists a tendency for the stimulus formerly derived from the export sector and from import-substitution policies to diminish. He suggests that there is a widespread belief among Mexican businessmen that the prospective future growth of the domestic market does not warrant expansion of productive capacity at the same rate as in the past. He argues that expansion of domestically financed public investment is circumscribed, on the one hand, by limitations on the government's ability to tax and operate public enterprises profitably and, on the other hand, by undue governmental reluctance to employ deficit financing. Neither does he foresee any great expansion of public investment supported by foreign borrowing nor any significant improvement in the quality or growth-producing stimulus of public investment—in the first instance because of the country's limited debt-servicing capacity and in the second instance because of the extent to which public investment decisions are influenced by political considerations. And finally he believes that, for political reasons, the government is unlikely to adopt policies consistent with a sharply higher rate of either domestic or foreign private investment.

Thus Vernon's prognosis is very pessimistic and indicates that Mexico presently is threatened with economic stagnation. In his own words:

In the early 1960's . . . Mexico's economy seemed to be approaching a new series of roadblocks. The country's growth appeared to be slowing a little and the prospects for some new impetus to growth seemed uncertain. If the economy were allowed to drift very long, the loss of momentum might expose the country to serious political risks and

economic losses. On the other hand, practically every major change in policy that offered some promise of stimulating the country's development also seemed to involve a considerable measure of political risk. The problem for Mexico's leaders was to find a way out of the dilemma—to seize the horn on which the country was less likely to be impaled (p. 176).

It seems clear, at least to this reviewer, that Vernon does not expect those who wield power within the Mexican political system to take the aggressive actions necessary to forestall the economic stagnation which he believes to be imminent.

Publication of Vernon's book has been anxiously awaited in Mexico not only because of anticipation that he would probe into politically sensitive matters (and just at the time activities surrounding the choice of the next president were at their height) but also because of Mexican awareness of Vernon's influence in U.S. government circles. While it is too early to gauge the over-all Mexican reaction, it must be obvious to anyone who has had an opportunity to discuss the book with Mexican readers that many are irritated by it. There is, first of all, resentment that "a foreign doctor" (as Vernon has been publicly referred to), whose knowledge of the patient's intimate circumstances is of recent origin, would presume to offer an uncomplimentary prognosis. Some Mexicans are also irritated by what they sense to be a certain disdain or condescension on Vernon's part as exemplified in his irreverent appraisal of contributions of certain national heroes (President Lopez Mateos is the object of several uncomplimentary remarks) and his somewhat derogatory use of the term "técnicos" (why not "technicians" or "economists"?). There also exists a feeling among his Mexican readers that Vernon's appraisal of the Mexican political system reveals an inability to free himself from ethnocentric, Anglo-Saxon preconceptions.

There are in addition several more substantive criticisms of Vernon's book which challenge the validity of his basic thesis. On the economic side it may be that Vernon has mistaken a cyclical decline in the growth rate for a secular trend. Furthermore, Vernon's argument seems to be premised on an exogenous or shock theory of economic development which leads him to attribute Mexico's past successes too much to fortuitous circumstances and too little to intelligent policies and the inherent, self-sustaining nature of the growth process. On the political side it may well be that Vernon has confused ceremony and function, and that while outwardly the political system may appear to constitute a barrier to further rapid economic development, inwardly the political machinery is capable of producing as decisive and hard-headed economic policies as that of almost any country (and especially, as my Mexican friends have pointed out, as that of the United States). Furthermore, in calling for reform of Mexican political institutions, Vernon perhaps underestimates the advantages of the existing system, given the present stage of the country's social and political, as well as economic, development.

The merits of the political system which has evolved since the Revolution are that it is responsive to public opinion, does permit peaceful changes

of administrations, and at the same time affords wide scope for personal freedom. Centralized decision-making authority vested in a presidency which strives for a high degree of unanimity may seem a peculiar arrangement, but while it imposes certain limitations and involves certain risks it also opens certain opportunities and holds certain other risks to a minimum. It seems clear from a comparison with the record of most other Latin American countries that Mexico's remarkable economic progress in the recent past is attributable in no small measure to the political stability that has been achieved. Furthermore, given reasonably good leadership from the next president and finance minister, Mexico seems more likely to successfully avoid economic stagnation than would be the case if political reforms were to be instituted which, in an effort to establish representative democracy, increased the country's vulnerability to paralysis by powerful legislative bodies or which, in an effort to free the presidency from concern for wide popular support, increased the vulnerability of the government to overthrow by dissident groups.

Whatever the merits of these criticisms, it is clear that Vernon's book will serve a highly useful function in Mexico and elsewhere by stimulating thinking and discussion about these matters. While many of Vernon's critical observations are similar to those occasionally voiced by responsible Mexican intellectuals, it is curious that these same intellectuals seldom are moved to express frankly their concern over problems confronting the country in a form accessible to a wide audience. The fact that this book had to be written by an outsider and the fact that many Mexicans are offended by his unaccustomed frankness reveal an unfortunate deficiency of healthy, objective self-analysis and self-criticism on the part of the Mexicans themselves.

DWIGHT S. BROTHERS

*Rice University*

*Journeys Toward Progress—Studies of Economic Policy-Making in Latin America.* By ALBERT O. HIRSCHMAN. New York: Twentieth-Century Fund, 1963. Pp. xi, 308. \$4.00.

This book is an attempt to push back the frontiers of the subject of development economics so as to incorporate the process of public decision-making in its territory. So far as economic decisions at least are concerned, this is almost virgin land, and the author starts with detailed studies of how three Latin American countries have coped with certain major economic problems—Brazil with its poverty-stricken Northeast, Colombia with its inequitable and inefficient systems of land tenure, Chile with its famous inflation.

The country studies are evidence that, whatever Anglo-Saxons may think, progress really does occur in Latin America, though its path is devious (symbolized, rather enterprisingly, by a reproduction of Paul Klee's "Highways and Byways" as a frontispiece). Setbacks are common; decades pass without any apparent improvement; action when it comes may be a by-product of attempts to solve another problem altogether; it may be the unintended

result of struggle between a number of factions, each of which favors a quite different solution from what finally materializes.

Yet, despite all the apparent chaos in policy formation, patterns are found. In the concluding chapters, the author reaches some descriptive generalizations on the mechanisms of change and uses them to draw normative conclusions on how reform can be achieved without revolution. His conviction that this is usually feasible, even in Latin America, was clearly one force which drove him to pick up his pen; another must have been a compulsion to show the complexity of policy formation. The treatment here of progress as the outcome of the pushing and shoving by many different interests, rather than the creation of a wise policy-maker, reflects his earlier work, which stressed that economic development is generally induced rather than intended. Dislike for sweeping and simultaneous political changes is the counterpart of his professional suspicions of "balanced growth."

The first question that may occur to the reader is: can one detach a really big problem from its national context for individual examination? Each of the three problems he studies is actually part of a total set of interrelated problems, constituting the historic challenge to the country concerned. *All three* countries in fact suffer, though in different degrees, from regional disparities in development, from inappropriate systems of land tenure and from price inflation—as well as from weak commodity markets, inefficient secondary industries, incomplete educational systems, inadequate transport networks, shortages of electric power, etc., etc. It could be argued that the necessarily limited resources of time and money were best spent by the author in studying selected issues of a manageable size; but, considering that a well-known economist is writing, the lack of general economic analyses for each of three countries is rather conspicuous. Even quite limited research would have provided useful background to the story of how each government attempted to deal with the problem chosen for detailed study. It is one thing to say that economic theory needs to take account of political factors, another to leave economics very largely out of the picture.

The paucity of economic analysis is a serious gap in the account of the Chilean inflation, since inflation is after all a phenomenon with many facets. The author of course recognizes this and shows how the theory of "structuralism" emerged as an attempt at a general analysis going beyond a purely monetary explanation, but he does not himself weight the causative influences or trace their interaction. Similarly a study of the problems of the Brazilian Northeast really requires examination of how the system of multiple exchange rates (together with high tariff walls) turned the terms of trade against this region, causing it to fall farther behind São Paulo and the other industrial centers. This criticism can less easily be made of the chapter on Colombia, which does bring out, for example, the role of the foreign exchange shortage in the demand for higher agricultural output; the explanation may be that this is the country with which Hirschman is most familiar.

To raise a more fundamental doubt on his final conclusions, one wonders whether it is not too late by now in Latin America to rely on a consciously

pragmatic, ameliorative approach. Even though it may somehow have worked in the past, this "style" of solving problems is admittedly (perhaps deliberately, if one probes deep enough) slow in producing results. In many countries it may no longer be a realistic description of what actually happens. Military regimes often take action which brings about social reform, but it is the result of running before political hurricanes rather than of tacking to take advantage of every light breeze. As the traditional mechanisms of change atrophy, the patterns of adjustment of policy to relieve growing social pressures seem to be dissolving into a series of sharp and largely unpredictable changes in course.

Even if the past is an adequate guide to the future, the treatment here of the theory of policy-making, complete with algebra and schematic diagrams, seems needlessly ponderous, though fashionable. A Galbraith would doubtless have handled the same material quite differently, and one would hesitate to say that his approach is less appropriate for this field.

Yet Hirschman's central themes are really established beyond question. The ways in which economic policy is formed and institutions adapted are shown to be much messier than is normally believed, or at any rate acknowledged, by economists. Since these changes are part of economic progress, *and yet are not fully determined by it*, the process of development is by implication far too complicated to be fitted into the elegant (or should one say naïve?) fantasies of the model builders.

The country studies also provide additional reason, though from a rather novel point of view, for thinking that economic advisers would be well advised to be either extremely modest or highly sophisticated in their proposals. What Hirschman brings out clearly is that the adaptation of policy to economic requirements consists, if it is made at all, essentially in a process by which those who make or influence policy learn, often unconsciously, from mistakes; political developments impose in time a solution on problems which are not being solved. How many economists realize that the creation and guidance of pressures in favor of a policy are as important as deciding what policy ought to be? Someone in power may even have to adopt a policy which he knows to be wrong, in order to summon up sufficient political support for what ought to be done.

So this is definitely a work to be recommended to economists engaged on development problems. It contains more practical experience than anyone would gain in a decade of work in Latin America and is full of suggestive insights. I was about to add that it is also a useful manual for the reformer—a "progressive" counterpart to Machiavelli's *The Prince*. The reader has the experience (a mildly intoxicating one for the academic!) of being initiated into a number of hard-headed propositions, such as that obscurantism helps to hold together a coalition or that one may have to betray quite consciously one set of colleagues after another (a procedure rather euphemistically described as "shifting alliances").

But here I have my doubts. Perhaps Hirschman's picture of the way society changes is also simplistic; one of the motive forces of progress is pre-

cisely a belief that change will result predictably from a certain political program, and that this will be an easier and quicker process than is in fact at all likely. Both the public and political leaders need a somewhat naïve view of the world if significant advance is to be achieved (just as Columbus had to believe that Asia lay out in the Atlantic if he was to "discover" America). To take away people's illusions may make it quite impossible not merely for these illusions to become reality, but for conditions to be even slightly improved. However, we need not worry lest this book should weaken the forces of change in Latin America. Policy-makers are, from the nature of their calling, too busy to read works on the theory of policy.

DUDLEY SEERS

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<sup>1</sup> This review reflects only the reviewer's personal opinions, which are not necessarily those of the organization for which he works.

*Economic Development and the Use of Energy Resources in Communist China.* By YUAN-LI WU WITH H. C. LING. New York and London: Frederick A. Praeger for Hoover Institute on War, Revolution and Peace, 1963. Pp. xv, 275. \$7.50.

To the increasing flow of literature on Communist China's economy Professor Wu has added this book. However, Wu's study differs from most previous works in that, instead of discussing the general economic situation in that regime or attempting to measure certain aggregate economic indicators such as national income, consumption, and investment, the author investigates one of the vitally important sectors. In fact, this study is probably the most comprehensive survey of the energy industries, chiefly coal and power production, in Communist China in terms of their growth, present situation, and prospects.

This book begins by describing briefly the importance of energy industries for developing countries in general and for Communist China in particular. In the next four chapters the author has assembled, examined, and evaluated the available empirical data concerning production capacity, output, and consumption of power and coal and other related statistics. Although the titles of these chapters refer to the period 1949-60, Wu often extends his data collection back to the 1930's to provide some background information.

Chapter 6 is devoted primarily to the calculation of value-added in the coal and power industries, that is, national income originating from the two industries. In Chapter 7, the author has appraised the successes and failures of the Communist authorities in China in using energy resources and the possible direction of future development.

Just before he summarizes the principal findings and makes some conclusions in Chapter 9, Wu has added a cursory chapter on the petroleum industry, with only 4 ½ pages of textual discussion. One cannot help but feel that the scope of this study would be much closer to the book title if a fuller treatment were given to the petroleum industry. As the author has indicated in Table 59, domestically produced and imported petroleum products com-

bined provided more energy than the power industry in that economy in the late 1950's. The petroleum industry deserves greater attention, especially in view of the possible cut-off of supply of petroleum products from Russia as a result of the Sino-Soviet rift and the drastically increased demand for diesel oil and lubricants in recent years due to the new emphasis on mechanized farming and the greatly increased number of tractors in use.

The reader will easily recognize that discussions of interfuel competition because of changes in the price structure in the fuel market, government regulations and other policy implications, which usually occupy a preponderant space in most research reports on fuel industries in the Western countries, are conspicuously missing in Wu's book. It is largely because of the institutional differences between the two types of economies involved. Wu's book, like most other works on Communist China's economy, is primarily informative in nature.

A great amount of statistical data has been compiled in this study. Part of them are official data gleaned from various Chinese Communist publications while the rest are estimates derived by the author himself on the basis of other relevant information. However, on some occasions it is not clear whether the author has made his own estimates simply because no official data are available or because the official data, though known, are regarded as unacceptable. For instance, in calculating the sectoral breakdown of power consumption in various years, Wu has accepted (p. 78) the industrial power consumption figures for 1952-56 given in *Major Aspects* but has not made use of similar figures given by the same source for 1949-51. Instead, he has taken a very roundabout process (p. 81) to arrive at some estimated figures for 1949-51, which differ insignificantly from the official data.

This leads directly to a broader issue, that is, whether or not the estimated figures in this book are reasonable approximations to the facts. There are a few sets of estimates whose validity is extremely questionable. Table 21, entitled "Electricity Import and Export," is an example. For 1952-56, the difference between the estimated "net domestic supply" and the estimated "total consumption" of electricity has been "attributed" to import or export (p. 81), depending on whether the difference has a minus or plus sign. For 1951 and 1957-60, it is uniformly assumed that Communist China had imported annually 152 million kwh of electricity from North Korea (p. 79). It seems to me that the so-called import and export of electricity in 1952-56 are nothing but statistical discrepancies between the estimated net supply and the estimated consumption. In the first place, Wu has not cited any documented evidence, official or otherwise, to prove that export or import of power has taken place in Communist China since 1951. Secondly, it seems peculiar that Communist China alternated from year to year between exportation and importation in a varying quantity. Ordinarily, power exchange between two countries, with or without a long-term agreement, would have, at least in a short period of time, a fairly stable pattern in which one country is the net exporting party and the other net importing party every year. Thirdly, it is even more doubtful that North Korea would be able to export 152

and 141 million kwh of electricity to Communist China in 1951 and 1952, respectively, when she was suffering from the most serious power shortage in her history owing to the Korean War. According to North Korea's official data, even in 1953 when large-scale military activities had already been discontinued and the truce was signed in July, her electricity output was restored to only 16 per cent of its 1949 level.<sup>1</sup>

To avoid further confusions about the already complicated names of places transliterated from the Chinese, it would be desirable to unify the transliterations if the city is the same. For example, Nan-ch'ung (p. 209), a city in Szechwan, becomes Nan-tung (p. 208); Chang-tu (p. 207) in the former Hsikang province is changed into Chamdo (p. 205); both Chang ch'ang (p. 208) and Chang-Chong (p. 209) are used for the same city in Hainan.

Reference is repeatedly made (p. 82 and p. 248) to "Appendix C"; there is no such appendix in the book.

Nevertheless, all the stains mentioned above do not invalidate any of the major findings and conclusions in this book. Nor are they damaging to its valuable contribution to our knowledge and understanding of the energy industries in Communist China. The book can be recommended either as a source of reference for China specialists or as a comprehensive study in this field to nonspecialists.

Finally, we share the author's hope that the "sectoral approach" or "analysis in depth" exemplified in this book "could mark the beginning of a new stage in the development of the Western World's information on Communist China."

KANG CHAO

*University of Michigan*

<sup>1</sup> See *Joint Publication and Research Service*, 901, January 15, 1960, p. 182. Other sources have indicated that Communist China was responsible for the reconstruction work of the Suifung hydro-electric plant on the Yalu River, which was completed only in 1958, and that the part of the restored generating equipment supplying electricity to Manchuria has been included in the official figures of total generating capacity in Communist China. See *Far Eastern Econ. Rev.*, April 8, 1954, p. 437 and *Chugoku Kenkyu Geppo*, Tokyo, No. 131, Feb., 1959, p. 15.

*The Development of the Indian Economy.* By W. B. REDDAWAY. Homewood, Ill.: Irwin, 1962. Pp. 216. Paper, \$3.75.

This is the first volume in a series of studies on the Economic Development of India conducted by the Center for International Studies at M.I.T. The book is supposed to give an over-all view of the development of the Indian economy and it does this job remarkably well. However, for those interested in a critical evaluation of Indian planning, the book does not promise too much. Most of Professor Reddaway's writing seems to be colored by the opinions of the people in the Planning Commission and in the Indian Government Service with whom he had close contact.

Part I of the book is titled "Analysis of the Problems." In this section Reddaway first discusses the limits to the growth of India's national income.

Chapter 1 mentions the population problem as the major limiting force. Chapter 2 discusses the nature of the unemployment problem in India. Chapter 3 on "International Considerations" mentions the limits to the expansion of export earnings, and Chapter 4 discusses the limits to the expansion of individual sectors. The next three chapters are devoted to planning techniques. Chapter 5 is on the "Broad Strategy of Development." Chapter 6 on "Some Problems of Choice" is a chapter on choice of technology and has a discussion of labor-intensive techniques. It is really surprising that this is the only problem of choice that has received the attention of Reddaway. Chapter 7 on "Planning in Practice" gives the basic principles of planning techniques. The major drawbacks of this section are the complete neglect of problems of monetary-fiscal policy and the prosaic discussion of international trade policy. Reddaway merely repeats the general view current in India that it would be extremely difficult to secure a substantial net increase in India's exports. As is common with this argument, he starts with tea, jute, etc. There is no discussion of the problem of devaluation or the drawbacks of India's foreign trade policy (the licensing scheme and thousands of rules, regulations, exceptions, taxes and subsidies). One relieving feature of this part of Reddaway's book is his discussion on labor-intensive techniques (pp. 72-77), wherein he corrects the mistakes in the logic of some proponents of these techniques. The problem of "choice of techniques" has a very strong hold on the minds of many Indian economists. Most of the confusion in this field could have been avoided by a precise specification of the objectives.

Part II presents a model of the Third Plan Period. This model closely approximates the official Third Five-Year Plan both in the assumptions and in the basic data. However, the carefulness with which Reddaway has handled the data and the systematic conceptual framework he has set up would be very useful for those working with problems of development planning. This section is very tedious and many of the things said sound obvious. However, Reddaway has done a good job in working out this exercise in detail and many of the people working in the Planning Commission will benefit from it. This is corroborated by the footnote on page 169 wherein Reddaway gives the opinions on his article on "Importance of Time-Lags for Economic Planning" (which is reproduced as Appendix A of the book). An English noneconomist's comment was "Well, that is all very clear and easy to understand, but it seems so obvious that I do not see why you need to write it"; an official concerned with Indian planning, on the other hand, said that he found the analysis most useful!

The appendices contain the reproduction of a couple of Reddaway's articles published in the Indian economic journals. One of these is the article on the capital-output ratio. No other concept seems to have such a strong hold on the minds of the Indian planners as this ratio. It is high time the limitations of this concept are emphasized. Unfortunately, Reddaway does this job only cursorily. The capital-output ratio is essentially a useless concept for problems of resource allocation or estimation of investment require-

ments. It has been long established in economics that these problems cannot be tackled by the use of capital-output ratios. Even the limited use of the capital-output ratio in industry in the underdeveloped countries rests on the by now well-discredited theory of zero marginal productivity of labor in agriculture in these countries. Looked at from every possible angle the concept is essentially useless (and its use dangerous) for planning resource allocation and estimating investment requirements. The use of marginal and sectoral capital-output ratios does not change the nature of the problem.

On the whole, Reddaway's book is very interesting and will be useful to many interested in Indian planning. However, the book would have been more useful if the theoretical issues had been discussed more elaborately.

G. S. MADDALA

*Stanford University*

*The Political Economy of Mexico—Two Studies.* By WILLIAM P. GLADE, JR. AND CHARLES W. ANDERSON. Madison: University of Wisconsin Press, 1963. Pp. vii, 242. \$5.00.

This book is comprised of two separate monographs growing out of the authors' participation in the Ibero-American Studies Program at the University of Wisconsin. The one written by Professor Glade is entitled "Revolution and Economic Development: A Mexican Reprise," and the one written by Professor Anderson is entitled "Bankers as Revolutionaries: Politics and Development Banking in Mexico." The justification offered for combining these two rather disparate studies under a single cover is that they "share as a common theme the saga of Revolutionary Mexico and its economic progress" and constitute "experiments in the use of interdisciplinary approaches as a way of getting at some of the basic problems of economic development" (p. v).

The basic hypothesis of Glade's study is that there is an "essential unity" to be found in the totality of Mexico's post-revolutionary economic and social experience, and that it is to the Revolution and the many changes it produced that one must look for the explanation of Mexican economic development. Accordingly, his study represents "an attempt to trace and appraise the impact of the several components of the Revolution on two sets of variables, sociological and economic, which bear on the capacity of the Mexican economy to produce ever larger outputs over time" (p. 23). This difficult assignment is complicated by his decision to employ "a 'field' concept of interacting forces rather than the more mechanistic concept of cause-and-effect relationships" (p. 23) as the basis of analysis. Thus he is led to search for evidence in a wide assortment of sociological, economic, political, anthropological, psychological, and artistic literature, relating to the experience of modern Mexico. Indeed, one of the most impressive things about Glade's work is the manner in which he has been able to draw upon the findings of many specialized observers in support of his thesis. But while he is deserving of congratulations for a good effort in attempting to carry out a difficult assignment, perhaps it is neither uncharitable nor evidence of lack

of sympathy with the interdisciplinary approach to suggest that Glade fails to develop a really convincing interpretation of either the Mexican Revolution or the record of post-revolutionary Mexican economic development—and certainly not of the relationships between the two.

In the same way that Glade's study is intended to have general relevance to "the problem of revolution as a possible precondition for economic growth" (p. vi), Anderson's piece on Mexican development banks is offered "as a case study whose relevance extends beyond the particular example under consideration insofar as it serves to explain the political process that surrounds all institutions of this type" (p. 108). More particularly, however, Anderson is concerned to show how the Mexican banking system has been employed as a means for both stimulating and regulating the process of economic development in accordance with prevailing political realities.

Anderson's monograph is marred not only by superficiality but also by occasional lapses into uncommonly opaque and evasive language. Furthermore it contains numerous erroneous statements (e.g., monetary policies during the 1930's were not conservative as claimed on page 120 nor were the first attempts to centralize the issuance of paper money made in the 1890's as implied on page 118) and questionable judgments (e.g., in the opinion of this reviewer Mexico has not modelled its development banking system excessively on those of other countries as suggested on page 184, but rather has, on the one hand, been quite original in devising institutions and procedures appropriate to its own peculiar circumstances and, on the other hand, has so far failed to borrow from abroad certain financial techniques which could be quite helpful). However, Anderson's basic contention that understanding of the Mexican system of development banks requires an appreciation of the political forces giving rise to the system and conditioning its operation is quite correct.

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*The British Economy in the Nineteen-Fifties.* Edited by G. D. N. WORSWICK AND P. H. ADY. New York: Oxford University Press; Oxford: Clarendon Press, 1962. Pp. xi, 564. \$8.00.

This is a first-rate (and sometimes quite lively) analytical and critical survey of economic events, developments, and policies in Britain during the decade of the 'fifties. Like its predecessor, *The British Economy, 1945-1950*, it consists of a series of carefully interlaced essays and papers by a group of Oxford economists under the editorship of G. D. N. Worswick and P. H. Ady. The high standard set by the previous book is maintained, if indeed it is not exceeded, by the present volume.

The book as a whole is for the informed general reader, yet its array of special chapters furnishes solid fare, as well, for the specialist scholar. A tightly and most competently written introductory chapter by Worswick presents an over-all running account of the period. It is followed by parts on "International Trade and Payments," on "Government and the Economy," and on

"The Consumer, Capital and Labour Markets." There is an annotated bibliography. The book gains much in interest and usefulness from its varied authorship; and, because of the skill with which the separate chapters have been planned, prepared, and put together, the gain has been purchased at a very low cost in direction and unity. There is something here for every economist—macro, micro, industrial, agricultural, labor, international, monetary, fiscal, consumer, everyone.

Turning to the chapters in which this reviewer finds himself most at home, those devoted specifically to Government and the Economy, the book treats of general regulations and controls affecting all or a large number of industries, of specific interventions in the affairs of particular firms or industries, and of more permanent or continuing policies toward particular industries. In all of these categories, typical cases are examined, and a critical analysis is made of programs and policies. In most instances the Government comes out considerably bruised and shaken from the examination—but it does come out, and under its own power! Not in all cases, but in most, the attack is delivered more against *the* program than against *a* program.

The relaxation of direct wartime controls was well advanced by 1950, but the trend was interrupted by the Korean affair. By the end of 1954, however, "the structure of controls had been largely dismantled," but still with some varied and interesting "survivals." A person unfamiliar with what had gone before could be pardoned for failing to appreciate that the galaxy of controls that still remain in Britain constitutes only a dismantled structure. With reference to the extent and pattern of the program of elimination, the book points out that it is probable that "the change of government in 1951, and the success of the Conservative Party in the two subsequent elections, made less difference than is often supposed" (p. 334). All of this, together with the fact that in several important instances (e.g., price controls over iron and steel scrap) business itself did not favor abolition of controls, suggests that a *British Economy in the Nineteen-Sixties* will almost certainly be compelled to devote even more space than does the present volume to the section on Government and the Economy.

In dealing with "Specific Interventions," and after making clear that the treatment does not pretend to be exhaustive, the book does a real job on government actions taken on sulphuric acid, titanium, fuel oil, locomotives, the "siting" of steel strip mills, the "rescue" of the cotton textile industry, and Cunard and the two "Queens." The treatment, under a full head of steam, continues with reference to the "Continuing" policies of government in relation to private industry; it loses none of its force as it rides over government and steel and government and the aircraft industry. Scarcely pausing to change authors, the drive proceeds relentlessly in the chapter "The Nationalized Industries" to take apart government investment and pricing and coordination policies in the fuel and transportation industries.

— Students of British regulation and public enterprise have become familiar with the propensity of British government agencies to worry about, and ever-

lastingly to experiment with, procedures and forms of organization. Awareness of the importance of procedures and organization in controlling and operating economic ventures in the context of British democracy, and frequent revision and reorganization in the wake of a steady flow of studies and enquiries, are imbedded in British thinking and practice. One gathers from the present study that even more introspection and retooling are in order. Four features of the British machinery of government are named which "make it difficult for questions of economic policy to be properly considered, and which collectively have an obvious effect on the quality of decisions taken by British governments": (1) the marked tendency to look at problems in strictly departmental terms (the danger is accentuated by the dual role often played by the Treasury—formulating general economic policy, and laying down economic criteria for policy decisions on the one hand and controlling and minimizing government expenditure on the other); (2) almost exclusive concentration on problems of immediate importance; (3) "a strong preference for secrecy and informality, for withholding information and preventing or abbreviating public discussion" (a practice rarely questioned even by the party in opposition); and (4) the failure to ensure that specialized economic advice is available within the departments which are necessarily concerned with economic questions (pp. 374-77). The tendency of Ministers to intervene in details of the management of nationalized industries without taking responsibility for failure of policy and the failure of the Government until after the close of the decade to announce a policy on the application of the nationalization statutes are thrown into the indictment for good measure. It is added that the policy as finally announced "codifies the muddles of Whitehall thinking and evades decisive judgments in the fields where it might be thought the business of Government to decide" (p. 416).

Worswick, in his introductory chapter, also pays his respects to the Government's general economic policy. Until the middle of the decade, the Government "appears not to have had any very definite economic aims"; thereafter, the Government seems to have regarded interrelated objectives of policy as "separate problems, which might be solved piecemeal and *seriatim*" (p. 72). The changeover from the use of direct control to "global" regulators comes in for questioning: to work, indirect controls must have the impact on individuals of direct controls, but they are less effective. Their use coincided with the "Government's preference for backstairs intervention" (p. 72). Finally, in a paragraph developing the theme that "The Government relied far too much upon an implied harmony of private and public economic interest," Worswick really punctures the "admonition to be responsible" balloon—a feat in itself worth the price of admission. Indeed, at this point the American reader is likely to turn to the title page, to make certain he is reading about the United Kingdom rather than the United States. It occurs to me that the same reader, if he is a professional economist, might pause for a moment as he races through this and other formidable accounts of governmental shortcomings, to consider whether, as economists, we might help mightily in

the business of government regulation and enterprise if we were to forego some of our insistence upon unattainable precision in these matters and were instead to attempt the formulation of simpler, blunter ends that are worth attaining and are attainable with the kinds of tools available to a democratic society, and which we might help to devise.

There are many places in the book at which discussions of, or comments on, British economic policy problems or practices strike the reader as having special relevance to the American scene. There is an excellent formulation of the "self-finance" problem that arises in determining the pricing and investment policies of regulated and nationalized industries (p. 425), which should be translated and made required reading for our own regulatory commissions. And throughout the treatment of the transportation morass there lurks an understanding of a problem that defies understanding. What are we political economists going to do with this transportation problem, anyway? It is difficult enough for us to proceed rationally where a discernible line can be drawn usefully between industries where competition works and where it doesn't work, and where the industries have some respect for lines. But where, as in transportation, the several component industries insist not only on crossing and recrossing all lines but in intermingling and cohabitating indiscriminately, the problem is clearly beyond us. We must, of course, continue and extend our studies, but only two lines of active policy seem open: to move in with a club, swinging, or just to relax and ride and ship.

The chapter on the consumer sector contains a brief, but informative, run-down on Britain's brave new experiment in antitrust, the 1956 Restrictive Trade Practices Act. In the British approach to the problem of monopoly, it is said, "the complicated valuations involved in the disposal of welfare issues are faced as each issue arises" as distinct from the U.S. approach which simply adopts "the particular set of valuations involved in the concept of the competitive optimum" (pp. 451-52). "The ending of some restrictive agreements was followed by mergers, and perhaps the 1956 Act was a contributory factor," but, at least, "by 1960 there was far more price competition . . . in British wholesale and retail trade than there had been before 1956, and the provisions of the . . . Act prevented this return to price competition from being frustrated" (p. 457). Well, we shall see.

"Measured by almost any of the available indicators, the fifties was a decade of economic progress." Production and consumption rose, consumer durables were extended to mass consumption; employment was full, and the area of economic freedom at home and the ability to spend abroad were expanded (p. 68). However, "the fifties ended on the note of record Christmas sales, but one could just hear voices saying that the sixties might bring the need for a new approach to economic policy" (p. 74). And, possibly, a new Government?

BEN W. LEWIS

*Egypt in Revolution—An Economic Analysis.* By CHARLES ISSAWI. New York: Oxford University Press for Royal Institute of International Affairs, 1963. Pp. xiv, 343. \$7.20.

Robert Solow wrote that: "Productivity may turn out to be the hot subject of the 1960's in economics." This reflects his parochial interests and neglects the issue not only of the 1960's but the one which will predominate throughout the remainder of this century—economic development. Many different theories ranging from the essentially Adam Smith variety to the full-blown modern Marxian type are currently being tested, explicitly or implicitly, in countries from Asia to Latin America. Potentially the analysis of the development process under various theories could be influential in guiding the choice of future strategies. Unfortunately most books about developing countries concentrate on describing change, rather than analyzing it. Typically they open with a few chapters of historical notes to set the scene, followed by a section which describes the objectives and framework within which current development is proceeding, then the bulk of the book is filled in with a sector by sector description of the structure of the economy, and finally a closing chapter summarizes the prospects the country faces. Economics desperately needs a James Joyce or a Lawrence Durrell who can put the interconnecting parts into a huge literary matrix where the interdependence becomes explicit and a new time dimension is added.

Professor Issawi has not attempted to forge a new frontier among contemporary historians. He offers a conventional study of present-day Egypt and how it got that way. The analysis of economic change clearly takes second place to describing the change. This is not to suggest the book is devoid of theory. The topics which have become the principal theoretical issues of economic development are scattered throughout the book, but they do not form the basis of its framework. Rather they provide an occasional interlude between facts, facts, facts.

A small but random sample revealed that there are probably about 22 quantitative data on each page, something in the neighborhood of 7000 quantitative facts in the book. In it we learn *inter alia* that:

... between the Revolution of 23 July 1952 and 1961 the share of national income absorbed by the government rose from a little over 20 per cent to some 60 per cent ...

... in 1961 the President declared that 95 per cent of industry is either owned or controlled by the government ...

... deserts constitute 95 per cent of the country ...

... whereas in the United States in 1945 an average acre of farm land was worth the equivalent of less than 10 days of the average worker's wages, in Egypt an equivalent farm land claimed a price equal to about 20 years of the average worker's wages ...

... all imports are now made through government-owned companies ...

... the government is responsible for 4/5 of total investment ...

... the total output of Egypt is probably not much larger than that of a city like Baltimore with a million inhabitants ...

. . . its population density on the inhabited portion is one of the very highest in the world at over 650 persons per square kilometer. . . .

These and other facts are remarkably useful to describe recent changes and in putting modern Egypt into a proper context, but on sum the sheer volume of them becomes a powerful sedative.

The book is organized into four distinct sections. The first comprises three chapters which set the historical background. In the preface the author has said that "during the last ten years Egypt has undergone more radical changes than at any other time in its long history, *with the possible exception of the 1820's*" (italics added). This earlier period of forced industrialization is, however, inadequately discussed, and no comparisons are made between it and the present development efforts. This section is followed by a lengthy chapter on Arab socialism (in Egypt) between 1952 and 1962, its development and ambitions. Civilian influence predominated between 1952 and 1956, but since then Egypt has become a "totalitarian socialistic state" due largely, in the author's opinion, to events beyond its control: Baghdad Pact, Suez, High Dam affair, and the general "world wide trend towards socialism and planning." Unfortunately neither of the critically important aspects of a centrally planned economy, the planning process and the methods of execution, is adequately discussed. Then comes the bulk of the book in eight chapters, each dealing with a different sector, the usual descriptive topics familiar to readers of development plans: human resources, national income, agriculture, industry, transport and communications, foreign trade and balance of payments, finance, and public finance. The final chapter begins with a long summary of the problems of developing countries generally and concludes with the problems Egypt will face in the future, about which the author is "convinced that Egypt's long-term prospects are good because it should, eventually, be able to make fuller use of its deserts." This is cold comfort indeed considering that no one has yet been able to break through the technological barrier to make the deserts bloom.

The book's accessories include a convenient table of conversion factors for the local Egyptian measurements, a practice which should be followed in all books of this sort. There are three short appendices. One lists public organizations by the controlling ministry. Another discusses various estimates of investment between 1946 and 1961. The last gives a few descriptive paragraphs about "the main facts regarding capital, employment, equipment, and output in the principal industries, as well as plans for expansion." Those wishing to pursue further specific aspects of Egypt's development will regret the absence of an extensive bibliography. The chapters are, however, heavily footnoted and the principal UAR official publications have been listed. French, German, and Latin are required to make sense of the several quotes which preface each chapter. This is certain to irritate those readers who feel that Max Schulman's sublime satire should have put an end to such practice.

Even though many economists would be hesitant to use *revolution* to describe economic change (especially those who have been closely involved

in advising, assisting, and encouraging progress among developing nations), no reader of this book should object to its use. Issawi has masterfully amassed the pertinent data documenting the economic revolution of post-Farouk Egypt and has provided enough extra data to confirm social and political revolutions as well. In closing and without offering a judgment the author poses a perplexing problem for the West: should it continue to support Egyptian development when its avowed aim is to spread Egyptian influence and its brand of Arab socialism throughout the Middle East and Africa?

Originally an active participant in Egyptian economic affairs, Issawi writes in this third and final study from an objective and detached position. Because of the facts and source references it contains, this book will become a standard text on contemporary Egyptian development. But owing to its emphasis on description, it cannot be generally recommended for its analysis of development under centralized planning.

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### **Economic Systems; Planning and Reform; Cooperation**

*Problemy optimal'nogo planirovaniia, proektirovaniia i upravleniia proizvodstvom.* (Problems of Optimal Planning, Project-Making and Production Management.) Moscow: Moscow University Press, 1963. Pp. 546.

In the two years that elapsed between the path-breaking conference on mathematical economics of April, 1960 and the "Theoretical Conference" held at Moscow University in March, 1962, the proceedings of which form the subject of this review, by far the most significant development was the revival of Soviet interest in demand theory. When we recall the pioneering contributions of Russian economists in this field in the first quarter of this century (by Eugene Slutsky, A. A. Konüs *et al.*), the intellectual poverty of the last three decades seems bleak indeed. Not only was utility theory ruled out in Stalin's time—as subjective, idealistic, and generally un-Marxist—but even the notions of income and price elasticity were discarded. A few years after the death of the Soviet dictator, these fundamental concepts crept back into statistical work and even acquired a modest measure of ideological respectability; yet the more abstract aspects of "bourgeois" demand theory were still resolutely condemned. When, for instance, Konüs, interrupting a long period of silence, exceptionally introduced a utility function in a theoretical paper he wrote for the *Festschrift* in honor of Academician Strumilin in 1957, he was soon after taken to task for doing so by A. Boyarsky, who for several years appears to have acted as the chief custodian of ideological purity in matters of mathematical economics. By 1962, however, the liberal school of Soviet mathematical economists had become strong enough to convoke and to dominate a conference under the auspices of the Department of Political Economy of Moscow University, known hitherto as a bastion of orthodoxy. The first group of papers presented at the Conference all dwelled at some length on the problem of formulating objective functions for programming purposes and of determining the relation between these functions and con-

sumers' preferences. In my comments, I shall deal only with the Conference papers of wider economic significance contained in this first group, while generally recommending to specialists in operations research the wide selection of papers on the optimal location of production activities, transportation problems, investment efficiency, and production scheduling contained in the rest of the volume.

The first paper presented at the March 1962 Conference, by V. A. Zhamin, a political economist at the University of Moscow, sounded the keynote of the proceedings:

It is well known that in the period of the cult of personality attempts to apply mathematical methods to economics received no support whatever. Relapses of this incorrect attitude to mathematics have been preserved in the consciousness of many economists to the present time. This serves as a direct hindrance to the progress of our economic science. The nihilistic attitude to mathematics is reflected, in particular, in that the works of bourgeois economists of mathematical orientation are often subjected to criticism, not for real mistakes and for the conclusions drawn from them, but for using the mathematical apparatus, for the use of terms, if the latter did not yet appear in the classical works of Marxism-Leninism or appeared in these works in some other aspects.

As a result, a few questions of importance for theory and practice are not worked out, because the specialists are aware that a serious investigation would necessitate the use of these terms. Why, for instance, are there practically no theoretical studies on the analysis of consumer demand in our literature? Ask the comrades dealing with these problems. They will answer: because the serious analysis of these problems requires the use of such concepts as utility, consumer preference, etc. And for these concepts one may be criticized (pp. 9-10).

A. Boyarsky, whose paper followed Zhamin's, fought a rearguard action in favor of a theoretically conservative approach to the use of mathematics in economics. Compared to his speeches at the Conference of April, 1960, he was willing to make limited concessions on marginalism and on the use of objective functions to solve economy-wide problems (as long as there were several such functions and a number of variants of the over-all plan which the authorities could choose from), but he was adamant in his rejection of shadow prices for economy-wide problems and in his refusal to recognize the notion of opportunity cost. Instead of letting sleeping dogs lie, Boyarsky, by stressing the doctrinal deviations of his opponents who dominated the Conference and by the weak arguments he summoned to defend the orthodox position, opened himself to rebuttals that probably did more harm than good to his cause. His more dogmatic theses were easily demolished in the discussion that followed the papers by a phalanx of critics including Konüs, V. A. Volkonsky, I. Girsanov and A. Lurie. The last two pointed out that Boyarsky was virtually the only participant at the conference to hold his obsolete views.

L. M. Dudkin of Moscow University, in an interesting paper on a "Mathematical-economic Scheme for an Optimal Material Balance of the Socialist

National Economy," made some specific suggestions as to how demand theory ought to fit into the task of designing planning models. The objective function—the maximand—of such models, he argued, should be "the greatest possible satisfaction of working peoples' needs," which, it is clear from the context, he interpreted as a Pareto optimum for consumer goods (p. 42). Dudkin recognized that the planners would have to ascertain the preference orderings of consumers "to enable us to choose the most preferred point consciously." Was there anything ideologically objectionable in that? Not at all, he alleged, since the existence of preference functions is implied by the existence of consumers' demand functions for every given set of prices and incomes. To substantiate this mathematical relation, he calmly referred his Soviet readers to the proof by H. Uzawa of Stanford University. He then went on to prick the dogmatists for the inconsistency of their position:

The working peoples' function of consumer preferences is nothing else but the counterpart of their demand function, and is really only its other mathematical expression, the result of the mathematical transformation of a demand function. This transformation is no less justified than any other mathematical transformation used in the concrete sciences. This will be confirmed by any mathematician who understands the real nature of the problem. Consequently, those who speak out against the use of the apparatus of consumer preferences without objecting to the use of demand functions are in fact opposing the legitimacy of applying mathematics to economic research, and withal, to economics (p. 43).

Although Dudkin recognized that consumer preferences should be corrected in the light of scientific research in the interest of society as a whole, he thought it was best to start off from these preferences and only then to make specific corrections for individual products, "inasmuch as there is not a single planning and scientific organ at the present time that is in a position to evaluate better than the consumers themselves the relation among their complex and variegated material and cultural needs (which relations evolved objectively in the course of long experience)" (p. 45). Volkonsky, in his oral intervention, added to Dudkin's arguments that catering to consumers' preferences was the "democratic approach" to the planning problem. These defenses of the rationality of consumers' choice strike a new note in Soviet economic writings, as does the postulate of consumers' sovereignty underlying Dudkin's entire model.

Dudkin sketches out his scheme in its static and dynamic aspects, maintaining in both cases his assumption of linearity in the constraints, but he fails to elaborate the mathematical properties of his models. These more technical aspects of the Dudkin model are taken up by the mathematician I. Girsanov, who, in an independent paper, examines the minimum assumptions required to build an operational program geared to consumers' preferences expressed in a free market for consumer goods. He finds it necessary to postulate diminishing marginal utility for every good and to make all goods gross substitutes. In the model he elaborates, the prices of consumer goods are those that would obtain if the quantities determined in the program were sold on

a free market. For calculation purposes, capital goods and intermediate goods would be assigned the implicit prices of the constraints imposed by the availabilities of these factors during the planning period. The entire labor force, he assumes, would have to be employed in every admissible program. Wages for different categories of workers would be endogenously determined in this centralized model (via prices and productivities). The planners' problem, in short, would be to find a bill of consumer goods that would generate an acceptable wage distribution corresponding to the prices for all goods in the system.

Finding an optimal solution to the Dudkin-Girsanov model would be tantamount to simulating a general equilibrium in a market economy. Was Enrico Barone excessively pessimistic when he warned at the turn of the century of "the difficulties—or more frankly, the impossibility—of solving such equations a priori"? Girsanov claims that an algorithm *can* be devised to solve concrete programs, within the limits of the linearity and other simplifying assumptions he builds into his model. It will be instructive to watch what further progress he and his colleagues will make in this direction.

Although Girsanov confines his speculations to the static model, he suggests that he could extend his results to a multiperiod scheme. To do this, presumably, the planners would have to know, in addition to the demand functions of consumers for all goods and prices in each period, their preferences for incomes accruing in different periods (if not their intertemporal preferences for every good). Dudkin and Girsanov seem to be so neoclassical in their conceptions that one may wonder whether they would accept Pigovian restrictions on consumers' sovereignty; whether, for example, they would allow that the economy's aggregate rate of saving determined by the intertemporal preferences of consumers must be corrected for their myopic appreciation of the future benefits of present sacrifices. (After years of austerity imposed on consumers by presbyopic planners, there is perhaps something to be said for letting consumers have things their way.)

What is the potential significance for planning practice of these models? Are these the Soviet equivalent of the Utopian visions and pipe-dreams of nineteenth-century economists? Or is there a real chance that they will be seized on to aid the planners in drawing up their plans? At this juncture, I cannot envision the Soviet authorities ministering to the sovereign interests of consumers. If the mathematical economists wish to do more than "play with numbers," as Stalin once taunted them, they will have to incorporate into their models the objectives laid down by the Communist Party, which are likely to diverge seriously from those that could be read from the revealed preferences of consumers. The question is, then, whether the Party authorities will be agreeable to making these objectives explicit enough to enable their translation into mathematical language. Will they be willing to put numbers on the relative priority of armaments and consumer goods? "Scientific planning" may entail the abandonment of certain time-consecrated practices, such as the setting of unattainable goals for farm output and for standard-of-living increases, which serve a hortatory purpose in mobilizing the pop-

ulation for support of long-range plans but which are undermined, in the course of executing these plans, by *ad hoc* allocation decisions deflecting resources from low-priority uses.

Girsanov, incidentally, hints that it may not be easy to find a common language with the operational planners, but he begs the question when he suggests that "one should separate theoretical from practical questions, and abstain from dealing theoretically with a number of difficulties that may be solved in practice by the intuition of the planners" (p. 68). For who is to draw the line between hidebound routine and intuition, between poor judgment and the fruit of long experience?

Once the semantic problems have been solved, the theoreticians will at last be in a position to confront their ideas and thrash out their differences with the administrators of the Soviet economy. If the highest authorities decide to let the views of the mathematical economists prevail, then a re-examination of the goals of Soviet planning may be in order. But if these economists are forced to back down, they may well have to confine their efforts in the future to areas that are less sensitive politically than the design of objective functions for nationwide planning.

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### Business Fluctuations

*The Puerto Rican Economy and United States Economic Fluctuations.* By WERNER BAER, Rio Piedras, Puerto Rico: Social Science Research Center, University of Puerto Rico, 1962. Pp. xvi, 155.

The stated purpose of this study is "to analyze the past, present, and possible future effects of economic fluctuations in the United States on the Puerto Rican economy" (p. 1). Actually, the study is much less ambitious, for it deals mostly, though intensively, with the effects on Puerto Rico of only the downturns of U.S. economic activity in only two instances, 1953-54 and 1957-58. The conclusion is that "the two Mainland recessions in the 1950's revealed a very mild reaction on the island. The 1953-54 recession was mainly felt through a substantial decline in net migration, thus causing a rise in unemployment. In the 1957-58 period the recession was passed on mainly through a widespread production curtailment of older firms in the export industries" (p. 141).

Despite the objective evidence provided by his statistical analysis, the author contends that "the true sensitivity of Puerto Rico to the Mainland cycle" (p. 141) was not manifested by Puerto Rico's reactions to the two downturns of the 1950's, principally because of the rapid growth of the island economy—a 70 per cent increase in real gross product from 1947 to 1958 (p. 16). He spends most of his pages uncovering institutional and structural changes which bear on the "real" sensitivity of the Puerto Rican economy.

One institutional change is the tendency toward lower excess reserves held by Puerto Rican banks. These reserves have in the past cushioned the economy from the impact of Mainland credit restrictions. A structural change of note is the increasing relative dependence of the Puerto Rican economy on exports: exports of goods and services as a percentage of gross product rose from 32 per cent in 1939-40 to 44 per cent in 1947-48 to 53 per cent in 1956-57. Over 90 per cent of both exports and imports have been with the U.S. Mainland all along (p. 18). Closely associated is the change in the type of exports. Food, for example, accounted for 70 per cent of exports in 1947-48, but for less than 30 per cent in 1957-58. A rather ingenious approximation of income-elasticity analysis, for U.S. commodity groups that include most Puerto Rican exports, "revealed that Puerto Rican exports are increasingly concentrated on products experiencing sharper fluctuations over the cycle" (p. 143). Imaginative use of the Leontief input-output table for the U.S. economy in 1947 enables Baer to calculate that the "changed commodity structure of exports . . . reduces the island's sensitivity to the Mainland cycle by increasing the import content of exports from not quite 11% to over 19%" in a decade (p. 122). In another statistical exercise the author relates "fluctuations in exports and labor earnings when both seasonal fluctuations and the trend have been eliminated" (p. 30). From very limited statistical evidence "it was estimated that without the mitigating growth factor, fluctuations in exports would have had an effect on labor earnings three times as powerful as was actually experienced in the last two recessions" (p. 147). A chapter is devoted to a detailed analysis of the behavior of a selected group of firms in an attempt to discover their reactions to Mainland recessions. The conclusion is that in the absence of the growth factor "fluctuations would be passed on in a slightly magnified way" (p. 107).

Baer does not, in the opinion of the reviewer, successfully demonstrate that the Puerto Rican economy will be increasingly vulnerable to U.S. recessions as the island's growth rate tapers off. In fact, on the basis of the reviewer's investigations, export instability for a country tends to be positively correlated with growth and negatively correlated with foreign trade involvement (ratio of trade to GNP).

Baer's study is far from being a model monograph. It bears the earmarks of having been rushed into print. It lacks precision, care, and polish. Boldness rather than scientific caution characterizes the drawing of inferences from the statistics. The author seems to be trying to prove a case instead of testing an hypothesis. The theoretical observations at the start and the policy discussion at the end could have been omitted to advantage.

In spite of these critical comments, the study tells much about the Puerto Rican economy and is an addition to the small stock of empirical studies dealing with the problem of international or interregional propagation of economic instability, a problem that has defied meaningful general theorizing.

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**Money, Credit, and Banking; Monetary Policy; Consumer Finance; Mortgage Credit**

*The Composition of Consumer Savings Portfolios.* By HENRY J. CLAYCAMP. Urbana: The Bureau of Economic and Business Research, University of Illinois, 1963. Pp. x, 99. \$3.25.

Theoretical and empirical analysis of portfolios is gaining increasing attention from two major groups of economists: those who attempt to explain consumer behavior by explicit reference to motivating conditions and forces and those who are developing saving functions by breaking total saving into changes in individual assets. Claycamp's study is a contribution to the stream formed by, among other studies, some publications by George Katona and the paper by Harold Watts and James Tobin, in *Proceedings of The Conference on Income and Consumption*, Vol. II.

The basic hypothesis of the book is that ownership of any given asset does not affect the probability of ownership of any other asset. This "independence hypothesis" predicts that the relative frequency of ownership of any combination of assets will equal the product of the relative frequencies of ownership of single assets that enter into the given combination. Absolute and relative differences between predicted and actual frequencies of holding various combinations of assets are presented in tables and the differences are submitted to "t" tests. The tests are applied to frequencies of ownership of 12 classes of assets and one debt class. Differences between predicted and actual frequencies are small, and, with few exceptions, the null hypothesis is accepted. Five rejections involve common stock. The tests of the hypothesis are carried out at an aggregative level, but the procedure is equivalent to a more direct microeconomic test using dummy variables for asset holdings and fitting least-squares regressions.

In a model of portfolio selection the author provides a plausible explanation for the lack of substitution effects. The model postulates: (1) many diverse motivations for saving; (2) assets which have many different characteristics in differing degrees; and (3) consumers who differ in their knowledge of and experience with different forms of assets. Thus two consumers may satisfy common motives with different portfolios, and a single portfolio may satisfy differing motives for two holders. The model suggests that the complex network of motivations, characteristics of assets, and perceptions of assets could produce an aggregate distribution of assets with no interdependence between individual assets.

A second major part of the book seeks an explanation of differences among consumers in the proportion of wealth held in assets which vary in money value (shares of stock, real estate, equity in unincorporated business). The major effects discovered were those associated with total assets, age of the head of the consumer unit, self-employment, home ownership, size of the consumer unit, and reported saving for a major purchase.

The analysis of portfolios in terms of fixed versus variable money-valued assets is related to the independence hypothesis only indirectly in that the

two analyses have a common motivational framework, Lewin's field theory. The relationship could be more direct, as Claycamp suggests, in other studies of portfolio structure. While independence between assets may exist at the extreme of disaggregation of wealth posited in Claycamp's study, interdependence of assets may exist at other levels of disaggregation, e.g., between assets with fixed as opposed to variable money values.

Claycamp correctly suggests that the independence hypothesis merits further testing. It is indeed a provocative hypothesis and additional supportive tests could establish a fairly well-defined static equilibrium position for consumers' portfolios. The forthcoming results of the Federal Reserve Board's 1963 Survey of Families' Financial Characteristics will be an excellent body of data for such tests. Other data are already available. The hypothesis does not imply that "in specification of the factors influencing the demand for a given asset, the interaction of that asset with other assets can be disregarded" (p. 29)—not if we are concerned with changes in the amounts held. If, in fact, the changes in assets were independent, the task of explaining total saving on a disaggregated basis would be greatly simplified. The flows of assets would be additive and it would no longer be necessary to lead respondents in financial surveys through tedious details of many assets.

*The Composition of Consumer Savings Portfolios* is the third of a series published under the sponsorship of the Inter-University Committee for Research on Consumer Behavior. The data used in the study were obtained in several sample surveys of a panel of households in two major metropolitan areas. The sample was designed primarily to seek methods of validating consumers' reports of asset holdings and respondents were selected from lists provided by cooperating financial institutions. The sample therefore does not represent a familiar universe, but it is appropriate for the questions posed. The data used are perhaps unique in that they were corrected for errors revealed in the validation process and are relatively free of the response errors that plague surveys of consumers' wealth. Claycamp has tested skillfully a new, and possibly significant, hypothesis about consumer behavior.

HAROLD W. GUTHRIE

*University of Illinois*

*Commercial Bank Loan and Investment Policy.* By DONALD R. HODGMAN. Champaign: Bureau of Economic and Business Research, University of Illinois, 1963. Pp. 181.

"The primary objective of this study," Professor Hodgman states in his introduction, "is to describe and analyze the habitual procedures, criteria of judgment, and attitudes of commercial banks as they make decisions concerning loan and investment policy." An understanding of the manner in which these decisions are made is important in contributing to our knowledge of the working of monetary policy.

The study is based on intensive interviews with the senior management in 18 commercial banks in Chicago, New York, and San Francisco. A comprehensive questionnaire served as the basis for and guide to these interviews.

Financial support was provided by the Merrill Foundation and by the bureaus of business research at the University of California and the University of Illinois. The 12 chapters in the volume cover such topics as loan and investment policies, commercial bank liquidity needs, attitudes towards capital adequacy, customer relationships, and the implications of the findings for public policy.

Based on a careful study of banking practices, policies, and attitudes, the study represents a pioneer work and makes an important contribution to our knowledge of credit and monetary processes. It stands in refreshing contrast to those studies of monetary and credit policy, constructed on hypothetical models which do credit to the vivid imaginations of the progenitors, but which do not reflect actual practices.

A considerable part of the study is concerned with the loan policies of commercial banks. The author finds that banks generally try to forecast both deposit fluctuations and loan requirements. Some employ sources and uses of funds analyses, some rely on area projections of personal income, some base aggregate forecasts on the individual forecasts of bank officers, some try an "intuitive" approach. Loan and deposit forecasts are important, even though the forecasts may prove erroneous, in compelling a bank to consider all of the factors involved in the determination of its liquidity requirements.

In periods of monetary restraint, when banks must select certain loans in preference to others, Hodgman finds that they give priority to commercial and industrial loans. The reason is that these loans not only carry important deposit relationships but have other ancillary advantages. Commercial loans, to be sure, do not afford the highest net interest return. This is obtained from repair and modernization loans extended to individuals, from retail automobile installment paper, and from installment cash loans. In evaluating the attractiveness of separate commercial loans, the author finds that banks give emphasis, in descending order, to the deposit relationship of the borrower, to his credit standing, to the type and marketability of collateral if any is to be offered, to the prospect of continued satisfactory customer relationships, to the rate of interest, to the purpose of the loan, and to the liquidity of the credit. The rate of interest which the borrower is willing to pay is by no means the most important of the desirable criteria.

In a period of credit restraint, commercial banks are not apt to curtail loans to those borrowers who have a satisfactory deposit relationship. Restraint falls on loans lacking this relationship, such as broker's loans, mortgage warehousing and real estate participations, loans to finance companies, certain term loans, etc. The reason that a policy of monetary restraint falls on loans not having a good deposit relationship, is that bank earnings, in so far as the individual bank is concerned, depend upon the deposit and not the loan potential.

When limits must be set to the over-all loan volume, commercial banks rely on loan-deposit ratios, although many banks declared that there is no magic in a particular ratio. Much depends upon the composition and quality of the loan and deposit volume. Banks, however, wish to keep in step with other

banks and do not like their own ratios to deviate greatly from the mode. The author finds that commercial banks ordinarily do not put limits on commercial loans but do so on consumer credit and real estate loans. Some banks relate the total volume of their real estate loans to time deposits, though many banks have not given careful consideration to the problem of relating maturity of assets to the maturity of liabilities.

The author does not look upon the loan portfolio as a source of liquidity, making the point that loan repayment is a source of liquidity for deposit withdrawals but not for loan expansion. True though this be, loan repayment does enable a bank to extend new loans and to maintain the loan volume. Loan turnover can be a very important factor in portfolio policy.

Changes in security prices, he finds, have an important effect, although not a readily determinable one, upon bank policy. If a bank decides to sell securities at a loss, it must weigh the tax advantages of such sales and the future earnings to be derived from the employment of the funds received against the effect of the loss on capital values. Depending upon all attendant circumstances, the "locked-in" effect may or may not be important.

In discussing interbank competition, the author points out that banks provide a unique and specialized service. There is product differentiation among banks as among manufacturing concerns. The prime rate which came into existence during the Great Depression has had the effect, he concludes, of reducing competition.

The requirement of compensating balances also has the effect of limiting interbank competition. It was not enforced in the decade of the 'thirties and was reintroduced in the 'fifties as a means of cementing deposit relationships and of raising interest charges.

By way of conclusion, the author states that the basic services of banks are the safekeeping and transfer of money. It is the increase in deposits which, from the point of view of the individual bank, permits a rise in loans and investments. This means that depositors and not borrowers are the crucial customers of banks. Policies of credit restraint have their impact on the non-depositor (save as it is ameliorated by extensions of trade credit on the part of depositor borrowers). Restrictive credit policy exercises restraint not so much through a rise in interest rates as through a downright refusal to lend. The conclusion is an important one and is receiving increased attention in analyses of credit and monetary policy.

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### Public Finance: Fiscal Policy

*Federal Tax Treatment of State and Local Securities.* By DAVID J. OTT AND ALLAN H. MELTZER. Washington: Brookings Institution, 1963. Pp. xiv, 346. \$3.00; paper, \$2.00.

This is the second volume to be published in the Brookings program of Studies of Government Finance, a major undertaking in the field of public finance, supported by the Ford Foundation. The book consists of a back-

ground paper (seven-eighths of the volume) for a conference held at Brookings in January, 1962, plus a summary of the discussion at the conference itself. The conferees were about equally divided between people who are involved in the municipals market on either the borrowing or underwriting-investing side and researchers not affiliated with market participants.

The exemption of interest payments on state and local government securities from federal income taxation has survived intact despite a good many assaults throughout the history of the income tax. Its survival, however, has never led to any signs of acquiescence by economists. Traditionally, the major ground for criticism has been the obvious and substantial adverse impact on interpersonal equity in the income tax structure. More recently, the attack has shifted somewhat; along with the general trends in public finance, much of the emphasis is now on the relative inefficiency of the subsidy to state and local governments which tax exemption of interest constitutes. Analysis of the relative efficiency of the device requires quantification, which is inherently worthwhile, since public finance hardly suffers from a superfluity of quantitative analysis. Moreover, emphasis on efficiency generates respectable economic arguments on behalf of the exemption for the first time in its history.

The principal efficiency argument has been that the interest costs saved by state and local governments through the lower yields permitted by the tax exemption are substantially less than the revenue foregone by the federal government. Or, as Roland Robinson concluded in his study, *The Post-war Market for State and Local Government Securities* (National Bureau of Economic Research, 1960), during the postwar period, an increasing share of the revenue loss has accrued to investors in state and local securities, rather than the borrowing governments, who are presumably the intended beneficiaries. A major explanation for such findings is, of course, the special capital market context for the argument. By and large, in recent years, the classes of investors experiencing the most rapid increases in investable funds have been institutions not fully exposed to federal income taxation—pension funds, life insurance companies, and savings institutions—and thus not strongly attracted by tax-exempt securities. Wealthy individuals, commercial banks, and stock nonlife insurance companies, with a considerable interest in tax shelters, have had much more modest growth rates as direct investors in securities.

The Ott and Meltzer study concerns itself largely with efficiency questions, although a wide range of other economic issues is noted in the book. Its major contribution is a new appraisal of the magnitude of the yield differential stemming from the tax exemption, the interest savings of state and local governments, and the revenue losses to the Treasury.

The analysis is careful and comprehensive and goes well beyond the various previous estimates by taking into account—often, necessarily, in a very rough fashion or under stated but reasonable limiting assumptions—a whole range of factors that complicate matters. For example, in dealing with the yield differential, the authors face up to the problem of comparing tax-exempt yields with yields on privately placed corporates, as well as open market yields. Characteristics peculiar to state and local bond issues are not over-

looked: the prevalence of serial, rather than term bonds; the typical steep yield curve within a single offering; the distinctive coupon/offering yield relationships. In estimating the revenue loss to the Treasury, the authors do so on the basis of explicit conclusions as to the shifts of holdings among investor classes that would occur if the tax exemption were removed (which affect the applicable marginal tax rates) and with attention to the consequences of differential tax treatment of capital gains.

The authors conclude, and the findings seem entirely reasonable, that long-term municipals issued in 1960 would have sold for an average of 1.2 to 2.0 per cent more than they did, if not for the tax exemption, an increase in interest costs of 31 to 54 per cent. Over the life of the 1960 issues, state and local agencies would have paid \$1 billion to \$1.8 billion more in interest than they will in fact pay. In contrast, since the weighted marginal tax rate applicable to holders of municipals, were they fully taxable, is estimated at 41-43 per cent, federal revenues (at 1960 rates) would increase by \$1.7 billion to \$1.8 billion under the minimum estimate of the yield differential, and \$2.1 billion to \$2.2 billion under the maximum estimate. By this standard, the tax exemption is inefficient indeed.

The most obvious flaw in the argument to this reviewer—and it is not an overwhelming problem—is the authors' rather ready acceptance of parallelism in the quality ratings of corporate and municipal bonds in estimating the yield differential. The rating systems for municipals involve a fair amount of Doomsday reasoning: if there is a national economic disaster of sufficient magnitude for defaults on general obligation municipals to occur, however wildly improbable this may be, which issue is likely to go under first? In actual fact, the risk of default on lower-rated general obligation municipals may be nonexistent, while the risk of default on equally-rated corporates (for example, railroad bonds) is very real. Thus, a Baa municipal may be equal in riskiness to an A or Aa corporate, and the appropriate yield differential for lower-rated issues may be a good deal smaller than Ott and Meltzer conclude. This, of course, would reinforce their conclusions on the inefficiency of the tax exemption.

Where municipals are low-rated, or not rated at all, because of the small size and unfamiliarity of the borrower, there could, however, be an illiquidity or information cost premium embedded in their yields. Moreover, to the extent that the obligations of small borrowers are not really marketed at all, but placed on a negotiated basis with local investors, the appropriate interest rate comparison in the absence of tax exemption might be with small business borrowing, rather than with the debt obligations of large but risky corporations, implying very high interest costs indeed. For such borrowers, the price of removing the tax exemption might well be some sort of federal marketing assistance, noted by the authors, to encourage large institutional investors in effect to reduce the information and illiquidity costs of the small issues by application of the insurance principle.

DICK NETZER

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*Fiscal Policy for Growth Without Inflation: The German Experiment.* By FREDERICK G. REUSS. Baltimore, Md.: The Johns Hopkins Press, 1963. Pp. xiv, 319. \$7.50.

The mechanisms underlying the striking postwar recovery of the German economy are not what they seem if one relies on eulogies from the financial press in this country or official speeches here and abroad. Chancellor Erhard himself continually emphasizes the *laissez-faire* virtues of the German "free market economy." However, Frederick Reuss suggests that "The hand that rules Germany is definitely not the invisible hand of Adam Smith, nor is it the heavy hand of the socialist planner. It is rather the smooth and hidden hand of a sophisticated and efficient government administration" (p. 28). The operation of this smooth and hidden hand through extensive and detailed manipulations of tax and expenditure policy is impressively and interestingly described by Reuss. In fact, the work is a comprehensive treatise on public finance in Germany and should directly interest students in this area as well as those concerned with economic development.

Because of this comprehensiveness, the reviewer can at best only indicate the general nature of what Reuss describes in detail with the help of extensive statistical tables on every aspect of fiscal policy. When the German monetary reform of 1948 made economic recovery possible, the German authorities found a disorganized economy with heavy unemployment, inadequate financial institutions for intermediation in the savings-investment process, and a generation of individuals whose saving incentives had been destroyed by their memories of two virtually total inflations. In addition, the occupation authorities had imposed a highly progressive corporate and personal income tax system to redistribute income. The new German government started by giving large, ranging down to complete, tax exemptions to individuals and firms who saved and/or invested funds in "acceptable" ways such as for capital goods or housing. The income tax structure was gradually eroded to a complicated tax on particular kinds of expenditures which fell heaviest on current consumption. Subsidies were also used. To take one of many possible examples, income placed in a savings and loan institution and kept there for a period of five years or more was exempted entirely from the income tax. Since this provision was of much more benefit to the wealthy than to the poor saver, in 1959 it was replaced by a straight nontaxable subsidy of 20 per cent of the amount saved, i.e., the effective interest rate on savings deposits rose by 4 per cent. For other forms of saving associated with housing and construction expenditures there remains an option between accepting subsidies or tax exemptions. Total saving in Germany (including that of the government) has been about 21 per cent of the Net National Product.

Besides giving very strong incentives to individuals and firms to save and invest, the federal German government has also acted as a financial intermediary by buying directly the securities of individual firms, undertaking direct government investments, and guaranteeing private bonds to private buyers. The great bulk of such government purchases was financed by current

income so that some 40 per cent of net saving in the economy occurred directly in the public sector in 1960, an increase from 29 per cent in 1950. Thus, this massive intervention by the government is not merely a temporary phenomenon associated with postwar recovery. The fact that government-owned or controlled enterprises are reinvesting profits at higher percentage rates than their private equivalents has led to an increasing share of industrial production accounted for by government-controlled enterprises. For example, in 1959 the government's share was 36.3 per cent in iron mining, 72.2 per cent in aluminum production, and 20.8 per cent in shipbuilding, although the government's heaviest direct investments have been in buildings. This concentration of economic power with the government has made (paradoxically) the German opposition Socialist Party uneasy and was a factor in the sale of the Volkswagen Corporation to private individuals. However, the concentration of economic power with the government remains very high and the complex web of interlocking power shared by government and private industrialists remains a serious social problem in Reuss' view.

Although the German fiscal system is generally considered to be a net success, one could never claim that it satisfies the basic textbook canons of neutrality, equity, and administrative simplicity. Tax exemption of current profits and income used for investment gives tremendous incentives for rapid growth to existing agglomerations of wealth; Reuss strongly suggests that the income distribution in the economy has become more skewed. Transactions taxes which now bring in more revenue than the personal and corporate income taxes (largely because of erosion of the latter two) are certainly regressive and have the further dubious distinction of being largely gross value rather than value-added taxes. Thus, they give significant economic advantages to large vertically integrated corporations. Reuss gives the example that "the tax percentage in the final retail price of men's clothing was estimated at 15.2 per cent for nonintegrated enterprises and 9.1 per cent for integrated enterprises" (p. 130). From an equity point of view, the tax burden is regressive and falls largely on the middle class, although this does not mean that German income distribution is necessarily "worse" than others. In fact, a thoroughgoing German social security system protects individuals at the lower levels of income, something the U.S. system does not do, thus making the equity of the two income distributions rather noncomparable.

A system of special exemptions dependent on the personal position of the taxpayer for the transactions or turnover tax has also grown up, which is administratively complicated like the income tax. This administrative complexity is compounded by a constitution which is unclear in the tax authority granted to the three levels of German government, resulting in shared and divided tax administrations. However, Reuss suggests that this divided authority has permitted effective power to shift increasingly to the federal government, a process which a number of court decisions in favor of the central government has accelerated. It appears remarkable that such a complicated fiscal system does work effectively in the face of these constitutional difficulties.

Throughout the comprehensive descriptive materials in the text, Reuss offers many explanatory and related hypotheses, most of which are insightful, but some could be improved by a more careful development of their analytical basis. For example, he suggests (p. 24) that an inflationary policy in Germany would have been politically and economically untenable because "... the terms of trade would soon have become adverse through inflation. . . ." He repeatedly stresses, and I believe correctly, that a simple Keynesian solution to the very heavy German unemployment in 1948 of simply turning on the tap of aggregate demand would have been entirely inappropriate. The degree of economic disorganization at that time was very great and the aggregate supply of goods and services would have been extremely inelastic in response to demand changes, as he indicates. Thus the policy of deliberately discouraging consumption and directly giving vigorous incentives to build productive capacity was the right one. However, given the present "mature" state of the German economy with great productive capacity and a high volume of personal savings, the Keynesian ideas of policing only the effective level of final demand to control employment and inflation become more appropriate. In fact the direct detailed manipulation of individual taxes is becoming less efficient because of the great erosion of the tax base and more objectionable in its income distribution effects. The incentives to reinvest profits in the same enterprise for the tax advantages greatly handicaps effective monetary policy, as Reuss points out. It is to be hoped that the German authorities do not become so enamoured with the success of their earlier policies that they fail to take into account these changed conditions.

RONALD I. MCKINNON

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*Federal Fiscal Policy in the Postwar Recessions.* By WILFRED LEWIS, JR.  
Washington, D.C.: The Brookings Institution, 1962. Pp. xv, 311. \$6.75.

This is a case study written in the meticulous detail one expects in works in the institutional or historical tradition of political economy. As appropriate as this approach may be, the treatment of a sequence of events over a somewhat short time span detailing day-to-day policy statements and rearranging statistical data, usually regarded from the same angle, does not make necessarily engrossing reading. That Lewis' book shows few of the latter shortcomings, and probably none for those especially interested in a documented account of the period, can be attributed to his familiarity with the material necessary to describe the recent history of federal compensatory finance. This familiarity has allowed him to chronicle events of the period 1948 to 1962, but with a surprising reluctance to assign economic cause to effect except when referring to fiscal stabilization. He provides an arrangement, however, which is most helpful for readers with differing interests in this apparently new variety of economic recessions.

Chapters 1 to 3 are an empirical examination of the effectiveness of built-in fiscal stabilizers. Chapters 4 to 7 form a second section in which the au-

thor devotes some 180-odd pages to an historical description of the fiscal actions in the four recessions since World War II. Four appendices of statistical computations will be of interest to readers who share Lewis' field of specialization. The bulk of the book can, however, be read with gain by any educated layman.

Although Lewis' monograph is empirical in its nature, there is a theory underlying the policy measures he describes. The same policy objectives, however, could stem from a partly different theoretical substructure that would require a different empirical investigation. As Lewis states, and maintains throughout the book, "... this study presumes agreement with ... the general proposition that the federal government ... use its fiscal powers to combat unemployment ... " (p. 2). In other words, Lewis in accord with one school of thought is primarily interested in the fiscal effects on aggregate demand as manifested through changes in national income. As much as I agree with the "general proposition," I think that the analysis is limited because the fiscal effects on aggregate supply are neglected throughout. I except the possible brief reference (p. 257) where tax credits for business investment are mentioned and other occasional references to business incentives and public works—all tending to increase the productivity of the economic resources of the United States.

In the first part, analyzing the built-in stabilizers, Lewis proceeds to define his strategic "Implicit Federal Surplus" (federal receipts and expenditures calculated at present rates and programs for high employment levels) and to evaluate the fiscal effectiveness of the major federal tax-revenue and expenditure categories in relation to this "surplus." Lewis finds that the corporation income tax ranks high in this respect; the individual income tax shows a small yield flexibility; and, neglecting others, the excise taxes show a high degree of cyclical sensitivity. As one would expect, unemployment compensation payments "constitute by far the most important built-in stabilizer on the expenditure side ... " (p. 57). When turning to the "economic significance" of built-in stabilizers (for Lewis, expressed in their "operation when incomes decline ... " [p. 65]), the author devises a useful classification of direct and indirect stabilizers according to whether or not they affect personal income and thus consumption. It may be worth noting that Lewis' observations confirm the theory that the indirect stabilizers (e.g., corporate income tax) primarily affect retained corporate profits, which in themselves are an important built-in private stabilizer. After a partly digressive discussion of the liquidity effects of the lags in cash receipts, the author concludes that "the combined effect of the various stabilizers has been a substantial source of stability" and that they "are well timed to counteract contraction" but (as in theory recognized long ago) they "also retard recovery ... " (p. 89).

The second part of Lewis' study is related to the political economy of compensatory finance. This again demonstrates most of the well-known constraints and practical shortcomings of anticyclical fiscal policy. If the intertemporal comparisons made possible by this book could be supplemented with some international comparisons, much might be gained. Certainly it

would be found that also outside the United States "discretionary actions" have usually been coincidentally correct rather than in fact wisely "discretionary." But some experiences seem to be historically related uniquely to the United States in her present situation. I may mention two: the role that military expenditures play in relation to the budget and the over-all performance of the economy (see particularly pp. 142, 167, 192, 274) and, second, the overwhelming temptation of presidential candidates—as distinguished from party leaders in a parliamentary democracy—to include in their "platforms" statements related not only to the allocational and distributional aspects of government finance but to economic stabilization as well. Executive fiscal action might be less hampered if commitments of this sort were less specific.

There is a final question. Though the book informs one of many aspects of fiscal policy in the postwar United States, state finance is suspiciously neglected. Were the budgetary activities of all the states at all times fiscally neutral from 1948 to 1962?

HERBERT GEYER

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*Fiscal Adjustment and Economic Development: A Case Study of Nova Scotia.* By JOHN F. GRAHAM. Toronto: University of Toronto Press, 1963. Pp. xviii, 278. \$6.50.

This book is the first of a series of Atlantic Province Studies. It provides (a) a survey and analysis of the economic position of Nova Scotia which will be of particular interest to Canadians, and (b) a theory of intergovernmental fiscal relations which will be of general interest to students of public finance. The former portion is competent and informative. Nova Scotia is a low-income province because of poor natural resources and geographical location. But John Graham makes sensible suggestions for more effective use of resources in order to accelerate economic development. High priority is assigned to improvement of educational training at all levels.

The theory of intergovernmental fiscal relations from which this book starts was presented and debated more than a decade ago by James M. Buchanan and A. D. Scott. Buchanan advanced as a principle of fiscal equity that the net impact of government should be the same for similar persons, no matter where they lived. Implementation would call for intergovernmental transfers to offset movement of people and resources out of low-income areas. In a low-income province taxes would be higher and/or governmental services less adequate than in a high-income one. An outflow of resources would result unless transfers, especially by grants-in-aid, are made by government from high-income areas. Scott argued that such transfers would be distorting. They would interfere with migration of labor, especially labor of low productivity, to areas where productivity would be higher.

The Buchanan-Scott debate was concerned with federal-state (provincial) levels of government, and so also is Graham. But he explores application of the principle of fiscal equity at the provincial-municipal level. Some idea of

what would be required by the principle of fiscal equity is provided by the following quotation (pp. 42-43):

If federal grants to a province were sufficient to provide revenues which, along with its own provincial and municipal revenues, would enable it to provide about the same level of services as in other provinces, and if provincial and municipal taxes in the different provinces were similar in nature and levied at about the same rates, fiscal equity could be said to be approximated in that a resident of any province would receive about the same fiscal treatment as a similarly situated resident in any other province.

With respect to application of the principle between the nation and Nova Scotia, Graham believes that Scott's generalizations are applicable and Buchanan's inapplicable. Movement of labor and capital out of the province is needed, and increased large federal transfers to Nova Scotia would impede this out-migration, especially of labor. Reluctance to move is strong; the "unproductive," low-income worker is often content to live on unemployment benefits, rather than to migrate.

But application of the principle of fiscal equity *within* the province, Graham concludes, has merit. The provincial government should assume full financial and administrative responsibility for certain general services; it should enlarge its system of conditional and unconditional grants to local governments, the grants being framed so as to be equalizing. To the objection that such a program would be so centralizing as to endanger the vitality of local governments, Graham replies that, for poor local governments, autonomy is barren without the financial means to provide public services at levels comparable to those of wealthy localities.

This reviewer hopes that Graham will continue his research in the field of intergovernmental financial relations. His book contains a profusion of ideas which need more detailed analysis, particularly with respect to the overwhelming practical difficulties in applying fiscal equity at both the interprovincial and intraprovincial levels.

JAMES A. MAXWELL

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### International Economics

*The Natural Resource Content of United States Foreign Trade 1870-1955.*

By JAROSLAV VANEK. Cambridge: The Massachusetts Institute of Technology Press, 1963. Pp. xvi, 142. \$4.00.

This short but difficult book, a revision of a doctoral dissertation submitted to M.I.T., represents one of the all-too-rare attempts to join the pure theory of international trade with an empirical investigation. Jaroslav Vanek starts with the hypothesis that the "natural resource" content of U.S. net exports has declined substantially over the last century. His principal explanation, following the tradition of Ohlin, is that factor proportions in the United States have shifted away from natural resources toward labor and capital.

Roughly half the book is devoted to the confirmation of Vanek's hypothesis by the facts. "Resource products"—raw materials and crude foodstuffs—have accounted for a steadily declining share of U.S. exports and a generally rising proportion of U.S. imports over the period 1870-1955. In the last century, the United States was a large net exporter of resource products; by the 1950's it was a heavy net importer of resource products. Using input-output techniques, Vanek shows that the same general trend is observed even when the indirect component of resource products in foreign trade is taken into account—presumably laying to rest the possibility that U.S. exports continue to be concentrated on heavy resource-using products, only in a slightly more fabricated form. The downward trend is much less marked in the case of "renewable" resource products—indeed it is questionable whether any downward trend exists at all during the last 30 years—than it is for nonrenewable (mineral) products.

The remainder of the book provides a theoretical explanation for this marked change. Vanek emphasizes the change in factor proportions in the United States: a rapid growth in population and an even more rapid growth in capital, relative to the growth in labor forces and capital stocks abroad, shifted the comparative advantage of the United States away from resource-using products. The shift in factor proportions was further extended by an actual fall in the supply of nonrenewable resources as they were used up. Rapidly rising factor productivity in the mining industries offset partly—but not fully—the adverse shift in factor proportions; but the growth in agricultural productivity over the whole period lagged behind productivity growth in manufacturing, thus reinforcing the shift in factor proportions away from agriculture.

Vanek was hampered at every stage by the unavailability of data that would fit precisely his theoretical concepts—or by the failure of the theory to lend itself to easy empirical application. He was forced to compromise at many points and made a free and imaginative use of simplifying assumptions and proxy variables. His most important compromise was the substitution of the share in foreign trade of "resource products"—products "making direct use of natural resources"—for the natural resource (pure rent) content in all exports and imports. Practical data considerations made some such substitution necessary, but it leaves the reader with less than full confidence in the empirical results. The boundary between "crude foodstuffs" (resource products in Vanek's study) and "manufactured foodstuffs" (nonresource products) is an arbitrary one, and while the former have risen in relation to total U.S. imports over Vanek's period, the latter have fallen. Taking a somewhat different approach to the measurement of resource scarcity in the United States, Barnett and Morse found in *Scarcity and Growth* that advancing technology has kept pace with the "depletion" of natural resources. Their data show that net U.S. imports of minerals maintained a roughly constant share of U.S. consumption of raw materials in the period 1870-1940, although a sharp rise (due largely to petroleum) has occurred since then. Vanek's hypothesis would imply a rising share throughout.

The changing factor-proportion explanation for the shifting composition of U.S. trade can be reversed for the past decade and possibly for the next. High postwar rates of investment abroad, especially in the other developed countries, could be expected to shift U.S. comparative advantage back toward resource-using products, particularly in view of the very rapid postwar growth in U.S. agricultural productivity. Recent changes in the commodity composition of U.S. trade are therefore consistent with Vanek's thesis.

Vanek concludes his book with two provocative observations:

1. The Malthusian pressure of population against limited resources has not occurred because many countries remain underdeveloped, and hence their demand for natural resources remains low. Had these countries developed with Europe and America over the 19th century, the price of natural resources would have risen relative to other prices. Thus, argues Vanek, it is easy to understand the concern of many Westerners about the "population explosion" engendered by economic development; it will raise the relative price of resources and thereby hurt the present industrial countries.

2. Because owners of natural resources generally have short time horizons, social costs of natural resources exceed private costs. As a result, the underdeveloped countries, as net exporters of natural resources, have been transferring wealth to the industrial countries—just the opposite of what most would agree is the proper direction for transfer.

The first of these observations is at least questionable. True, more widespread and earlier industrialization would have raised greatly the demand for natural resources. But as Schmookler has suggested, invention and discovery are positively related to economic activity. More widespread industrialization would have greatly increased the number of inventors and innovators—the size of the research and development "industry"—and the greater talent devoted to finding cost-reducing techniques might have kept up with the rising demand for resources.

The second point, if correct, provides a persuasive rationale for the increasing practice of taxing production and export of natural resources.

Apart from the necessarily detailed discussion of data and assumptions which make difficult reading, the book unfortunately suffers from poor arrangement and occasional redundancy. And the empirical discussion has important deficiencies; practically nothing is said, for example, about changing factor proportions abroad; yet Vanek's explanation hinges on a change in factor proportions in the United States *relative* to those abroad. Moreover, such exclusive attention is paid to changing factor proportions in the United States that not even passing mention is given to many other factors that affected the observations, such as the inability of Europe and Japan to supply nonresource goods in the immediate postwar period. This inability biased postwar U.S. imports toward resource products. From 1953-55, when Vanek's observations end, to 1960-62, finished manufactures rose from 22 per cent to 36 per cent of total U.S. imports, while foodstuffs and crude materials together fell from 54 per cent to 44 per cent.

Individual parts are better than the book as a whole, however; Chapter 3,

for instance, offers an excellent theoretical discussion of the effects of growth on trade.

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*Price and Quantity Trends in the Foreign Trade of the United States.* Studies in International Economic Relations, No. 2. By ROBERT E. LIPSEY. Princeton: Princeton University Press for National Bureau of Economic Research, 1963. Pp. xviii, 487. \$10.00.

This volume, like many other NBER studies, performs a dual function: first, presentation and description of new basic data in the form of annual and quarterly price and quantity indexes for U.S. exports and imports (Chs. 3-6, Appendixes A-F); and secondly, initial exploitation of the analytical implications of the trends revealed by the new annual data (Chs. 1-2, Appendixes G-H). Each function merits separate attention.

The principal statistical product is a reasonably consistent series of price and quantity indexes and dollar values for total exports and imports and five major economic classes of exports and imports for the years 1879-1960. New indexes were actually constructed only for the years 1879-1923, for a decision was made to accept the official Department of Commerce series for the period after 1923 and to concentrate on the earlier years for which the data were less reliable, not broken down by commodity groups, and heavily over-weighted with primary products. This decision can be justified on grounds of reliability and cost-savings; however, it also implies acceptance of the existing classification system, which ignores differences between "productivity-oriented" and "natural-resource-oriented" finished manufactures and obscures thereby certain major trends in trade.

For the years 1879-1923, furthermore, annual Fisher price and quantity indexes and dollar values are published for 22 export classes and 23 import classes as well as for selected intermediate and minor classes. In addition, there are presented for 12 selected major classes annual Paasche and Laspeyres price indexes, quarterly Fisher price and quantity indexes, and quarterly dollar values. All the indexes were constructed in four segments: 1913-23, 1899-1913, 1889-99, and 1879-89, using the final year of each as base so as to obtain the benefits of improving statistical coverage. Interpolative quarterly series are included to fill the gap between 1923 and 1929 (when the quarterly Department of Commerce series begin).

The description of the new indexes reflects the meticulous standards which we have come to associate with the NBER. There is detailed consideration of such questions as the measurement of sampling errors, measures of coverage and changes in coverage, the quality of the underlying trade data, and the problems of weighting bias. Particularly noteworthy is the decision to publish some underlying Paasche and Laspeyres indexes. Comparison of these series not only serves the technical purpose of facilitating judgment as to whether apparent differences between American and foreign indexes "rep-

resent real differences in behavior or only the results of comparing dissimilar types of index number" (p. 83), but also serves the broader purpose of helping to eliminate such vestiges of elasticity pessimism as may remain among economists.

Turning to the second or analytical function, one is impressed by new evidence of the *cumulative* additions to knowledge being provided by the NBER statistical studies. Robert Lipsey, for example, combines his price data with Kendrick's indexes of output per man-hour and total factor productivity to construct two series of the single factorial terms of trade; these exhibit some fourfold improvement over the period covered. More significantly, he combines his quantity data with Shaw's commodity output and Kuznets' GNP series to demonstrate that the U.S. trade-output ratio has now returned to the level of the 1880's after a long period of decline. Lipsey offers a careful analysis of the various factors that have contributed to shifts in this ratio. But the present reviewer might have been tempted to push the interpretation of the data a step further and advance the hypothesis that, for the United States, declines in the trade-output ratio occur during periods of rapid economic growth relative to the rest of the world, while increases in this ratio take place when the comparative U.S. growth rate is low.

The author separates his analysis of trends in prices and the terms of trade from his treatment of trends in values and quantities. This reviewer found the latter much more informative and original. Not only does the investigation of values and quantities include a thoughtful exposition of the theoretical implications of changes in the trade-output ratio, but it also provides an excellent analysis of the role of agriculture and farm exports in nineteenth-century growth and a highly imaginative discussion of differences between long-term and short-term patterns of price-quantity behavior. Shifts in supply functions are treated as being essentially long-term and yielding negative price-quantity relationships, while changes in demand functions are considered mainly short-term and are associated with positive price-quantity relationships.

The particular emphasis used by Lipsey in his interpretation of the price-trend data is best revealed in his own words:

Two widely held beliefs regarding net barter terms of trade find no confirmation in the data for the United States. One is that there has been a substantial long-term improvement in the terms of trade of developed countries, including the United States; the other, that there has been a significant long-term deterioration in the terms of trade of primary as compared to manufactured products" (p. 76).

This interpretation seems to be based on a common misconception of the nature of the terms-of-trade hypotheses advanced in the literature (a misconception shared by most proponents of such hypotheses). As persuasively if not conclusively demonstrated by Nurkse in the case of low-income economies, the terms-of-trade hypotheses should be interpreted as *ex ante* rather than *ex post* arguments because the law of supply and demand must be presumed to work in the long run: Changes in the terms of trade, by inducing

"shifts in production and in the distribution of resources, are apt to be washed out in the long run"—with the result that, for economies which are characterized by a limited capacity to transform, what may be a "terms-of-trade problem in the medium run" becomes a "development problem in the long run."<sup>1</sup>

In the light of the significant degree of transformation capacity which has characterized the U.S. economy, the chances of terms-of-trade movements being washed out in the long run would appear to be considerably larger than in the case of underdeveloped countries. Analytical emphasis should have been placed, therefore, not on the inherently truistic statistical developments, but on the mechanism of adjustment through which the secular constancy of the terms of trade was achieved. Lipsey's own data and analytical subconclusions are in themselves quite useful in explaining the nature of the adjustment mechanism for the United States. There was, for example, the growing industrial transformation of the economy which is reflected in the fact that "the behavior of U.S. terms of trade, independent of or even inverse to that of Europe before 1920, became quite similar after that date" (pp. 14-15). Furthermore, the conflicting trends in trade between agricultural and manufactured products are revealing of the unique quality lent to developments by the considerable diversity of the U.S. economy itself; between the 1880's and the 1950's there simultaneously took place a "great decline in the ratio of export prices to import prices for manufactured goods" and a "large increase in the ratio of export prices to import prices among agricultural products" (p. 20).

Beyond this difference regarding the interpretation that should be given to terms-of-trade hypotheses, this reviewer found Lipsey's analysis to be sound, imaginative, and quite stimulating to his own thinking. The underlying statistical series themselves, of course, constitute an indispensable tool in the kit of any economist who wishes to deal seriously with U.S. trade problems.

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<sup>1</sup> Ragnar Nurkse, *Equilibrium and Growth in the World Economy*, Cambridge 1961, p. 334.

*The Communist Foreign Trade System.* By FREDERICK L. PRYOR. Cambridge: Massachusetts Institute of Technology Press, 1963. Pp. 296. \$7.50.

As the author notes in the preface to this study, "academic research on the Communist bloc presents particular difficulties. . . ." In his case, these "difficulties" included six months of imprisonment in East Berlin. The academic initiative so harshly rewarded included a series of interviews with East German foreign trade officials. The material provided by these interviews helps to make this informative analysis of the Communist foreign trade system a unique source of information and insight into the complicated subject with which it deals. Frederick Pryor's arrest by the East German regime is

highly ironic, since he is less critical of Communist foreign trade policies than are many other Western observers. Much of this book is a vigorous and detailed criticism of bloc foreign trade practices and theory which amply documents past and present inefficiencies. Nevertheless, Pryor presents evidence of increasing information, better theory and gradually improving performance and his over-all judgment is that "the development of the Communist foreign trade system has been impressive."

Throughout the study, Pryor stresses the theoretical inadequacy of bloc analyses of foreign trade problems, and the significance of this shortcoming for policy. Thus: "The split between recognized need [for more, and more profitable, trade] and relative neglect [of foreign trade] lies . . . in the fact that foreign trade planners failed to demonstrate concretely to the top policy-makers the actual trade potentialities" (p. 130). Or, in discussing CEMA's tasks for the future, "First of all, Bloc economists must develop a theory of foreign trade and economic integration and must devise a set of rational criteria to decide the most satisfactory division of labour between the individual nations" (p. 228). Pryor's samples of bloc foreign trade theory, both in this book and in his *Soviet Studies* article on this subject,<sup>1</sup> are appalling examples of theoretical flabbiness resulting, in part, from trying to squeeze theory into a Marxist straightjacket. But Pryor perhaps overemphasizes the relative significance of faulty theory compared to the institutional and political barriers to a more optimal use of bloc resources. Moreover, there is far from complete agreement even in the West on the relationship of trade and development. While the rigid application of a Stalinist model of development in each nation of Eastern Europe clearly leads to gross absurdities, what is the optimal degree of specialization? Pryor concludes that: "The concern for large-scale industrialization in every Bloc country, although often buttressed by some incorrect autarkist theories, is certainly valid" (p. 229). I think he would agree that an adequate theoretical formulation of "this concern for large-scale industrialization" would not be easily come by.

Pryor, of course, recognizes the difficulty of building an adequate theory of foreign trade when the elementary construction material—prices, costs, exchange rates, etc.—is shoddy and unreliable. An inconvertible world where prices are arbitrary, costs unclear, and exchange rates misleading is not a world where an optimal trade pattern is likely to be arrived at—good theory or no. Where even the struggle for feasibility is fierce, the cause of optimality must inevitably be a lost one. And Pryor's documentation of the battle to fit foreign trade into the planning process is one of the major virtues of this volume. However, he attempts to show that the significance of one institutional barrier often cited by Western observers—the barrier of bilateralism—has been greatly exaggerated. Such a conclusion is a novel one and, if sustained, would call for the withdrawal of one of the major criticisms of the Communist foreign trade system.

Pryor's major piece of evidence is an index of multilateralism—relating the absolute value of bilateral imbalances to total trade turnover—which

<sup>1</sup> "Foreign Trade Theory in the Communist Bloc," *Soviet Studies*, July 1962.

shows little difference between the Common Market and the European Soviet bloc in degree of multilateralism. As Pryor recognizes, this index is difficult to use for comparative purposes: identical indices of multilateralism for the EEC and CEMA could well reflect vastly different degrees of departure from a free trade equilibrium. Moreover, Michael Michaely's September 1962 article in this journal on "Multilateral Balancing in International Trade," which Pryor cites but was not able to take fully into account, shows that of the 65 nations in his study, the five Soviet bloc nations have the lowest degrees of multilateralism. And in each case the degree of multilateralism sharply drops between 1938 (or 1948) and 1958. Before accepting Pryor's conclusions, these results would have to be explained.

More fundamentally, the benefits of multilateralism are not to be found in mechanically totting up bilateral imbalances. What is significant is the freedom a multilateral system allows to use credits with one country or group of countries to offset debits with others. Even a system of huge bilateral imbalances where no multilateral clearing is permitted would approximate the optimal multilateral solution only by the sheerest chance. Pryor notes that some intrabloc arrangements have been made for shifting trade balances but also agrees that these arrangements have been clumsy and difficult to work out. The contention that the present clearing bank has not been given greater powers because the need for multilateralism "did not impress the Bloc policymakers enough to make any radical changes" is unconvincing. Isn't it more likely that the practical and theoretical difficulties that would arise from attempting to establish a multilateral system within a bloc of planned economies are simply too knotty to be easily overcome?

Theoretical and institutional obstacles to a rational bloc division of labor are accompanied, as in the West, by political obstacles. Pryor's remarks on these problems are brief but sensible. Hitherto docile Rumania's sudden and successful display of intransigence last spring suggests that they are no less pronounced in Eastern than in Western Europe. The Rumanian episode also focuses attention once again on the issue of Soviet economic dominance. Could Rumania really turn to the West if CEMA continued to press forward vigorously with its original plans of economic integration? By insisting on developing her own industrial base is not Rumania tightening the bonds which tie her to the USSR? One issue which quickly arises from speculations of this sort is that of alleged Soviet exploitation of her East European allies. Pryor demonstrates the statistical difficulties involved in documenting this contention. In particular, he points out that whereas the terms of trade of the USSR with other bloc nations were more favorable than those with the West, this was also true for every other bloc country. This does not mean that there is no discrimination, rather that there may be mutual discrimination, the net effect of which is difficult to estimate. Thus, Pryor finds that trade bargaining among socialist countries is no less spirited than among capitalist countries and concludes that "Although Bloc trade officials played sweet tunes of proletarian internationalism in public, they practiced price discrimination in private."

Pryor's study ranges over a wide variety of other issues—East-West trade, CEMA's operations, foreign-trade criteria, intrabloc price setting, foreign-trade monopolies in Communist countries—and many others. In general, his discussion of these issues is informed and enlightening. However, he is perhaps too conscientious in trying to mine the available statistics for material to support his conclusions. Readers of this study will be well advised to check carefully the underpinnings of Pryor's tabular and statistical material to assess for themselves its reliability and significance. But if this study suggests more than it proves, this is all that can be reasonably expected, given the information available and the complexity of the problems discussed. Moreover, the subject of Communist foreign trade seems to lend itself to heated partisan controversy; this book, however, is a serious and scholarly study, essentially unbiased in tone and approach. For students of Communist foreign trade, this is an indispensable volume.

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*Trade Blocs and Common Markets.* By SIDNEY DELL. New York: Alfred A. Knopf, 1963. Pp. xviii, 375, x. \$5.95.

It has been rather common to regard the recent emergence of the European Common Market as a revolutionary but wholly beneficial development in world affairs. The study by Sidney Dell, however, very forcibly amends this impression by an appeal to common logic and by use of uncommonly concise historical analysis. Here it is shown that the formation of EEC is yet another reaction to world economic trends where economic groupings are constantly emerging to correct excessive economic forces of other areas. The world economy is viewed almost as an organism of regional growth and contraction, of conflict and collaboration, and of constant regrouping and reorganizing.

The challenge is clearly made in the book that free trade is hardly the basic aim of regional groupings of nations. More exactly, and in correct historical perspective, the aim of most regional groupings, especially those of common markets, is seen as an attempt to establish preserves of economic endeavor shielded by protective barriers.

The main objective of this book is to examine origins of the numerous common markets emerging in different parts of the world and, incidentally, to reveal the patterns of national and regional interaction. Recent regional developments both in Europe and in other continents are comprehensively examined. It has not been the author's purpose to present the free trade case but rather to set down instances of its denial and of lip service paid to it.

The chief conclusions of the book are that the newest element in regional evolution is the merging of industrial giants on a geographical basis and a grouping of new nations which were former dependencies of the industrial powers. For the present the EEC has emerged as the strongest and most advanced of the regional groupings. As a reaction the regional groupings of underdeveloped countries appear as an eventual and even necessary step. The

EEC is seen both as a rival and ally of the United States. Stress is given to the view that common markets do not merely unite; they divide as well.

Although economic advantage is seen as the motive for historical development, the author finds little economic advantage to the new groupings of nations. Economies of mass production have been largely realized in the industrial countries, and there are no strong economic ties among the underdeveloped countries. Nevertheless, attention is directed towards the intensification of competition, the possibility of a withdrawal of government in the field of regulation, and a weakening of the labor unions in the new integrated complex. The consequence is a strengthening of the larger business concerns, a prospect which has inspired fears in labor circles in the United Kingdom and elsewhere.

The case of the underdeveloped countries is strongly and objectively put by Dell. Trade restrictions appear as a means for regulating the growth of underdeveloped countries. The perversity of international economic policy is such that restrictions are oddly combined with increased amounts of foreign aid. The underdeveloped countries in their present state of fragmentation have little hope of economic viability and little power in bargaining for reductions in tariffs against their commodities.

In some respects Dell has gone too far with his method of analysis. In his search for origins of trade groupings he uses the exclusive test of free trade versus protectionism. He sees no middle ground between the extremes of free trade and protectionism. He does not enlighten us on how we could measure a free trade or a protectionist economic policy. The economic union of the states of the United States and the confederation of the German states during the nineteenth century are not, for this reviewer, completely analogous to the current developments in the common market. German policy during the 1930's and parallel movements in Japan with its aim of a "greater co-prosperity sphere" are drawn too heavily in economic terms. Reliance on the forces of economic materialism makes Dell see too much in the economic factors involved in colonialism, whether it be the old colonialism of the seventeenth and eighteenth centuries, the imperialistic brand of colonialism of the nineteenth century, or neocolonialism in the twentieth century as seen in Yankee imperialism, international economic capitalism, or the more recent and more widespread types of Soviet colonialism.

In stressing tariffs and protectionism other policies and activities of government tend to be overlooked. Tax policy and banking policy as well as military and social policies tend to be underplayed as contributing factors. For example, in considering U.S. tariff history Dell overlooks the instances where tariffs were necessitated by the impossibility of acquiring revenue through income taxes and other devices. This also happens to be a difficulty for governments in underdeveloped countries searching for tax revenues at the present time.

On the whole this book is a refreshing review of recent regional economic developments on a broad plane of analysis. The relevance of EEC in an economic world of transition is thoroughly examined. The author does not ignore

the conflicts within the Community or those created outside of the Community. Attention to the problems of underdeveloped countries gives a balance of viewpoint absent in many other works dealing with common markets or *the* Common Market. This book is worthy of close reading by all those interested in international economic problems.

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**Business Finance; Investment and Security Markets; Insurance**

*A Study of Mutual Funds.* By I. FRIEND, F. E. BROWN, E. S. HERMAN, AND D. VICKERS. Prepared for the Securities and Exchange Commission by the Securities Research Unit, Wharton School of Finance and Commerce, University of Pennsylvania. Report of the House Committee on Interstate and Foreign Commerce, 87th Congress, 2nd session. Washington: Superintendent of Documents, 1962. Pp. xxxiii, 595.

One may sardonically observe that the "Soaring Sixties" is thus far an apt description of economic developments for which the term was coined. However, it is good strategy to put a nonearning asset to some profitable use. The term, "Soaring Sixties," can justifiably be used to describe the number of investigations and reports that have been produced on the operations of the financial system in the United States.

Despite their plethora, there have been no major indictments emanating from these studies. These reports may therefore bewilder the owners and managers of institutions comprising the financial system, who understandably wonder why all these studies were necessary in the absence of any major difficulties. On the other hand, one might observe that financial institutions—among the most regulated enterprises in the United States—have often had the regulations pertaining to their operations dictated by the aftermath of emergencies or crises. As such, the patchwork of regulation that has grown up has often been retrospective, attempting to cure past ills rather than prospective.

The present rash of reports and recommendations has one virtue at least. The reports are the products of an era of no demonstrated flagrant abuses in the financial community and no critical shortcomings in the performance of financial institutions. In this reviewer's judgment there is a virtue to a continuous surveillance and responsible investigation and reporting of findings on the operations of the financial system. For the consequences of the performance of this function, assuming it is responsibly discharged, will be to provide for an evolving set of regulations which corrects abuses before they become excessive and dangerous to the stability of the economy as well as ensuring opportunities for adjusting the regulations, either statutory or administrative, to the present and foreseeable needs and requirements of the economy.

In August, 1962, the Chairman of the Securities and Exchange Commission transmitted *A Study of Mutual Funds* to the Chairman of the House Committee on Interstate and Foreign Commerce, under whose imprint it was

published. The study was undertaken pursuant to Section 14B of the Investment Company Act of 1940, which authorizes the Commission from time to time "to make a study and investigation of the effects of size on the investment policy of investment companies and on security markets, on concentration of control of wealth and industry and on companies in which investment companies are interested. . . ."

Conforming with this authority, the Commission engaged the Wharton School to make a fact-finding survey of "certain aspects and practices of open-end investment companies or mutual funds." The *Study* was produced by four members of the faculty of the Wharton School of Finance and Commerce: Irwin Friend, F. E. Brown, Edward S. Herman, and Douglas Vickers. The completed manuscript is a massive document consisting of 595 small-print pages including 273 tables, exclusive of subtables distributed among these numbered tables. This publication represents the first extensive description and analysis of the growth of the mutual fund industry since the earlier SEC *Report on Investment Trust and Investment Companies* published from 1939 through 1942.

The coverage of the report is reflected in the titles of its chapters: Chapter 1 is a "Summary and Conclusion"; Chapter 2, the "Structure and Control of Open-End Investment Companies"; Chapter 3, "Mutual Fund Growth 1952-1958"; Chapter 4, "Investment Policy"; Chapter 5, "Investment Company Performance"; Chapter 6, "Impact of Investment Funds on the Stock Market"; Chapter 7, "Open-End Investment Companies and Portfolio Company Control"; Chapter 8, "Investment Advisors of Open-End Investment Companies."

Out of the welter of material included in this study, five general topics will be singled out for comment. These are (1) a description of the mutual fund industry; (2) the effect of size on concentration of control within the industry and the degree of control over portfolio companies by mutual funds; (3) the impact of the industry on the stock market; (4) the performance of the mutual fund industry; and finally, (5) potential conflicts of interest that may arise in the mutual fund business.

(1) Students of finance will be in debt to the authors of the Wharton School *Study* for a long time. The extensive data relating to the anatomy of the mutual fund industry, its structure, growth, and control are exceedingly informative. The material on the size characteristics of firms in the industry, the fund inflows, and share turnover supply us with a wealth of detail. Some of the most useful information is the detailed examination of investment policy in Chapter IV. Portfolio distribution classified in a number of ways, the mechanism of portfolio transactions, and data on portfolio turnover all serve as a prelude to a discussion of the response of mutual funds as investors to movements in the prices of securities. This material should enrich the literature of portfolio policy and asset choices which is rapidly becoming an important subject in monetary economics.

(2) The authors of the Investment Company Act were clearly motivated by two long-standing concerns of public policy in this country. The first of

these is the almost ingrained fear of size and concentration which is a pervasive element in public policy prescription not only in the finance business but in the whole area of competitive policy for industry. However, this fear of size is compounded by a second concern which has a long history in the United States, namely, concentration of control over economic activity by financial firms through their holdings of corporate securities. Since mutual funds hold largely equities, hence potential voting power over managements of (portfolio) companies, it is easy to perceive the thread of concern in the 1940 Act that concentration of control in the mutual fund industry provides a possible avenue for concentration of control over all economic activity in this country.

On the first of these issues, the authors report "One general conclusion . . . is that the main problems affecting mutual funds do not seem to relate to the size of individual funds or companies but rather to the industry as a whole. This is not to say that size of individual funds may not be a problem at some time in the future but simply that there is little evidence it is one at the present or that it is any more of a problem currently than it had been earlier" (p. 3; see also p. 78). The authors then go on to say "The most important current problems appear to be those which involve potential conflicts of interest between fund management and shareowners, the possible absence of arm's-length bargaining between fund management and investment advisors, and the impact of fund growth and stock purchases on stock prices. But these problems are not related to company size except in the sense that questions arise about the distribution between fund shareowners and investment advisors of benefits resulting from large-scale operations" (pp. 3-4).

With respect to the possibility of abuse arising from control over portfolio companies, the Wharton School *Study* concludes: "Despite the growth of large holdings of mutual funds, outright control of portfolio companies by these organizations is a rarity and is confined mainly to small companies as envisaged by the authors of the act of 1940. Mutual funds with large holdings undoubtedly exercise varying degrees of influence with portfolio companies, but as of late 1958 neither the extent nor character of their influence appear to be such as to warrant serious concern" (p. 26).

(3) In attempting to assess the impact of mutual fund activity on stock prices, the *Study* attempted to ascertain the relationship during 1953-58 between fund purchases and the level of market and individual stock prices over varying periods of time. The authors show a wholesome respect for the difficulties of this task (pp. 360-63).

Despite the general inconclusiveness of their tests, the Wharton group concludes that "it seems likely that the growth in the funds' net purchases of common stock has stimulated stock prices markedly during the past decade or so, during which the industry has expanded enormously" (pp. 21, 359).

In buttressing this argument, which is based partly on their statistical analysis and partly on a priori reasoning, the authors appear to argue that a substantial portion of the money invested in the funds and hence in the equity markets is secured from saving that is not traditionally channeled into the

stock market. Until and unless we have better information on household savings habits the findings of the *Study* will continue to be the proper subject of debate.<sup>1</sup> In my opinion the authors have suggested some pieces of evidence which add to our knowledge of this subject, although the evidence is hardly conclusive.

(4) The aforementioned findings were received with relative calm or complete indifference. However, the assessment of the mutual funds' performance created a furor. The *Study* concluded: "For the 5¾ years covered . . . , the Standard & Poor's Composite Common Stock Index was definitely superior to the average performance of the funds, but the disparity can be explained by the portfolio structure of the funds; i.e., the division of their portfolios among common stock, preferred stock, corporate bonds, government securities and other assets. When adjustments are made for this composition, the average performance by the funds did not differ appreciably from what would have been achieved by an unmanaged portfolio with the same division among asset types. About half the funds performed better, half worse, than such an unmanaged portfolio" (pp. 17-18).

The performance of a hypothetical "unmanaged portfolio" does not reflect the costs and resources required to reproduce every change in composition reflected in the market averages. In addition the authors say "since perhaps the major function effectively served by mutual funds is the provision of diversification, a feature particularly important to small investors who can ill afford large risks, it is important to point out that such an investor who attempted to achieve a comparable degree of diversification by direct purchases might incur acquisition costs in excess of the 8 per cent charge typically imposed by the funds."

Testing the performance of the investment company industry is a complex and difficult task (pp. 289-94). The Wharton group proceeds on the assumption that performance is properly measured by the results realized from holding a portfolio over time. Because they are interested in performance and not investor experience (pp. 289-90), they sought out an index of the securities market as a whole to enable them to make comparisons of the results of investment companies to external securities market standards (pp. 293 ff.).

They define a performance index as the ratio of end-of-year net assets, dividends, and distributed profits per share to net assets per share at the beginning of the year. This measure is then applied annually to a population of 152 mutual funds. To arrive at a cumulative performance, an index was calculated. The resulting performance figure shows that the growth of the 152 investment funds was 96.7 per cent from 1953 to 1956. This performance index was then compared with the cumulative growth rate of a weighted average of the various Standard & Poor's security indices. The weights were derived from the percentage distribution of assets held by the mutual funds. This weighted average had grown by 98.2 per cent during the 5¾ years.

<sup>1</sup> In a subsequent study the authors offer some corroborative evidence for the position that a substantial portion of money invested in funds would not have otherwise been channeled into the stock market. See *SEC Report of Special Study of Securities Markets*, Part IV, Ch. 11, Appendix XI-A-II, p. 358.

Farrar has put forth an alternative test of performance.<sup>2</sup> He has shown that if the desirability of portfolios is judged according to their rate of return and risk (price variance) then the various classes of mutual funds hold nearly optimum portfolios. Thus one can optimize a portfolio *at a given moment of time* in the light of the *past* average returns and variance of the securities irrespective of whether they were held or were just acquired.

It is clear that the Wharton study rejects this definition of performance. In this reviewer's judgment these views need not be alternatives but complementary. For an investor should be interested both in what the fund accomplished in the past and in what it is currently holding.

(5) The report points up the separation of ownership and control in the mutual fund industry. Since this phenomenon is now recognized as a fact of corporate economic life, one is led to the conclusion that mutual funds are, in this respect, like most other corporations. And mutual fund shareholders like other shareholders can evince their displeasure with managers by parting with their shares.

Further aspects of the structure of the industry, the nature of competition and possible conflicts of interest between the fund, its advisors, and its shareholders are discussed in the final chapter. The fees that investment advisory concerns charge mutual funds tend to be higher than those charged by the same advisors to other clients. The Wharton group argues that owing to economies of scale, the expenses involved in advising mutual funds were less than those incurred in advising other clients.

The report further finds that mutual funds pay their advisors an annual fee of approximately  $\frac{1}{2}$  per cent of the annual average of net assets. Since sales of funds' shares have been a means of expanding their assets and such increases produce increases in the absolute fees collected by the advisor, a prospective conflict of interest arises between a mutual fund's shareholders and the fund's investment advisor with respect to selling efforts.

Other possible conflicts of interest were also noted. Among those advisors interviewed approximately 40 per cent were affiliated with brokerage houses and 95 per cent with underwriting firms. The implication is that portfolio churning may be profitable for the advisors in their major business efforts rather than in the discharge of their advisory function for the funds.

On the assumption that entry is free and competition among funds is present, it is not immediately clear what action is required by the findings in this section. On the matter of entry, it is noteworthy that 37 new firms came into being over the period 1952-58 (p. 77). As to the existence of competition within the industry, the *Study's* summary concludes:

Competition among mutual funds has provided investors with a wide range of choice as to fund type, loading charge, etc. It has stimulated efforts to achieve outstanding investment performance since fund sales, at least to some extent, have been positively correlated with fund performance. Competition in promoting sales directly as well as indirectly through winning and stimulating dealer sales efforts with respect to par-

<sup>2</sup> D. E. Farrar, *The Investment Decision Under Uncertainty*, Englewood Cliffs, N.J., 1962, *passim*.

ticular funds has been an important characteristic of the mutual fund industry. Thus competition in the mutual fund business has assumed the principal nonprice forms—variety of product, product quality, and sales promotion—but for the most part it has involved price, at the investor level, only insofar as size of sales charge has become a competition factor as between the principal funds in the industry and the no-load companies, which still constitute a relatively small fringe of the business. Management fee rates and the allocation of brokerage business have not as yet elicited important competitive responses for a major part of the industry (p. 35).

Apparently the presence of nonprice competition is disturbing to the authors of the report. But in the absence of collusion it is difficult to argue vigorously for changes in the present regulations in this industry. In addition to the struggle for the saver's dollar that is at work within the mutual fund industry, other forms of competition also exist. Presumably a saver interested in acquiring variable priced assets may do so on his own.

It should be mentioned, however, that an alternative interpretation of the report's findings is its expressed concern over possible monopoly power based on lack of knowledge (including alternative sources) available to savers.

The *Study* should serve to stimulate changes in the mutual fund industry. This reviewer's interpretation is that the *Study* suggests that more efficient operations should enable firms to compete effectively with existing practices. Alternatively, the description of the industry should serve as an open invitation to either new firms or new practices, which should reduce costs of operation and increase returns to savers. If the analysis of the structure and efficiency of the industry as outlined in the *Study* is correct, one likely outcome will be alteration of practices or increases in the number of firms, either of which should redound to the public's advantage.

In conclusion, while this reviewer has suggested possible points of difference with the findings of the report, it is his studied opinion that the Wharton School group has provided the raw materials for continuous studies in the mutual fund industry. The concern over some current practices in the industry should heighten public interest in the conduct of the affairs of mutual fund companies. The public is in debt to this group for a careful and searching scrutiny. Undoubtedly some conclusions will be discarded as a result of a careful and scholarly examination of the findings by others. However, when the smoke of battle dies down, the industry will probably be the better for the report—a view which is undoubtedly not well received by the industry at present.

ELI SHAPIRO

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*Trends in the Distribution of Stock Ownership.* By E. B. Cox. Philadelphia: University of Pennsylvania Press, 1963. Pp. xiii, 221. \$6.00.

Growth in the distribution of stock ownership has received widespread publicity in recent years. It has led some to "the heartening conclusion" that

"a quiet economic revolution is reshaping America" and that "America can be a society composed of many millions of private capitalists." These are comments made by Keith Funston in his introduction to the 1959 census of shareowners by the New York Stock Exchange and the idea has gained wide acceptance. Therefore, it is startling to read in Professor Cox's "Trends in the Distribution of Stock Ownership" that in 1959 one out of eight adults owned stock, or approximately the same proportion of the population as in 1930! The 1962 Stock Exchange census is not included in Professor Cox's study, but doubt is not dispelled by a further rise in ownership to one out of six adults. That survey was made early in 1962, since which time there have been signs of a general public withdrawal from the stock market following the sharp decline in stock prices in May and June of that year.

It is a fair guess, therefore, that stock ownership is still not significantly broader than it was at the height of the stock market boom in 1929. In the interim, the number of stockholders dropped sharply and then recovered. From ten million in 1930, the number declined to about five million in 1950. It is the growth since 1950 which appears so impressive until it is related to the previous decline and to the growth in population. Viewed in this light, the expansion in the number of stockholders to 12.5 million in 1959 and 17 million in 1962 is probably at least as much a cyclical movement as a resumption of the long term growth trend that raised the number from 1 million in 1900 to 10 million in 1930. It is too bad that so little has been done in this area to separate the cyclical variations from the trend.

Cyclical variations in this context need mean nothing more specific than the ebb and flow of public interest in common stocks. From the earliest experiences in the Mississippi and South Sea bubbles, there have been recurrences of intense public interest in stocks by hordes of people who are not ordinarily investors in such securities. They are people who are attracted not for reasons of safety or income, but in the hope of quick profit. When profits cease to accrue—as happens sooner or later—they lose interest and return to the safety and income of bonds and savings accounts. How much of the recent growth in the number of stockholders may have been due to speculation by newcomers who leave the market when prices drop is a moot point. Signs of public withdrawal from the market since the price decline in the middle of 1962, however, are numerous. They include brokerage reports of reduced activity on the part of the small "investor," the sharp drop in sales of mutual funds accompanied by a record rate of redemptions, and the drastic decline in the sale of new issues of common stock. Coupled with these are a sharp rise in time deposits and a flood of advertising for savings accounts.

The usefulness of Cox's work is primarily the historical perspective in which he has placed the data on stock ownership. He has done this efficiently, tracing the studies from the earliest ones by Warshaw in 1924 through Berle and Means' extension of those figures and the TNEC report in the late 1930's. Then he takes up the postwar period and the more extensive studies, mostly by the New York Stock Exchange. He recognizes the vast difficulties in obtaining reliable information when ownership is so often lodged in that faceless creature, the nominee. But his effort to expand our knowledge by means

of a questionnaire sent to some 80 companies is not very helpful. Nor do the isolated company surveys of their own stockholders add much. For determining the numbers and characteristics of stockholders the broad sampling techniques, such as those used in the Stock Exchange surveys, will probably continue to be the main source of this kind of information.

What is not to be found in the material which Cox reviews is a thorough analysis of the trends in stock ownership as between individuals and institutions and the trends among the institutions themselves. (The Stock Exchange specifically excludes from its surveys the holdings of all nonindividual stockholders.) Those who work closely with securities are acutely aware of the enormous impact on common stocks from the phenomenal growth of pension funds and their tendency to put increasing portions of their assets into stocks during the 1950's. Information on the holdings of pension funds, mutual funds, insurance companies and other institutional investors should be more valuable than data showing that individuals who own stock have better than average incomes, education, and liquid assets; that they tend to be older persons, and that they come from the professional, managerial, or entrepreneurial occupations.

Finally, there is a set of data which should be integrated into any study on stock ownership. These are the little-publicized figures of the SEC on the dollar amounts of stock added to or subtracted from their holdings by individuals. They appear quarterly under the heading "Savings by Individuals in the United States." For several years now, these figures have provided a sharp contrast to the rapid growth in the number of stockholders, for they show a persistent and growing tendency for individuals as a group to dispose of stock. Beginning with a net reduction of \$.3 billion in 1958, the reduction was \$.9 billion in 1959, \$1.9 billion in 1960, \$2.2 billion in 1961 and \$3.5 billion in 1962. For each of the first two quarters of 1963 the reduction was \$.8 billion, or very nearly the same rate as in 1962. Presumably the few are giving up stock even faster than the many are acquiring it, the difference being made up by institutions. Surely these figures bear some import and merit consideration in a study of stock ownership.

SIMEON HUTNER

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*The Life Insurance Enterprise, 1885-1910: A Study in the Limits of Corporate Power.* By MORTON KELLER. Cambridge: Belknap Press of Harvard University Press, 1963. Pp. x, 338. \$7.25.

Scholars familiar with life insurance history have long been impressed by the importance of the Armstrong investigation of 1905 in bringing about a much needed reform in the operation of American life companies, especially the five industry giants domiciled in the New York City area. The author of this well-written and well-documented monograph does not question the significance of the Armstrong investigation for the subsequent history of the industry. He does, however, point out that the managements of the big five were under growing pressure from internal forces prior to 1905 to reform their

policies and practices. Yet progress in this direction was slow, with the result that "the Armstrong investigation focused the concern of an aroused community, exposed the already shaky structure of the societies to public view, and stimulated a profound evolution in their corporate being" (p. 292).

This book unquestionably puts the events affecting American life insurance during the critical period from the mid-1880's to 1910 into clearer historical perspective. As a case study, it also substantiates the conclusion of the author that to the traditional sum of external checks on corporate power in a free society, as outlined by Galbraith and others, should be added "the complex but significant factor of internal restraints: the problems and consequences inherent in the processes of institutional growth and maturity."

HAROLD F. WILLIAMSON

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### **Business Organization; Managerial Economics; Marketing; Accounting**

*Soviet Marketing—Distribution in a Controlled Economy.* By MARSHALL I. GOLDMAN. New York: The Free Press of Glencoe, 1963. Pp. vii, 229. \$5.95.

This volume provides an up-to-date account of institutional and organizational aspects of the distribution of consumers' goods in the USSR. Successive chapters describe: (1) the distribution networks and the administrative apparatus under which each operates; (2) planning and pricing procedures, given the allocation of resources between consumption and nonconsumption; and (3) the system of financial controls exercised by the state, and especially by the State Bank, over the marketing operation. No such recent account is available elsewhere in English, and here lies a primary value of the study.

How well, how efficiently, does the distribution system work? In sections of the institutional chapters and in one separate chapter, Professor Goldman considers the responsiveness of the system to consumers' preferences and the response of the system to the tests of performance and incentive in force, and he finds notable inadequacies and inefficiencies in both respects. This discussion exemplifies for a particular sector the kinds of problems of economic organization that other students of the Soviet economy have pointed to more generally.

A concluding chapter inquires into the role of marketing in a controlled economy. Goldman emphasizes that the USSR has now reached a stage of development at which the level of real income is sufficiently high to yield familiar problems in the production and distribution of consumers' goods, quite apart from the Soviet institutions involved. Because incomes have risen, choices proliferate and demand becomes a variable that is both influential and subject to influence. Everything produced is not automatically sold. The marketing function expands from the elementary activities of packaging, storing, and transporting to research and merchandising. Thus, Goldman observes recent complaints in the Soviet press about excessive inventor-

ies and the occurrence of various merchandising developments: price reductions on goods in excess supply; the expansion of advertising and product differentiation; the introduction of installment credit for certain durables; the creation of wholesale agencies which organize trade fairs and circulate information on stocks and requirements in trade. The prediction is that as incomes continue to rise Soviet marketing practices will tend to resemble more and more those of the capitalist West.

Without questioning that prediction, I wonder whether additional attention should not be given to other possible developments. From recent Soviet discussions of the role and power of the Soviet firm, can one foresee changes that would increase the firm's responsiveness to consumer preferences? From the new Soviet interest in the application of mathematics to economics—apparently including models relevant to demand estimation<sup>1</sup>—can one foresee the introduction of large-scale econometric models for planning purposes?

Finally, there is an empirical chapter on distribution costs. For 1960 Goldman calculates the ratio of the distribution markup (retail and wholesale distribution costs plus profit) to retail sales. The result—20 to 30 per cent, depending on some component estimates—is compared with the U.S. markup-to-sales ratios of 37-50 per cent and with the official Soviet claim that retail and wholesale distribution costs are only 7.5 per cent of retail sales. The calculated ratio differs from the official ratio mainly because the official ratio includes turnover taxes in the denominator, excludes profits and the costs of procurement from the numerator, and excludes collective farm market sales and costs.

Goldman also compares retail sales per worker in the USSR and the United States by applying average ruble-dollar price ratios to sales per worker in native currencies. The calculation is repeated for 1954 and 1958, with USSR and U.S. weights, and for sales-per-worker data of varying scope. In rubles, sales per worker in the USSR tend to be about 80 per cent of those in the United States; in dollars, the ratio is greater and, for some calculations, greater than unity.<sup>2</sup> For reasons with which I tend to agree, Goldman is reluctant to make much of any of these international comparisons as efficiency comparisons. Although this reluctance may explain the somewhat casual character of the comparisons, it is not clear why he has bothered with them here at all.

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<sup>1</sup> E.g., A. Agranbegan *et al.*, *Primenenie matematiki i elektronnoi tekhniki v planirovanii*, Moscow 1961, Section III.

<sup>2</sup> See Table 18, pp. 180-81. Goldman does not put the results quite in this way. He seems to be unaware that the logic of alternative USSR- and U.S.-weighted price "deflators" is alternative comparisons in rubles and in dollars. It should also be pointed out that (a) the average ruble-dollar ratios he uses include a sizable component of services which do not enter retail sales and which have much lower ruble-dollar ratios than foods and manufactured goods; and (b) although the average ruble-dollar ratios he uses refer to 1954, he uses them without adjustment to "deflate" 1958 as well as 1954 sales data at current prices.

### Industrial Organization; Government and Business; Industry Studies

*The Development of Firms—An Empirical Study with Special Reference to the Economic Effects of Taxation.* By A. S. MACKINTOSH. New York: Cambridge University Press, 1963. Pp. x, 305. \$7.00.

Growth-conscious governments, of which there are many in the modern world, are necessarily much concerned with the factors that limit the development of individual business enterprises. In order to study these factors, and in particular the impact of income taxation on them, Mr. A. S. Mackintosh, now Deputy Director of the British Overseas Development Institute, made in 1955-56 an intensive analysis of the postwar behavior of 36 Birmingham manufacturing firms. The results, which were first made available to the Board of Trade in 1957, have now been published after considerable delay and should be of interest to economists in both the fiscal and the development fields.

The book opens with the exposition of a general theory of business behavior that is both more flexible and less precise than the standard theory of the firm. Taking the basic business goal to be the greatest possible increase in the firm's ability to earn profits over a planning period of five to ten years, Part I briefly describes various means of attaining that goal and then focuses on five factors, called boundary conditions, that can limit the firm's rate of development. These factors are: (1) the amount of finance available to it from various sources; (2) the strength of the ambitions and incentives of its managers; (3) the availability of the factors of production needed for its operations; (4) the expandability of the markets in which it currently sells or to which it can readily shift its attention; and (5) in the absence of any of the above, the maximum rate at which growth can be effectively sustained even under the most favorable conditions. In his sample survey, the nature of which is described in Part II of the book, Mackintosh found that finance was the most frequently encountered of these limitations upon development (14 firms named it as an important restriction), with maximum rate second (9 firms), followed by lack of ambition or incentives (5 firms), factor supplies (5 firms), and market (3 firms).

Finance is also, of course, the one restriction to growth that can be directly eased by a reduction in income taxes, and Mackintosh gives it special attention in Part III, where the survey evidence is analyzed in detail. While the sample used is too restrictive to support general conclusions, three propositions especially suggestive of further research are put forward:

1. There is some profit rate,  $P_1$ , which most firms must achieve over a five to ten year period unless they are to be limited in their growth primarily by lack of finance, and some higher profit rate,  $P_2$ , above which firms are unlikely to be subject to a finance limit. For his sample of businesses Mackintosh estimates  $P_1$  to be  $16\frac{1}{2}$  per cent (profits before taxes divided by net assets) and  $P_2$  to be 22 per cent, regardless of the size of the firm or of the industry in which it operates. The possibility that higher income taxes mainly restrict

investment by low-profit enterprises raises interesting welfare questions which Mackintosh does not discuss.

2. Lower taxation of corporate profits will raise business investment directly by only a small fraction (estimated at 10-33 per cent) of the foregone tax yield, whereas considerably more of the tax saving will be used, particularly by large corporations, to repay outstanding indebtedness, to acquire financial assets, or to pay dividends. If this is so, the indirect effects of tax changes on investment, exerted through financial markets, may be even more important than the direct effects.

3. British investment allowances (subsidies) affected investment decisions in only 25 per cent of the firms studied, and their quantitative impact was small in most cases. Mackintosh regards this result, however, as an underestimate because of the newness of investment allowances (they were introduced only 20 months before the survey interviews were made), and argues strongly for them as growth stimulators in the place of straight tax rate reductions. He is apparently unconcerned about the interpersonal inequities imposed by this policy choice.

Dividend policies and the effects of the use of accelerated depreciation for tax purposes are also discussed in Part III, but the treatment is disappointing in view of the far more satisfactory work done on these topics in the past ten years in the United States (to which no reference is made).

The second half of the book (Part IV) is devoted to an extended analysis of each of the 36 firms studied. This is not a section likely to sustain the interest of the general reader, but the specialist is offered a wealth of information concerning such things as profit rates, sources and uses of funds, investment planning, and price policies. One gets the impression that Birmingham entrepreneurs, with some notable exceptions, are a cautious and conservative lot, and it would be interesting to compare their attitudes and behavior with those of businessmen elsewhere in Britain or in other countries. As other studies of this kind become available, much should be learned about the mysteries of economic growth.

GEORGE F. BREAK

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*British Shipping and World Competition.* By S. G. STURMEY. New York: Oxford University Press; London: Athlone Press, University of London, 1962. Pp. x, 436. \$8.00.

Charges of unfair competition through governmental aids to national merchant marine fleets have filled diplomatic dispatches and periodical literature of western European countries in recent years. Government subsidies, flag discrimination, tax aids, and transfers to flags of convenience have been among the leading charges. Particular focus has been given on occasion to the assistance to the U.S. merchant marine through construction and operating subsidies, cargo preference arrangements, and the acceptance of the auxiliary role of U.S. beneficially-owned bulk carriers under Panamanian and Liberian registry.

Are such conditions the explanation for the relative decline in the position of British shipping in the world from 45 per cent in 1900 to about 16 per cent in 1960? This is the question that is probed by S. G. Sturmev of University College, London, in his study. Through historical and economic analysis, he strips away layer by layer the factors that have entered into this decline. At the hard core, he finds decline resulting from the absence of self-evaluation in merchant shipping circles, accompanied by adherence to out-moded patterns of operation in the face of changing requirements.

The supremacy of British shipping in the period from 1850 to 1914 was the product of a unique set of circumstances. These included the primacy of British industrialization, the substantial trade with its colonies, the importance of its domestic coal supply for world bunkering stations and local markets, and the advantage of adequate domestic ship repair facilities and ports. The merchant marine was aided by the practice of British importers and exporters who sought in the terms of sale and purchase to be free to nominate the ships of use, namely British flag ships.

The historic supremacy of British shipping and its economic tie with coal resources are demonstrated by Sturmev to have served as constraining influences for over half a century. The development of oil resources and the diesel engine undermined the competitive advantage which high-grade domestic coal had given British tramps over lower-wage Norwegian and Greek ships. The patterns of ship ownership and operations which developed in the pre-war period remained substantially unchanged. British shipowners did not view tankers as ships, hence missing the opportunity to enter the newly expanding tanker trade in the interwar period. By contrast, the Norwegians were able to make their transition to the requirements of the transport and the utilization of oil. In the crucial period of the twenties, new enterprisers of Norway entered ship operations, gathering capital where they could at home and abroad. They organized themselves as supplementary providers to meet the needs of British and other petroleum companies who were operating a portion of their own ship requirements. The Norwegian practice of using extended time charters with petroleum companies as the basis for construction credits developed then and continued after World War II. Unlike British shipping interests which have continued to separate liner, tramp, tanker and bulk-carrier operations, Norwegian shipping interests fused their operations, thus hedging against cyclical fluctuations and assuring some flexibility in their ship operations.

To the contentions of the British shipping industry that foreign governmental aids in the form of subsidies and flag discrimination have thwarted the retention of the British shipping position, the author develops historical evidence to show that the British position had already been downgraded before subsidization had become extensive. He draws on the Norwegian experience in the 'twenties and the postwar period as evidence that adaptation to changing requirements has been the avenue for substantial and profitable entry into the world market, despite the absence of governmental aid in the form of subsidy and flag discrimination and the absence of a substantial national

trade. He views the subsidized U.S. flag fleet as being substantially circumscribed both in the world shipping market and in U.S. trade. On flags of convenience, the author points out that companies registered abroad may, and do in fact, register their ships under British flags. While established British interests have complained about the development of the Panlibhon registries, the author points out that they actually place the participation of foreign interests in British flag shipping on the credit side of the British shipping industry's ledger. The labor cost factor is dismissed as a cause of decline, for although British shipbuilding costs may be higher than those abroad, seamen's wages are shown to be substantially in line with those of other leading European maritime countries. Labor-management relations are described as having had a company-interest orientation, with labor officials emphasizing their concern for the industry. British law remains unchanged in making no distinction for trade disputes in port for disciplinary purposes. Official strikes have not occurred in the past 30 years. The result has been rank-and-file rebellion on two recent occasions.

Basically, intrinsic conditions are found to account for the decline. In addition to the failure to adapt extensively to diesel engines, there has also been the failure to move with the times with faster and larger ships. There has been the continuing concern with traditional organization in liner and tramp operations, and only recently efforts have been made to explore new opportunities associated with the growth of tankers in world trade. In the tramp area, the author suggests that, in view of the higher wage position of Britain along with the other northern European countries, the need here is for faster, larger ships to compensate for the lower crew costs but older and slower ships operated by Greek shipowners.

The emphasis given by the author to self-evaluation and self-help warrants an observation on the documentation in the study. Despite the importance of British shipping to the national economy and the national image, the British literature cited in the report is relatively thin. The latest official report referred to is dated 1939. The latest shipping industry reports were made in 1939 and 1960. The author indicates that family control of shipping enterprises has resulted in avoidance of public discussion and disclosure. Actually, much of the data relating to British costs is derived from the hearings and reports of U.S. congressional committees on the subsidized merchant marine. In the United States, continuing governmental scrutiny has been the result of defense concern, consideration of the appropriateness of governmental allocations and policy in support of the U.S. flag merchant marine, and flare-ups resulting from union jurisdictional disputes. The role of such governmental concern as goad to industry efforts to meet the challenge of intensified competition through improvements in ship technology, size, and speed warrants study. The analysis and message contained in Sturme's study are obviously needed in Britain, but will prove useful elsewhere.

JOSEPH P. GOLDBERG

*Washington, D.C.*

*The Technology of Urban Transportation.* By DONALD S. BERRY, GEORGE W. BLOMME, PAUL W. SHULDINER AND JOHN H. JONES. Evanston: Northwestern University Press for Transportation Center, Northwestern University, 1963. Pp. xiv, 145.

The sudden rise to prominence of urban planning and the calling forth of economists, sociologists, and other social scientists into service make this little book timely.

With the passage of the 1962 Federal-Aid Highway Act, much of the planning activity at this stage is involved in trying to solve the problems of mobility within the metropolitan complex. In some cities, this has engendered heated arguments over the relative efficiencies of various transportation media.

After a brief survey of highway facilities and operating characteristics of the automobile, there is a good coverage of transit operations and equipment. Despite the glamor of futuristic-appearing rail rapid transit, equal space is given to buses and railroad commuter services. Included are tabulations on transit maintenance and operation costs, motor bus operating speeds on free-ways and exclusive curb lanes, capital outlay requirements on proposed rail rapid transit systems, and possible innovations in urban travel. Much of the material has been sifted out of the copious volumes of data in metropolitan area transportation studies. Other sources are included in their very comprehensive and useful bibliography.

The final section of the book has an excellent comparison of the capabilities of various types of transportation systems. The importance of finding the combination best suited to each area's characteristics of population density, economic development, and land use is stressed rather than reliance on any one mode of travel.

The authors evidently wrote the book with the idea of supplying information on the least-known subjects. It would seem, however, that in presenting a survey of this type, the characteristics of automotive transportation deserve more attention. This leads to a major complaint about the book; it should have been twice as long. Despite its compactness, however, the authors have managed to present a book written with considerable clarity which will appeal to the interested nonspecialist.

RALPH L. TABOR

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### **Land Economics; Agricultural Economics; Economic Geography; Housing**

*Housing and Income.* By MARGARET G. REID. Chicago, Illinois: The University of Chicago Press, 1962. 415 p. \$7.50.

This is a book with a message. This message, which is transmitted with great fervor, contains essentially two parts. First, the permanent income hypothesis of Milton Friedman provides a useful framework for the study of

consumer behavior. (The essence of this hypothesis is that consumer expenditures and savings are determined primarily by long-run, or permanent, income rather than by current income.) The value of this framework becomes apparent when it is used as a basis for the study of consumer demand for housing, which is the subject of this book.

Second, the income elasticity of housing is much larger than has previously been supposed. In particular, the usual highly inelastic income elasticities obtained from analysis of cross-section data with consumer units as the individual observations provide misleadingly low estimates of this elasticity. In other words, Schwabe's "Law of Rent," that the proportion of income spent for housing declines with income level, is not true. Estimates of income elasticity of housing obtained from such cross-section regressions are biased because the principal independent variable is measured income, not "normal," or long-run income. Since families gear their housing expenditures to normal income and not necessarily to current income, the discrepancy between the two variables represents, in effect, an error of measurement, which according to statistical estimation theory is bound to bias downward the estimate of income elasticity. This bias is shown to be substantial, and housing is asserted to be highly income elastic, the true value of the elasticity being in the neighborhood of 2.0.

The basis for this conclusion is the permanent income hypothesis. Miss Reid, following Milton Friedman, represents current, measured, family income as the sum of two components: normal (equivalent to permanent) income and transitory income. Housing expenditure, being of a long-term nature, is asserted to be a function of normal income. Therefore, true income elasticities can only be obtained when the transitory component of income is eliminated from measured income. This she attempts to do, again following Milton Friedman, by combining consumer units into groups which may be expected to have more or less the same level of normal income. Since transitory income for a group of families homogeneous in normal income is, by definition, composed of erratic and more or less random elements, this combination procedure serves to reduce greatly the relative importance of transitory income. As a result, regressions of housing expenditure on income and on other relevant variables for these groups are much more likely to yield true income elasticities than are the usual regressions with the individual family as the unit of observation.

The empirical work that supports this reasoning comprises the bulk of this volume, and rests on a virtually bewildering number and variety of regressions, some of a cross-section nature and others utilizing time series data. Initially, cross-section estimates of income elasticity based on regressions between average income and average housing expenditure by income level are shown to be quite low (Chapter 5). These elasticities are reduced even further if variables are introduced holding constant such variables as occupation, education, race, and urban area. Since these variables are presumably proxies for normal income, their introduction serves to increase the relative importance of transitory income, thereby increasing the bias and reducing

the estimated income elasticity. Estimated income elasticity rises if family size and age of children are introduced into the housing-income regression, these variables presumably serving to increase the importance of normal income.

To get rid of the bias due to the imperfections of measured income and of measured housing, Miss Reid proceeds to estimate the cross-section income elasticity by correlating average income with average housing expenditure, first for cities as a whole (Chapter 6), next for particular strata that would reflect normal income within cities—house quality and Census tract (Chapter 7), and finally by differences in income and housing expenditures between central cities and their suburbs (Chapter 8). These regressions produce a wide variety of income elasticity estimates, ranging from .8 to almost 3.0, but averaging roughly 2.0. It is largely on the basis of these data, which are asserted to be least contaminated with transitory income effects, that Miss Reid concludes that housing expenditures are actually highly income elastic. In the course of doing so, she finds that total expenditures constitute an unsatisfactory proxy for normal income (Chapter 9), which is fortunate because the elasticities thereby obtained tend to be appreciably below unity.

The time series evidence provides less impressive support. Regressing total annual housing expenditures on annual income yields low elasticities for the pre-World War II period, though rising to about 1.4 for the postwar years (Chapter 10). Regressions of temporal changes in housing expenditure on changes in average income and in various other variables, with cities as the unit of observation, yield income elasticities ranging from 1.1 to 2.0 for 1919-1934, but the elasticities are not much above 1.0 for 1918-1950.

In contrast to the preceding material, all of which suggests that the demand for housing is highly elastic, an analysis of the demand for rooms (Chapter 12) yields low elasticities, ranging from .2 to .5. According to Miss Reid, "the difference suggests that it is useful to look upon demand for rooms as relatively inelastic, whereas the demand for quality housing is highly elastic with respect to income" (p. 348). A final empirical chapter (13) presents a brief analysis of data from the 1960 census, suggesting that all of the previous results receive additional support.

The book concludes with two chapters summarizing the principal findings and discussing their implications for public housing programs and for the analysis of consumer behavior. In particular, she emphasizes the potential applicability of the same techniques to other categories of consumer expenditure.

Despite the tremendous amount of painstaking empirical work presented in this volume, the permanent income hypothesis receives much less support than one would expect, considering the exclusive focus of the book on the single most important category of consumer expenditure. From a conceptual point of view, the question of why housing expenditures should be related to "normal income" is ignored, except for a few brief passages in which this relation is accepted as obvious. To be sure, it is intuitively plausible that consumers should gear their housing outlays to some longer-run enabling measure than current income. But do they do so? Can they do so even if

they think in terms of normal income? And, if so, how is normal income estimated? To what extent do biases in these estimates and the presence of uncertainty affect any such planned expenditures?

No discussion or empirical evidence are to be found on such issues. Yet, the very size of housing expenditures suggests that such questions may be far more important for the applicability of the permanent income hypothesis to this category than to any other type of expenditure. To mention a few reasons relating to housing why the hypothesis may need modification: (1) a young couple might not have the financial resources to buy a house commensurate with its level of normal income; (2) because of satisfaction with such facilities, the family may not care to move to more expensive housing even when its income rises to or above its level of normal income; and (3) unavailability of housing at all price levels for minority groups will interfere with the operation of the hypothesis.

The meaning, use, stability, and consistency (among consumer units) of normal income are even more basic questions that might have been considered, although they relate not to housing alone.

Under the circumstances, question may well be raised of the extent to which the empirical results truly support the permanent income hypothesis. Even further question may be raised in the light of the weaknesses of the basic data, particularly with respect to the necessarily arbitrary estimate of housing expenditures of consumer units in owner-occupied dwellings. In such cases, housing expenditure is assumed equivalent to the rental value of the dwelling, which is in turn estimated at 10 per cent of the owner's estimate of its market value. In view of the substantial appreciation of housing prices during the post-World War II period, housing consumption in the sense used in this study may be substantially overstated by the use of rental value, because of offsetting saving in the form of capital appreciation. This is particularly true of the many homes purchased with large mortgages before the fifties, since these mortgages have been paid off with much cheaper dollars. The estimation of rental value as a constant percentage of the value reported for all types of homes raises further questions. Little support is provided for this procedure, and it is hard to believe that any single percentage would provide an unbiased estimate of rental value of all types of homes, in all cities, for all types of people, and in all years.

In view of the conceptual and empirical difficulties encountered in attempting to reconcile the empirical results with the permanent income hypothesis, it is only natural to seek other hypotheses that might also explain these results. No doubt other such hypotheses exist, some stemming from economic considerations while others may stem from the nature of the data themselves. The latter point of view, which has been hardly explored in the past, would seem to suggest at least one such explanation. This explanation relates to the little-known phenomenon of data combination, which, as noted previously, comprises the statistical basis for all of Miss Reid's results. Evidence exists that incomes are understated in consumer surveys and that the extent of understatement may increase with income level, both in absolute and in relative terms. If this is so, housing-income regressions using income

level as the unit of observation will be biased downward substantially, simply because of errors of measurement. On the other hand, if such regressions are computed using cities as the unit of observation—units large enough to permit averaging of errors over all levels of income—the errors in the data are bound to be much less than in the previous case, and the resulting income elasticities will be substantially higher. Thus, these results would support Miss Reid's basic assertion, that housing expenditures are truly income elastic, though not for the reasons cited.

This is not the place to explore the various ramifications of this hypothesis. All things considered, however, it is not at all clear that the results presented in this volume provide strong support for the permanent income hypothesis, even though this hypothesis serves as the framework for the volume. It is clear that Miss Reid has provided us with a highly provocative piece of work, carried out with great care and painstaking detail. Considerable doubt has been shed on the long-held belief in the income-inelastic nature of housing expenditures. This is a major contribution to the study of consumer behavior.

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*Politics and World Oil Economics—An Account of the International Oil Industry in Its Political Environment.* By J. E. HARTSHORN. New York: Praeger Publishing Co., 1962. Pp. 365. \$8.50.

J. E. Hartshorn has addressed himself to a topic which is of vital interest to the Free World and which virtually cries out for a competent economic discussion of its various policy facets. Of all the commodities that flow across national boundaries, probably none is more important than oil for modern economies.

Hartshorn's book is divided into three parts. Part I (about half the book) is an attempt to develop an historic and analytical perspective about the international oil industry. Part II attempts to develop an understanding of the present problems of the industry vis-à-vis the various governments of both the producing and consuming nations. The third and shortest section of the book attempts to assess the future roles of consuming nations, host governments, and the international firms.

The author has described his work as "a descriptive book with some attempt at analysis" (p. 28). Unfortunately both the descriptive and the analytical sections suffer from inaccuracies. Since Hartshorn eschews footnotes, it is impossible to check on the veracity of the many statistical and historical "facts" which fill this volume; however, the glaring inaccuracies and omissions of some obvious facts—e.g., placing Eugene V. Rostow on the Harvard faculty (p. 207), or the omission of any mention of the dumping of "distressed" gasoline by refiners as a factor in U.S. retail gas price wars (pp. 123 ff.)—raise doubts about the accuracy and/or the completeness of all the descriptive portions of the book.

The book's greatest fault, however, lies in its incorrect analytical treatment of the basic supply and demand factors that affect the industry. Instead of

providing the reader with some new analytical insights about the oil industry, Hartshorn tends merely to repeat many of the cliché rationalizations which he has obtained by "asking questions . . . of [unidentified] people inside this industry, or in a position to influence it" (p. 26). This *post hoc ergo propter hoc* analysis, however, which is often used by industry spokesmen to justify the industry's position, simply cannot be accepted at face value. Hartshorn's naïve acceptance of such analysis results in many incorrect statements and leads him to at least one major indefensible conclusion, namely that the international supply of oil is "the same as might be expected to arise from the operation of the law of comparative costs in a freely competitive international market" (p. 340). Had Hartshorn properly analyzed the wealth of evidence available (including much of the descriptive material presented in his book), he might have reached a quite different judgment.

Space limitations prevent a complete cataloguing of all the implicit and explicit analytical errors committed by the author. A few of the more important errors will be suggested below, in the hopes of alerting the reader to the incorrect analytical foundation of Hartshorn's major conclusions, and to suggest, where possible, the proper line of analysis.

Perhaps the most obvious blunder which appears throughout is Hartshorn's confusion about the nature of the economic concepts of marginal and average costs. Despite Hartshorn's endless repetition, it does not automatically follow that since fixed costs are high relative to operating costs, the crude oil segment of the industry is *always* "a situation of 'decreasing marginal costs'" (p. 57). (Declining average total costs do not necessarily imply decreasing marginal costs.) Similarly, the high capital intensity of the refining stage of the industry does not necessarily mean that there are "diminishing costs and increasing returns for each incremental barrel up to the limits of capacity" (p. 69), as Hartshorn claims.<sup>1</sup>

Furthermore, Hartshorn is apparently completely unaware of the ramifications of his belief that the oil industry is one of decreasing marginal costs. He is oblivious to the well-established analytical conclusions that *if* the industry is one of decreasing marginal costs, then (a) competition is incompatible with stability, and (b) such an industry will exhibit a market failure, i.e., it will not efficiently allocate resources if entrepreneurs are motivated by profit maximization in a market system.<sup>2</sup> Accordingly, Hartshorn's conclusion about the efficacy of the logistics of oil under present arrangements (p. 340) is inconsistent with his major (but not necessarily correct)<sup>3</sup> belief that the oil industry is one which exhibits decreasing marginal costs.

Furthermore, Hartshorn's own discussion of the marketing aspects of the international oil industry belies his own belief in the economic efficiency of

<sup>1</sup> Hartshorn's complete lack of understanding of cost concepts can also be found on page 111 where he confuses average variable costs and total variable costs when he claims that low rates of operation raise "total operating expenses." On pages 69-70, Hartshorn incorrectly concludes that one cannot obtain the marginal cost of each product when products are jointly produced in variable proportions.

<sup>2</sup> See F. Bator, "The Anatomy of Market Failure," *Quart. Jour. Econ.*, Aug. 1958, 72, 365-69.

<sup>3</sup> Adelman has presented a strong argument suggesting that (1) a high fixed cost/

the present situation. Hartshorn's constant references to (1) the barriers to entry in world markets (pp. 73, 103-04, 106-07, 112, 144-45), (2) the present surplus of oil (pp. 53, 207), (3) price discrimination practices (pp. 125, 222), (4) basing points (pp. 134-35), (5) the oligopolistic nature of the international market and the parallel interest of the seven major international companies (pp. 155, 159), (6) the ubiquitousness of nonprice competition (pp. 155-56), (7) the improved channels of communication between rival companies as a result of joint ventures (pp. 162-65), and (8) the protective practices of consumer governments for domestic energy industries (pp. 142, 206-07, 248), call attention to the ludicrous nature of his conclusion that "the economic logic of this industry indeed relies upon the concept of the world as a single market, across which resources can freely be traded to the benefit of all; it is one of the few genuinely international manifestations of private enterprise" (p. 167).

Despite the large number of pages (in Part I) that Hartshorn devotes to the discussion of the price of oil, he never correctly analyzes supply and demand factors;<sup>4</sup> rather he tends to discuss supply and demand in the slipshod language of the financial reporter which results in the conclusion that the price is what it is because no one has made it anything else—e.g., see the discussion of the price of Libyan oil (pp. 149-50).

In his discussion of conservation, Hartshorn does provide the reader with an insight as to why, though he peppers his prose with economic jargon, he consistently misuses analytical economic concepts. For example, he gives the correct economic definition of conservation, i.e., "if the discounted future value of oil exceeds the cost of doing without it today, restricting output would be economically logical" (p. 187). He then dismisses such a "purist criteria" [sic] (on the grounds that it involves assessing an uncertain future) and in doing so indicates his distaste for economists since they "are well placed to point out the weaknesses in other people's approaches to questions of conservation, but less competent to follow out the ramifications of their own (a posture some may consider characteristic of the profession)" (p. 188). Such anti-intellectual comments are not likely to advance the public discussion of conservation practices! Uncertainty about future events is not a reason for scrapping marginal user cost analysis<sup>5</sup> in determining the optimum rate of extraction that maximizes expected welfare. Although this method is subject to human fallibility in predicting the course of future events (as is any method of conservation), user cost analysis suggests the correct approach for husbanding resources—a claim that cannot be made for other prorationing schemes.

Hartshorn's discussion of royalty payments also suffers from a poor analytical treatment. Despite the fact that the author recognizes the "economic

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variable cost ratio cannot be taken as *prima facie* evidence of economics of scale and (2) the oil industry is actually one of increasing costs. See M. Adelman, "The World Oil Outlook: An Academic View," forthcoming paper to be published in 1964 in *Resources and Economic Development* under the auspices of Resources for the Future.

<sup>4</sup>For a much shorter and more cogent analysis of oil prices, see M. Adelman, "Oil Prices, 1963-1975" to be published in the *Revue de l'Institut Français du Pétrole*.

<sup>5</sup>See P. Davidson, "Public Policy Problems of the Domestic Crude Oil Industry," *Am. Econ. Rev.*, March 1963, 53, 91-98.

rent" nature of royalties, he advocates a flat barrel royalty payment, and hopes that the companies and host governments can work out a new agreement which shifts the incidence of the royalty payments to the general taxpayers of the home nations of the oil firms (pp. 321-22). Hartshorn does not even realize that such a royalty agreement may be inequitable to the home taxpayers, and, even more importantly, it results in an inefficient allocation of resources. Royalties which are paid on either a per barrel or a percentage of gross revenue basis tend to convert a supplementary cost (a rent) into a prime cost, and therefore it will affect the short-run supply function.<sup>6</sup> In a world of perfect certainty and competition, of course, the income distribution between producers and host nations would not be any different than if royalties were paid as fixed sum. With uncertainty, however, it is possible for host nations to receive an amount which differs from the *ex post* rents involved. Since the concession contract may last for many years, the short-run marginal royalty costs can have a substantial and long-time effect on the rate of production. Consequently, proper analytical treatment would suggest that royalties should be paid on a unit of time basis rather than on a production basis. This would make royalties a supplementary cost—which, since royalties are economic rents, is as it should be.

In summary, it can be stated that although Hartshorn has addressed himself to an area that urgently needs objective study in order to formulate an "efficient" public policy, he has done little to promote an understanding of the problem. In fact, the nonexpert reader in this field is likely to be misinformed by this work.

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\* *Ibid.*, pp. 90-91. Hartshorn provides an example of the impact of payments based on production to host nations on production rates when he notes that production from Venezuela declined significantly when payments to the host government were increased (pp. 273-74).

*Middle East Oil Crises and Western Europe's Energy Supplies.* By HAROLD LUBELL. Baltimore: The Johns Hopkins Press, 1963. Pp. xv, 233. \$8.75.

Considering the economic and political implications of Western Europe's growing dependence on imported fuels for the Free World, it is not surprising to see that the above-mentioned book is part of a research program carried out by The RAND Corporation for the United States Air Force. The author of this study, Harold Lubell, has previously examined for The RAND Corporation the Soviet oil offensive [1] and is an expert in oil economics with a great grasp of the political factors which always seem inseparably tied with questions concerning oil all over the world.

The pivotal issue of the present study is Western Europe's predominant dependence on oil from the Middle East and economic consequences if supplies from this source are, for one reason or the other, denied to her. The crux of the problem lies, as the author observes, in a pronounced lack of concern in Western Europe about a possible threat to its vital overseas oil supplies. The reason usually advanced for this attitude is a belief that the oil-

producing countries, particularly those in the Middle East, cannot be expected to act in unison (a) for a host of mutual political differences; and (b) because the present and territorially diversified great surplus of oil would make an attempt to cut Europe from its Middle East supplies economically ineffective.

Lubell, who doubts the soundness of such opinions, therefore, starts his inquiry with a preliminary examination of political and economic issues which led to the Iranian oil stoppage from 1951 to 1954 and the Suez crisis of 1956. The conclusion he draws from these precedents, that a repetition of similar stoppages in the Middle East oil shipments cannot be excluded from consideration is, of course, not startling. His position also finds support in the present tension generated by growing demands pressed by the steadily enlarging Organization of Petroleum Exporting Countries (OPEC).

It should be observed, however, that the scope of Lubell's inquiry is restricted, first, to the economic impact of a partial or complete interruption of oil deliveries from the Middle East only. Second, it is presumed that an ultimate alternative source of oil for Western Europe will be the Western Hemisphere, which practically means the United States and Venezuela. Thus, the study excludes any examination of the role which Soviet oil may play during an effective stoppage of Middle East shipments; neither does it consider the weight which potential availability of the Soviet crude may carry in a decision-making process of other oil producing countries. Of some significance for the author's conclusions is his further assumption that African oil will not materially diminish European dependence on the Middle East since, as he surmises, such oil will replace crude from the Western Hemisphere rather than from the Middle East. This in brief represents the framework of reference for Lubell's analysis which is executed at great depth and supported by an impressive array of quantitative data.

The analysis is divided into three parts. The first deals with several possible patterns of crises in the Middle East and their varying effect on the area oil output, oil transportation routes, tanker requirements, and freight rates. The second part is a detailed scrutiny of technical and economic problems that the oil industry in the Western Hemisphere would face under conditions explored in the first part of the book. In turn, the anticipated repercussions of an emergency expansion of oil output in Western Hemisphere on the oil-delivered prices in Western Europe are analyzed. In the third part, the new fuel supply situation is examined from the point of Western Europe's interest in staving off, or at least mitigating, damaging effects on her economy of a possible breakdown in the Middle East oil shipments.

In main, only two fundamental alternatives seem to be open to Western Europe. One is the attainment of the highest possible degree of autarchy in indigenous energy production and this essentially means maintenance of excessive coal-producing capacity. The other alternative is to rely predominantly on Western Hemisphere fuels and supplement them with an appropriate stockpiling policy. It would also require maintenance of excess oil capacity in Western Hemisphere and surplus tankers under Western control. If the comparative economic cost only is considered, the second alternative seems to be the most advantageous for Europe.

Once the author's premises are accepted, no serious objections can be raised to his conclusions. Of course, a few doubts may be expressed with respect to the validity of some of the study limitations and assumptions. For one, availability of African oil *does* affect the significance of the Middle East for Europe. This is clear from the fact that the former's share in total oil supplies of the Economic Community rose from 7 per cent in 1960 to (estimated) 19 per cent in 1963, whereas the relative share of the latter area declined from 71 to (estimated) 62 per cent in the same period [2]. The elimination of any analysis of the impact which availability of Soviet oil may have on a potential outbreak of another Middle East crisis may also be questioned. It is not hard to imagine, though, that in spite of all appearances the fact that the Soviet pipelines now extend to the very confines of Western Europe may be one of the main reasons for European complacency about security of its future oil supplies.

Nevertheless, these remarks hardly diminish the great contribution Lubell makes to our knowledge of oil economics. Four appendices, of which the linear programming of the economic cost for Europe of restricted availability of oil, written by K. J. Arrow, merits special attention, add to the value of this scholarly and objective study.

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#### Labor Economics

*Government and Agriculture: Public Policy in a Democratic Society*. By DALE E. HATHAWAY. New York: Macmillan Publishing Co., 1963. Pp. viii, 412; \$8.95.

This book is a combined economic and political analysis of agricultural policy. While it is not entirely unique in combining these two elements of policy, it does put more weight on economics than previous books (e.g., Wesley McCune, *Who's Behind Our Farm Policy*, New York, 1957) which emphasized the political aspects and more weight on politics than other books (e.g., Harold Halcrow, *Agricultural Policy of the United States*, Englewood Cliffs, N.J., 1953, and Earl O. Heady, *Agricultural Policy Under Economic Development*, Ames, 1962) which have emphasized the economic aspects of agricultural policy. The contents of the book are weighted more heavily to the political foundation of policy than to fundamental economic analysis of farm programs and policy formulation.

The book is largely a literary analysis of farm policy and can be under-

stood readily by the undergraduate and noneconomist. Hence, it should have a broad impact on increasing public understanding of the origin of farm problems, alternatives in their solution, values and beliefs underlying policy positions of various groups, and the political basis of conflict in farm policy. It is well written for these purposes, using even geometry very sparingly in the economic analysis.

Professor Hathaway initiates his analysis with a discussion of the beliefs and values underlying U.S. farm policy and returns to this foundation throughout the book. In this respect, paralleling other farm policy discussions of the last few years, he emphasizes political and social stability, economic stability, economic organization, economic growth, equality of opportunity, and sharing of U.S. abundance abroad. Chapter 2 includes a detailed numerical description and a detailed interpretation of economic conditions in American agriculture. Chapter 3 includes a short discussion of such policy goals as family farms, parity, equal bargaining power, food for peace, and freedom. Part II of the book includes an empirical presentation of the economic reasons for farm problems and the author's interpretation of the relevant variables and forces affecting the income and structure of farming. Part III deals with the processes of policy formulation, the role of the various participants: Congress, the executive, and farm organizations. Part IV deals with alternative policies or proposed policies and their economic and political possibilities in solving the major farm problems of disequilibrium, instability, and poverty. Policies discussed in detail include free market prices, demand expansion programs, output control, direct compensation and taxation and programs outside of agriculture (e.g., full employment, labor migration, etc.).

The author assumes or concludes that the free market will neither solve the problem of disequilibrium in agriculture nor be acceptable to farmers and other groups closely related to agriculture. He makes a highly complete and clear interpretation of certain economic facets of programs that have been used or proposed as substitutes for a market economy. He does not, however, detail a systematic set of policy elements to solve the major economic problems of agriculture. His interlacing of the complexities in economics and political structure goes far in explaining realistically why "the farm policy mess" exists.

To a slight extent, the book is a debate with W. W. Cochrane (*Farm Prices, Myth and Reality*, St. Paul, 1958) on the reasons for the farm problem and the alternatives in its solution. The author rejects the static competitive theory of the firm as a basis for understanding farm policy problems. Yet where he dips deepest into economic tools, he resorts (e.g., pp. 110-25 and 279-80) to this very set of concepts.

Hathaway has provided a "down to earth" analysis of farm policy problems and formulation. It is a treatise which will broaden the knowledge of a wide group of possible readers.

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### Population; Welfare Programs; Consumer Economics

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### Related Disciplines

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# NOTES

## ECONOMICS LIBRARY SELECTIONS

The Department of Economics and the Center for Regional Economic Studies of the University of Pittsburgh announce that they are resuming publication of the Economics Library Selections List effective during the winter of 1963-64. This series, originally published at the Johns Hopkins University in March, 1954, was discontinued in the fall of 1962.

Series I of the Economics Library Selections List is a quarterly publication providing *annotations* of all books presently being published in English. It also contains annotations of many books published in German, French, and Spanish. In addition, a recommendation regarding the minimum size library (based on annual appropriation for economics books purchases) is made. Each book is also "evaluated" in terms of the type of teaching or research library for which it might be suitable. The books are arranged according to the principal subdisciplines of economics.

It has become necessary to sell these annotated bibliographies. The price to individuals (not associated with a subscriber library) is \$5.00 per year. The price to libraries is \$10.00. However, any library subscribing to the series is entitled to enter supplementary subscriptions for faculty at that institution at the rate of \$2.00 per subscription and for its students at the rate of \$1.00 per subscription. It will, of course, be necessary to identify the faculty and student subscribers.

The distribution of this publication is to be handled by the publishing firm of Gordon and Breach. Communications regarding subscriptions should be sent directly to them at 150 Fifth Avenue, New York 11, New York. Queries regarding publication policy and all editorial matters should be addressed to the Economics Library Selection Lists, 211 S. Dithridge Street, Pittsburgh 13, Pennsylvania.

## JOURNALS

*The National Banking Review* is a new quarterly journal. It is published by the Office of the Comptroller of the Currency and includes articles, both practical and theoretical, from the points of view of the banker, the economist, the lawyer, and the public administrator. Current banking and economic developments and current changes in law and regulation will be presented in each issue. Address inquiries to Editor, *The National Banking Review*, Office of the Comptroller of the Currency, U.S. Treasury Department, Washington 25, D.C.

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The Library of International Relations is publishing, quarterly, *International Information Service*, an annotated listing of materials in the fields of economics, political science, geography, and others. The coverage is approximately 1000 current periodicals published in all parts of the world. Inquiries should be sent to 660 North Wabash Avenue, Chicago 11, Illinois.

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A new journal, *The German Economic Review*, appeared recently. This is an English language quarterly on German economic research and current developments. The initial issue contained articles, book reviews, a selected bibliography, reports, and news. Inquiries may be addressed to the Editorial Office, D. Held, Authenriethstrasse 21, 74 Tübingen, Germany.

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The Institute of Asian Economic Affairs is publishing in English a new journal, *The Developing Economies*. Inquiries should be sent to the Editor, 42 Honmura-chō, Ichigaya, Shinjuku-ku, Tokyo, Japan.

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*The Journal of Air Law and Commerce* is now being published at the School of Law of Southern Methodist University. Authors are invited to submit manuscripts dealing with questions of the regulations of the industry, domestic or international, economic or financial problems of the airlines, or such other topics as may be included within the subject matter suggested by the *Journal's* title. Manuscripts should be submitted to the editor, Professor Richard J. Barber, School of Law, Southern Methodist University, Dallas, Texas, 75222.

#### ANNOUNCEMENTS

The American Council of Learned Societies invites interested scholars to call to its attention the titles of books in the humanities and social sciences, written in languages not widely known in this country, that deserve to be published in translation for the use of American scholars. The ACLS has initiated a program devoted to making such works available by sponsoring their translation into English; it will not itself publish them, but it will arrange for their publication.

Works proposed for translation should be major works of scholarship that are not available in any of the four languages—French, German, Spanish, Italian—that American scholars may be assumed to read without great difficulty. Suggestions should be addressed to the Scholarly Translations Program, American Council of Learned Societies, 345 East 46th Street, New York, 10017. Each nomination should include an analytical précis of the book in either English or French, together with the name and address of at least one scholarly authority who is competent to evaluate the work's importance to American scholars.

For the 17th consecutive year, the Survey Research Center of the University of Michigan will hold a Summer Institute in Survey Research Techniques. The Institute is designed to meet some of the educational and training needs of men and women engaged in business and governmental research and other statistical work, and graduate students and university instructors interested in quantitative research in the social sciences.

The 1964 Institute will be presented in two four-week sessions, the first from June 22 to July 18 and the second from July 20 to August 15. These two sessions may be taken independently or successively. For further information please write to the Survey Research Center, University of Michigan, P.O. Box 1248, Ann Arbor, Michigan, 48106.

#### Deaths

Lee Bidgood, Tuscaloosa, Alabama, May 17, 1963.

Kenneth H. Campbell, Washington, D.C., March 24, 1963.

John Maurice Clark, professor emeritus, Columbia University, June 27, 1963.

John Culliton, professor, department of economics and political science, McGill University, October 31, 1963.

Editha Hadcock, DePauw University, December 9, 1963.

Abram Harris, professor of economics, University of Chicago, November 16, 1963.

H. Gordon Hayes, New Orleans, Louisiana, May 4, 1963.

H. P. B. Jenkins, Lawrence, Kansas, formerly at the University of Arkansas, January 26, 1963.

Vernon D. Jolley, professor, department of business and economics, Marshall University, November 14, 1963.

Otto von Mering, Lucerne, Switzerland, June 17, 1963.

Mildred Benedict Northrup, chairman, department of economics, Bryn Mawr College, October 18, 1963.

Dennis Robertson, Trinity College, Cambridge, England, April 21, 1963.

Richard C. Wilcock, Institute of Labor and Industrial Relations, University of Illinois, December 8, 1963.

*Retirements*

Arthur R. Burns, Columbia University, June 1963.

Howard L. Jones, professor of statistics, Graduate School of Business, University of Chicago, October 1963.

Samuel S. Stratton, president, Middlebury College, September 1963.

V. Judson Wyckoff, DePauw University, June 1964.

*Visiting Foreign Scholars*

Oje Aboyade, The University College, Ibadan, Nigeria: visiting assistant professor of economics, University of Michigan, academic year 1963-64.

Edwin O. Alcántara-Pons, Universidad Raval Landívar and Consejo Nacional de Planificación, Guatemala: visiting lecturer, department of economics, University of Texas.

Paul Braesz, University of Cologne, Germany: visiting professor of economics, Pennsylvania State University, spring 1964.

Sukhamoy Chakravarty, Delhi University: visiting associate professor of economics, Massachusetts Institute of Technology, academic year 1963-64.

Ragnar Frisch, University of Oslo: visiting professor of economics, University of Pittsburgh, winter trimester 1964.

George Morton, London School of Economics: visiting professor of economics, University of Wisconsin-Milwaukee, academic year 1964-65.

Labhras O'Nuallain, University College, Galway, Ireland: visiting lecturer in international business, College of Business Administration, Boston University, 1963-64.

M. M. Poston, Cambridge University, England: visiting professor of economics and history, Institute of Industrial Relations, University of California, Berkeley, spring semester 1964.

Jan Sandee, Central Planning Bureau, The Hague, Netherlands: visiting professor of economics, Massachusetts Institute of Technology, academic year 1963-64.

*Promotions*

Redmond J. Allman: professor, economics department, Bentley College.

Albert K. Ando: associate professor of economics, Massachusetts Institute of Technology.

George W. Berry: associate professor of finance, Texas Technological College.

Dale L. Cramer: professor, department of economics, University of Alabama.

James A. Crutchfield: professor of economics, University of Washington

Edward Y. George: associate professor, Bentley College.

Manuel Gottlieb: professor of economics, University of Wisconsin-Milwaukee.

William D. Heier: assistant professor, College of Business Administration, University of South Florida.

Lester S. Levy: professor of economics, Texas Technological College.

Harry A. Marmion: associate professor of economics, U.S. Coast Guard Academy.

Alvin L. Marty: associate professor, department of economics, The City College of New York.

Richard E. Neel: associate professor, College of Business Administration, University of South Florida.

Phillip Nelson: associate professor, department of economics, New School for Social Research.

William H. Peterson: professor of economics, Graduate School of Business Administration, New York University

John H. Reese: associate professor of finance, Texas Technological College.

- Philip Ross: assistant professor, Graduate School of Business, University of Pittsburgh.  
 Franklin R. Shupp: associate professor, University of Illinois.  
 Robert M. Stern: associate professor of economics, University of Michigan.  
 Charles M. Sullivan: professor, economics department, Bentley College.  
 Lynn Turgeon: associate professor of economics, Hofstra University.  
 James Vaughan: assistant professor, Graduate School of Business, University of Pittsburgh.  
 St. Stanley Wasowski: associate professor, department of economics, Georgetown University.  
 June Zaccone: assistant professor of economics, Hofstra University.

### *Administrative Appointments*

David A. Baerncopf: acting head, department of accounting and business statistics and associate professor of business statistics, School of Business Administration, University of Oregon.

Dwight S. Brothers: acting chairman, department of economics, Rice University, 1963-64.

Frank C. Child: chairman, department of economics, University of California, Davis.

Gregory C. Chow: head, Economics Research Group, IBM Research Center.

Darwin W. Daicoff: director and chief economist, Office of Economic Analysis, State of Kansas.

Thomas W. Douglas: director, Industrial Relations and Personnel Management Program, The American University.

William C. Frederick: acting dean, Graduate School of Business, University of Pittsburgh.

Charles E. Johnson: dean, College of Liberal Arts and professor of accounting, University of Oregon.

L. C. Jurgensen: chairman of the accounting program, College of Business Administration, University of South Florida.

James W. Kelley: acting dean, College of Business Administration, Boston University.

Walter H. Klein: assistant dean, College of Commerce and Finance, Villanova University.

Robert E. Miller: chairman, department of economics and business, University of Tampa.

David L. Mosconi: chairman, department of finance, College of Business Administration, University of Denver.

James Owens: assistant to the dean, in charge of MBA program, School of Business Administration, The American University.

Stuart A. Rich: director, Forest Industries Management Center and associate professor of industrial marketing, School of Business Administration, University of Oregon.

Marshall A. Robinson, University of Pittsburgh: director, Program in Economic Development and Administration, Ford Foundation, New York.

Daniel D. Roman: director, Production and Research and Development Management Programs, The American University.

Warren L. Smith: chairman, department of economics, University of Michigan.

Wolfgang F. Stolper: director, Center for Research on Economic Development, University of Michigan.

William S. Vickrey: chairman, department of economics, Columbia University, July 1964.

Jacob Weissman: acting chairman, department of economics, Hofstra University.

### *Appointments*

- Warren E. Adams, University of Texas: program economist, USAID, New Delhi, India.  
 Andrzej Brzeski, Wayne State University: acting assistant professor of economics, University of California, Davis.

Robert H. Burton: assistant professor, College of Business Administration, University of South Florida.

Ivan T. Call, Valley National Bank: assistant professor of business administration, Brigham Young University.

William Capatch, University of Pittsburgh: Kent State University.

Simone Clemhout: lecturer, department of economics, The City College of New York.

Charles Cole, University of Southern California: assistant professor of economics, Occidental College.

W. D. Comer: assistant professor of finance, Texas Technological College.

Albert Conley: associate professor of economics, School of Business, East Carolina College.

Guenter Conradus, University of Washington: instructor in economics, Occidental College.

George Constantacopoulos: assistant professor of economics, Rutgers—The State University.

Charles Cooper, University of Pittsburgh: Indiana State College.

W. K. Cunningham: assistant professor, College of Business Administration, University of South Florida.

T. M. Dickerson: lecturer, College of Business Administration, University of South Florida.

Richard E. Dutton: assistant professor, College of Business Administration, University of South Florida.

James O. Eaton: visiting professor of accounting, School of Business Administration, University of Oregon.

Robert W. Fogel, University of Rochester: associate professor, department of economics, University of Chicago, October 1964.

Padraic P. Frucht, U.S. Dept. of Commerce: associate professor, department of economics, College of Business Administration, University of Florida.

David Funk: instructor, department of economics, Wellesley College.

John J. Griffin, Jr., Federal Reserve Bank of New York: instructor, department of economics, Graduate School of Business Administration, New York University.

Delmar Hartley: assistant professor of finance, Texas Technological College.

John Howard, University of Pittsburgh: Columbia University.

Ralph James: associate professor of economics, University of California, Davis.

Richard A. Jenner: assistant professor of economics, Rice University.

Alice Hanson Jones: visiting instructor, department of economics, Washington University, academic year 1963-64.

Hiromitsu Kaneda: lecturer, department of economics, University of California, Davis.

Richard F. Kosobud: lecturer in economics, University of Michigan.

Edward Kuriger: instructor, department of economics and business administration, Geneva College.

Richard A. Ladd: assistant professor of economics, U.S. Coast Guard Academy.

Mordechai Lando: instructor in economics, Hofstra University.

Richard E. Low: assistant professor of economics, Pennsylvania State University.

Anthony Luchek, University of Wisconsin-Milwaukee: Labor Affairs Adviser, USAID, Indonesia.

Paul W. MacAvoy: assistant professor of economics, Massachusetts Institute of Technology.

J. F. McMullan: assistant professor, College of Business Administration, University of South Florida.

Thomas Mayer: professor of economics, University of California, Davis.

W. G. Modrow: assistant professor, College of Business Administration, University of South Florida.

R. Joseph Monsen, Brigham Young University: associate professor, department of general business, College of Business Administration, University of Washington.

Roy E. Moor, staff, Joint Economic Committee: administrative assistant to Senator William Proxmire.

Oscar Moore: associate professor of economics, School of Business, East Carolina College.

Alan Page Murray, Office of Tax Analysis, U.S. Treasury Department: staff, Joint Economic Committee.

Hong-Lan Oei, University of Texas: junior economist with U.N., ECAFE, Bangkok.

Roy Ott, Jr.: associate professor, department of finance, University of Alabama.

Robert A. Parsons: assistant professor, economics department, Bentley College.

Louis Pondy: instructor in business administration, Graduate School of Business, University of Pittsburgh.

Martin L. Primack: associate professor of economics, Bentley College.

Otto R. Reischer: economic adviser, Investment Advisory Center, Government of Pakistan, Karachi.

T. Donald Rucker, Michigan Medical Service: research economist, National Association of Blue Shield Plans.

Trevor Sainsbury: assistant professor, Graduate School of Business, University of Pittsburgh.

Lewis M. Schneider: assistant professor of business administration, Harvard University.

Richard Thomas Selden, Columbia University: professor of economics, Cornell University.

W. A. Shannon, Jr.: instructor, College of Business Administration, University of South Florida.

T. Y. Shen, Wayne State University: associate professor of economics, University of California, Davis.

David Singer: instructor in economics, Hofstra University.

Herbert Stein: State Supreme Court, Harrisburg, Pennsylvania.

Lewis M. Stewart: instructor in economics, Texas Technological College.

Vladimir Stoikov, Wesleyan University: associate professor, Queens College, City University of New York.

John J. Surmeier, RAND Corporation: operations analyst, Research Analysis Corporation.

Edwin Terry: assistant professor, department of economics, Washington University.

Arthur L. Thomas: assistant professor of accounting, University of Oregon.

Robert P. Thomas, Northwestern University: acting assistant professor, University of Washington.

Richard H. Tilly: lecturer in economics, University of Michigan.

Gerhard Tintner: professor of economics and mathematics, University of Southern California.

Donald L. Thompson: assistant professor of marketing, University of Oregon.

Robert J. Trusk: economist, Near East and South Asia Section, Eximbank, Washington, D.C.

Sheila Tschinkel: assistant professor of economics, University of Alaska.

Hirofumi Uzawa, Stanford University: professor of economics, University of Chicago, July 1964.

Ella May Webb: instructor, College of Business Administration, University of South Florida.

Byron White: associate professor of economics, School of Business, East Carolina College.

*Leaves for Special Appointments*

W. H. Locke Anderson, University of Michigan: staff, Council of Economic Advisers, academic year 1963-64.

John H. Auten, Rice University: economic consultant, U.S. Treasury Department, 1963-64.

Wesley Ballaine, University of Oregon: consultant, University of the Andes, Venezuela, May to July 1964.

Barbara Berman, Brandeis University: Brookings Institution, academic year 1964-65.

Arthur I. Bloomfield, University of Pennsylvania: visiting professor, Princeton University, fall term 1963-64.

John S. Chipman, University of Minnesota: visiting professor of economics, University of Rochester, first semester.

George Dalton, Northwestern University: International Labour Office, Geneva, 1964-65.

Laurence deRycke, Occidental College: consultant, University del Valle, Columbia, December 1963 to summer 1965.

Edward O. Edwards, Rice University: Ford Foundation in Kenya, 1963-64.

Milton Friedman, University of Chicago: visiting research professor, Columbia University.

Daniel R. Fusfeld, University of Michigan: director, Manpower, Development, and Training Activities Branch, Office of Program Planning, U.S. Office of Education, academic year 1963-64.

W. Lee Hansen, University of California, Los Angeles: staff, President's Council of Economic Advisers, 1964.

Harvey J. Levin, Hofstra University: Carnegie Fellow in Law and Economics, Harvard University.

Fritz Machlup, Princeton University: visiting professor, doctoral program in economics, City University of New York, 1963-64.

Sid Mittra, University of Detroit: Senior Economic Consultant, Simulmatics Corporation Project, Venezuela.

David H. Pollock, Chief, United Nations ECLA Washington Office: Special Assistant to the Secretary-General of the United Nations Conference on Trade and Development, through mid-1964, in Geneva, Switzerland.

Melvin W. Reder, Stanford University: visiting professor of economics, University of California, Berkeley, spring semester, 1964.

Stefan H. Robock, Indiana University: United Nations Special Adviser on Economic Planning, Bolivia, 3 months 1963.

Wilson E. Schmidt, George Washington University: visiting professor of economics, Bologna Center, Johns Hopkins University, 1963-64.

Stephen Spiegelglas, Boston University: AID mission to Turkey.

Lynn Turgeon, Hofstra University: Institute of Economics, Academy of Sciences, USSR on exchange, sponsored by the American Council of Learned Societies, fall 1963.

Donald A. Watson, University of Oregon: consultant, University of the Andes, Venezuela, January to March 1964.

Robert E. Weintraub, The City College of New York: senior economist, House of Representatives Banking and Currency Committee, spring 1964.

J. N. Wolfe, University of California, Santa Barbara: Brookings Research Professor, 1963-64, attached to National Economic Development Office, London.

James S. Youtsler, Skidmore College: visiting professor, American University of Beirut, 1963-64.

*Resignations*

Marto A. Ballesteros, University of Washington.

Francis M. Bator, Massachusetts Institute of Technology.

James F. Jackson, Jr., University of South Florida.

Robert B. McNee, The City College of New York.

*Correction*

The issue of December, 1963 carried an item, under appointments, about David McCord Wright that was misleading. It should have stated that his permanent position is with the University of Georgia; that he resigned as trustee of the Foundation of Economic Education; and that he was appointed economic consultant to the Federal Reserve Bank of Atlanta.

## IMPORTANT NOTICE TO MEMBERS

The Executive Committee of the American Economic Association has authorized our participation with ten other national professional associations in the National Science Foundation's 1964 Register of Scientific and Technical Personnel. In connection with this project, you will be receiving a questionnaire sometime during March. Because the information provided by your answers will enable us to make a detailed analysis of the structure of the economics profession, members are urged to cooperate by filling out and returning the questionnaire at their earliest convenience.

HAROLD F. WILLIAMSON  
Secretary

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# THE AMERICAN ECONOMIC REVIEW

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Manuscripts and editorial correspondence relating to the regular quarterly issues of this REVIEW should be addressed to John G. Gurley, Managing Editor of THE AMERICAN ECONOMIC REVIEW, Stanford University, Stanford, California 94305. *Style Instructions* for guidance in preparing manuscripts in acceptable form will be provided upon request to the editor.

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# The American Economic Review

VOLUME LIV

JUNE 1964 NUMBER FOUR, PART I

THE AMERICAN ECONOMIC ASSOCIATION, 1904-29

By A. W. COATS\*

## *Prologue*

At its birth in 1885 the American Economic Association was a healthy infant whose lusty cries for attention and nourishment soon attracted enough interest and support to ensure that it would enjoy a vigorous and effective life. Yet even the most auspicious beginnings could not entirely counteract the effects of heredity. The offspring of mixed parentage—European scholarship and Yankee social reform—the Association revealed even before its seventh year those schizoid tendencies which were to continue in evidence throughout its adolescence and early adulthood. The inherited tensions resulting from the conflicting attractions of pure science and public service were, admittedly, reinforced by environmental influences. But even when the environment became more favorable, when the internal divisions among its members had faded into the mists of memory, and when the foundations of a long and fruitful career had been securely laid, there still remained doubts—not only about the most effective methods of achieving the Association's purposes, but also about the very nature of those purposes.

## *I. Introduction: Scholarship or Survival?*

It is often difficult to discern clearly demarcated turning-points in the life of an individual or the development of an organization, and there are no compelling reasons why the second instalment of the history of the American Economic Association should begin in 1904 [6] [8]. However, that year witnessed the formal burying of a hatchet and the inauguration of what the Secretary called an "era of good feelings" [1, F. A. Fetter to E. R. A. Seligman, Apr 4 1904] for, after delicate and protracted negotiations, it was decided that the next annual meet-

\*The author is reader in economic and social history at the University of York, England. This article is a sequel to one published earlier in this *Review* [6], and the author wishes to thank the Secretary of the Association, Harold F. Williamson, and his Research Assistant, Richard Buth, for their help in finding and photocopying source materials.

ing should be held at the University of Chicago, whereupon J. L. Laughlin, Head of the Department of Economics at Chicago and the only prominent survivor of the "old school" opposition to the American Economic Association, graciously accepted the inevitable and at last became a member. This happy event<sup>1</sup> did not, of course, mark the complete disappearance of "the petty and ridiculous politics that seem to be inseparable from the conduct of all American associations."<sup>2</sup> The wide variety of interests and the geographical dispersion of the Association's membership were themselves sufficient to ensure the persistence of disagreements about its policy—as, for example, in the perennial problem of choosing a suitable location for the annual meeting. Nevertheless, from the early years of this century, personal, doctrinal, and institutional rivalries played a subordinate role in the organization's history, and under its benevolent and enlightened leadership the unity of the American economists was assured.

In a sense, 1904 marks the end of the parochial phase of the A.E.A.'s history, and in subsequent years its officers were free to devote more of their time and energy to the task of establishing and enhancing its position on the national stage. This does not mean that their trials and tribulations were over: for although the Association's position as the leading organization of American economists was rarely questioned, its precise character and purpose remained in doubt, and from time to time financial difficulties threatened its very existence. In this case, as so often occurs, ends and means were interdependent, and in times of crisis the future of the organization seemed to hang on the outcome of a struggle between scholarship and survival. This struggle was not solely or even mainly attributable to financial difficulties; nor was it an unusual experience for a scholarly organization in the United States, since it reflected the basic insecurity of the intellectual's position in American society. Nevertheless, the problem presented itself to the economists in an especially acute form because the scientific status of their discipline was open to question, and they were torn between the conflicting claims of scholarship and public service. Both as individuals and as an organized body, the American economists sought to enhance their public prestige and influence, especially

<sup>1</sup> "Laughlin's paper alone . . . was quite enough for the entire session, and I felt much sympathy with his exclamation at the close that he wished we had three hours to debate this subject. . . . Certainly the meeting in Chicago must be counted a very successful one from the point of view of attendance, spirit and interest" [1, Fetter to F. W. Taussig, Jan 6 1905].

<sup>2</sup> [1, F. H. Giddings to Fetter, May 3 1902]. This remark was made in the course of the interminable debate over the question of an A.E.A. journal, a topic which was first raised in the early 1890's and finally settled with the publication of the *American Economic Review* in 1911. An account of this debate must be postponed until a subsequent occasion.

with the business community, without at the same time jeopardizing their reputation as detached, impartial scientists. To the individual economist the problem of deciding how to divide his time and energy between the groves of academe and the market place was a subjective one, to be resolved by a personal decision which, even if conscious, was seldom irrevocable; and for many economists the outcome must have been largely determined by the chance availability of employment opportunities. To the officers of the American Economic Association, on the other hand, this problem necessarily assumed a more overt, institutional form, and the solutions adopted are revealed in the membership campaigns, in the effort to avoid public policy commitments, in the conduct of the Association's annual meetings, and in the form and content of its publications. In view of the wider issues concerning the position of the professional economist in American society, an examination of these mundane affairs possesses an interest which transcends the parochial or the antiquarian. If, for example, we find that by the end of the 1920's the American Economic Association had solved its internal, organizational problems and had come to terms with its environment, we may thereby obtain some indication of the progress (or perhaps the corruption) of American scholarship.<sup>3</sup> If, however, these problems were still unsolved, we are automatically provided with a number of pertinent questions about the subsequent development of American economic thought.

## II. *The Size and Character of Membership*

If one may judge an organization by its membership, the American Economic Association has never been a purely scholarly and scientific body, and in this respect its policy has consistently adhered to the wishes of the young rebels who founded it in the 1880's partly in protest against the domination of American economic thought by what they conceived to be an exclusively orthodox "school" [6] [8] [7]. In its initial, reformist phase, when the first Secretary, Richard T. Ely, was largely responsible for these matters, determined efforts were made to enlist the support of a wide variety of nonacademic persons, and the early membership lists included a high proportion of clergymen (some of whom were also college professors). Yet even at that stage there were some members who wondered whether the desire for comprehensiveness and catholicity were being pushed too far, and this doubt recurred from time to time in later years.

During the late 1890's, a conscious effort was made to arouse the

<sup>3</sup> The question "Are the Economists a Bunch of Kept Cats?" was (not too seriously) discussed at the 1963 meeting of the Mid-Western Economic Association at St. Louis. For comments on the problem of professional ethics in the 1920's, see [9, pp. 208-11].

support of business and professional men [6, p. 571], and the first systematic membership campaign was undertaken by Ely, almost single-handed, in 1900-1902, while he was President of the Association. The timing of this campaign was no coincidence, for Ely not only possessed the requisite combination of character, skill, and outlook; he was also seeking to demonstrate his indispensability to an organization which he regarded, with only partial justification, as his own offspring. It was, however, a coincidence that the second membership campaign, launched in 1909, was conducted by a man of similar qualities, Thomas Nixon Carver. Like Ely, Carver possessed dynamic energy, considerable administrative skill, a taste and a talent for popularization, and a firm belief that the Association should play an active part in disseminating "sound" economic ideas and shaping public policy. His attitude towards the A.E.A. was, of course, less possessive than Ely's; but neither man displayed any hesitation in impressing his own forceful personality on the organization.

Both of these campaigns were prompted by a desire to increase the Association's activities at a time when its membership revenue was expanding slowly. The finances were, it is true, much stronger in 1909 than they had been ten years earlier; but the need for revenue was even greater because of the decision to begin publishing the *American Economic Review*. When Carver was elected Secretary, at the 1908 annual meeting, a membership committee was also appointed; but although the ensuing campaign gathered momentum the prospects of eventual success seemed poor for, as Carver showed in a lucid economic analysis of the problem incorporated in his Secretary's report for 1911, a very large increase in membership would be needed to make a significant addition to the Association's income. The campaign expenses were so high that each new member added a net marginal income of only \$1.40, and the rate of growth of income was far too slow to cover the expected rise of expenditure [3, pp. 132-37]. However, this analysis attracted the attention of a New York lawyer, Albert de Roode, one of Carver's former Harvard students, and he and a group of his business colleagues offered to provide a special fund of \$1,000 for the campaign. As he explained in a letter to Carver:

Our interest in the work is because we understand that the Association wishes to extend its scope and influence among the non-academic members of the community—in other words, the propagation of sound economic doctrine among the unenlightened and the stimulation of discussion on economic subjects among those not professionally interested in it.<sup>4</sup>

<sup>4</sup> [1, de Roode to Carver, Apr 9 1912. Also de Roode to Carver, Mar 18 1912; Sep 24 1912; Carver to de Roode, Apr 13 1912]. De Roode's name was at first unknown even

Carver readily accepted these terms of reference because they were in complete harmony with his own conception of the Association's functions, and de Roode's plan involved no change in either the character or the direction of the campaign. The most active person on the membership committee was Roger W. Babson, a professional statistical forecaster who became Chairman of the Committee in 1912, and he too was anxious to strengthen the Association's links with the business community. As it transpired, despite a considerable expenditure of time and money, the last stage of the membership campaign yielded disappointing results for, as Carver pointed out, a condition of diminishing returns had been reached, and a high proportion of the nonacademic recruits retained their interest for only a year or so and were sometimes aggrieved when they were subsequently asked to pay up their outstanding dues!<sup>5</sup>

In the present context, the numerical and financial details of the membership campaign are less interesting than its implications for the character and functions of the American Economic Association. Some members favored the "open" membership plan because it was "democratic," and when a Council member attended the inaugural meeting of the British Economic Association (later the Royal Economic Society) in 1890, he reported with evident satisfaction that the British economists had followed the American example in rejecting all proposals designed to restrict membership [11, p. 166]. Others, however, objected to the policy of "going into the far-flung highways for more neophytes" [1, O. F. Boucke to Secretary, May 1 1925], and when one circular called upon members to solicit subscriptions from local organizations and suggested that they might "take out membership for some friend as a Christmas present,"<sup>6</sup> there were grounds for thinking that the limits of scholarly and scientific dignity had been

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to members of the membership committee. [Cf., 1, R. Babson to Carver, Apr 16 1912; Carver to Babson, May 6 1912; Fetter to Carver, Apr 12 1912; Carver to Fetter, May 21 1912].

<sup>5</sup> [4, p. 202]. During the period 1909-13 a net increase in membership of 1483 was claimed, and by December, 1915 Carver's successor, A. A. Young, considered that a state of "normal equilibrium" had been reached [5, p. 221]. However, the annual secretary's reports give somewhat different total membership figures, e.g., 1900-774; 1902-1111 (during Ely's campaign); 1908-1030; 1909-1360; 1913-2513; 1915-2422. The totals were continually being "pruned" by removing the names of delinquents. For subsequent figures, see *infra*, note 47.

One nonacademic member who had asked by what authority the Secretary proposed to cut him off on the grounds of nonpayment of his subscription eventually enclosed a check in settlement "of all claims of every kind and description, scientific, commercial or otherwise" [1, H. Stewart to Secretary, Mar 8 1919].

<sup>6</sup> This is taken from an undated circular, probably drafted in November, 1911, which may not have been actually sent out to members.

exceeded. Some of those invited to join accepted the honor with alacrity, remarking that they had only held back because they had assumed that they lacked the necessary qualifications;<sup>7</sup> but occasionally those who refused took the opportunity of expressing their opinion of academic economists and current economic policy in no uncertain terms!<sup>8</sup> The *reductio ad absurdum* of this promiscuous membership policy was the businessman who, having accepted Babson's invitation to join, wrote to the Secretary asking him to outline "a course of study that would not be too heavy, and to which a business man could give attention day by day with the hope of a fair mastery of this subject"; and Carver seems to have accepted this pedagogical function as a normal part of his secretarial duties.<sup>9</sup>

Another corollary of the Association's purely pecuniary criterion of membership was its unwillingness to be held responsible for either the personal character or the views of its members. For example, when the Secretary of the New York Reform Club offered to extend its facilities to members of the American Economic Association, he was warned:

... there is no test for membership in the Association. . . . The payment

<sup>7</sup> In a letter reminiscent of the responses to Ely's appeals in the early days of the A.E.A., the Pastor of the Congregational Church at Northbridge Center (Mass.?) expressed his appreciation of the nomination for membership and hoped soon to be able to send the Treasurer the proper credentials so as to ensure his enrollment. "In view of present world conditions I believe it is important that leaders of the new World Order [sic] such as leading economists either in actual business life or associated with it should unite to study the problems involved" [1, W. W. Evans to Secretary, May 23 1921]. (*Plus ça change, plus c'est la même chose.*)

Another respondent was pleased that he was not considered too radical to be a member but asked to see the Constitution in order to reassure himself that the A.E.A. was not like such other organizations as the New Haven Railroad, the International Motor Co., etc. "in each of which the membership has been ignored by the management and finds itself without recourse" [1, H. F. Stimpson to Secretary, Oct 18 1913; Nov. 14 1913].

<sup>8</sup> One respondent sent a curt telegram stating "Make your publications worth the money" [1, E. A. MacClean, New York City, Feb 4 1914]. Another associated the work of the A.E.A. with "the attempts of half-educated writers to direct and lead public thought," which had helped to feed the "mad ambition and lust of power by the reckless demagogue who once brought this country during the most prosperous period of its existence to the verge of ruin" [1, J. H. Moore, San Francisco, May 9 1912]. (He was presumably referring to Theodore Roosevelt.) However, the correspondent who described the economists as equivalent to "intellectual prostitutes" apparently had not been invited to join the A.E.A. [1, S. O. Gordon to Secretary, Apr 5 1922].

<sup>9</sup> [1, E. C. Miller to Carver, Mar 18 1912; Carver to Miller, Mar 26 1912]. Roger W. Babson had invited Miller to join the Association.

Another insight into the character of the membership was provided by the correspondent who, when asked to renew his subscription, replied, "Our subscription has been made to you from year to year for the purpose of being helpful to you, and not with a view to our own improvement. We never asked for your literature, nor did we expect to receive any . . . if it has been received we have been so unfortunate as not to have perused it" [1, F. Beidler to Secretary, Jun 11 1914]. Nevertheless, he paid his bill in full!

of annual dues is in itself prima facie evidence of an interest in economic questions, but our membership includes people of very different political opinions and social walks of life. There is a remote danger that if membership offers the additional privileges of your library, some might join for that purpose.<sup>10</sup>

Similarly, when Richard T. Ely's Institute for Research in Land Economics and Public Utilities was attacked in 1926 as "a cunning propaganda institute in disguise,"<sup>11</sup> the Association's officers resisted the demand for an investigation, not only because Ely was a distinguished officer of the Association and the source of the complaint was prejudiced, but also because "such an investigation would be outside the functions of this organization."<sup>12</sup> A somewhat similar sentiment had been expressed earlier by the Secretary, Allyn A. Young, in 1915 when the American Association of University Professors was founded, and some prominent members of the A.E.A. were heartily relieved at the prospect that the difficult and inconclusive business of investigating complaints of violations of academic freedom would be taken over by another organization.<sup>13</sup>

Unlike the governing bodies of the Royal Economic Society and the Scottish Economic Society,<sup>14</sup> the Executive Committee of the

<sup>10</sup> [1, Fetter to M. R. Maltbie, Oct 29 1904]. Maltbie had assumed he could "take it that the fact that they are members of the A.E.A. is sufficient guarantee of their character, and that no improper advantage will be taken of the library privileges." Fetter was not so sure, for he recalled how people had unsuccessfully sought membership in the A.E.A. in order to obtain free sets of the Census volumes which had been offered to members.

<sup>11</sup> E. O. Jorgenson, a representative of the Manufacturers' and Merchants' Federal Tax League, protested that the Institute "has laid down its conclusions in advance of its investigations" and "has, before any of its facts have been gathered, condemned certain far-reaching measures which an increasing number of scholars believe to be beneficial to the people and approved certain other far-reaching measures which many scholars also believe to be detrimental to the people," thereby completely disregarding "the principles of fairness and impartiality." Moreover, the Institute's conclusions "harmonise exactly with the views of the corporations and special privilege groups which are financing it." Although the Institute's host institution, "Northwestern University, strange to say, has ignored our protests entirely," Jorgenson enclosed a copy of his book, *False Education in our Colleges and Universities*, and demanded "prompt and rigid investigation" by a committee of the A.E.A. [1, Jorgenson to E. W. Kemmerer (President of the A.E.A.), Jul 31 1926].

<sup>12</sup> [1, Kemmerer to Jorgenson, Dec 30 1926]. This letter was written after a meeting of the Executive Committee of the A.E.A. Cf. Young to F.C. Deibler (Secretary): "The request that the Association institute an investigation of the Institute for Land Economics will be turned down flatly, I hope. This is not only because it is not the business of the Association to make such investigations, but also because the proposal is an insult to Professor Ely and his associates. It comes from an irresponsible and prejudiced source" [1, Dec 11 1926].

<sup>13</sup> [1, H. C. Adams to Young, Apr 26 1915; Young to Adams, May 5 1915].

<sup>14</sup> According to the by-laws of the Royal Economic Society, "if any Fellow or Honorary Fellow shall so conduct himself that, in the opinion of the Council, it would not be for the credit and advantage of the Society that he should longer continue to be a Fellow or

A.E.A. had no power to request the resignation of any member whose conduct was held to be prejudicial to the interests of the organization; nor is there any evidence either that such power was sought or that the economists ever even contemplated the establishment of any procedure for dealing with cases of unprofessional conduct. In this matter, fear of destroying the "open" character of the Association was reinforced by an acute awareness of the virtual impossibility of obtaining agreement about the nature and tests of professional work in economics.

### III. *The Problem of Professional Identity and Standards*

Like many of its individual members, the American Economic Association had a foot in two distinct worlds, the world of learning and the world of business, and it has never been easy to serve two masters simultaneously. The "open" membership policy, the question of professional standards in economics, and the problem of deciding how far the Association should cater to the needs and interests of businessmen were all interrelated, and while Carver and Babson were proponents of an activist policy which emphasized the Association's links with and responsibilities to the business community,<sup>15</sup> Carver's successor, Allyn Young, who was Secretary from 1913 to 1920, adopted a quite different standpoint. Emphasizing the scholarly and scientific objects of the organization, Young admitted his "reluctance to try to extend our membership among business men except in so far as they thoroughly understand our work"; and to Davis R. Dewey, the managing editor of the *American Economic Review*, he confided:

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Honorary Fellow thereof, the Council shall take the matter into consideration at a meeting convened for the purpose." The Council of the Scottish Society of Economists, in the early years of this century, could delete the name of any member "at their discretion, and thereupon such member shall forfeit all rights and interest in the Society without the right of appeal." Cf. *infra*, note 18.

In response to a specific enquiry as to "what may be regarded as the proper standards of practice in the line of business which you represent," Deibler replied, almost facetiously: "You should know that our middle name is 'Ethics,' but we have no particular code" [1, R. A. Clemen, Chicago, to Secretary, Nov 15 1929; Deibler to Clemen, Nov 19 1929].

<sup>15</sup> Although on one occasion Carver wrote that "the main purpose" of the A.E.A. was "to run the affairs of the country in an economical and progressive way," he was aware of the duality of interests. "I hope the Association will never forget that it exists primarily in the interests of scholarship. Nevertheless, I think we have another important function, namely to act as a leaven to the whole national lump, and to do what we can to spread the habit of sound economic thinking among the business and professional classes, who have so much to do with the determination of national policies. Just how to maintain a proper balance between the interests of pure scholarship and those of popularisation must always be a difficult problem" [1, Carver to J. E. Woodbridge, Jan 20 1912; Carver to Prof. B. H. Putnam, Mount Holyoke College, Jan 24 1912].

I believe that we can make a sounder and more effective appeal to business men by putting the matter squarely on that basis (i.e. that they feel it an honor to be associated with us rather than merely getting their money's worth) than by attempting to cater in the slightest degree to their special interests as contrasted with those of the professional economists [1, Feb 18 1915; cf. Young to E. A. Pratt, Nov 11 1915].

The crux of the problem, however, lay hidden in that portmanteau term "professional," and this definitional difficulty assumed concrete form as Young went on to criticize the *Review* for printing too many merely "practical" and "topical" leading articles.

Such considerations as those of timeliness and of practical interest should be made distinctly subordinate in the selection of leading articles. Of course we ought to have at least one timely article in each number, but I do not think it should be necessary to go beyond that. Let me say frankly that this seems to be the opinion of a very large majority of members of the Association whom I have heard talk about the *Review*. They are satisfied with it in every way except the leading articles, and these they feel have not in general a sufficiently professional quality. [*Ibid.*]

These remarks not only raised questions about the precise meaning of the terms "practical" and "timely," "scholarly" and "professional"; they also suggested that academic and nonacademic members of the Association possessed distinct and contrasting interests—a notion that was not confirmed by the discussion evoked by Young's comments. To deny "professional" caliber to practical and timely articles was, *ipso facto*, to condemn them, for it clearly implied that the *Review* was failing to maintain the standards appropriate to a scholarly and scientific organization. Yet Young himself, when pressed, found it difficult to define the term "professional" in a satisfactory way. In a subsequent letter to Dewey he explained:

By articles of a distinctly professional or technical nature I meant articles which were shaped with primary reference to the interests and needs of economists, whether dealing with theory or with other matters. I would not put a merely descriptive article, written without reference to its bearing on fundamental questions of principle, under this head [1, Apr 12 1915].

This sweeping reference to the "needs of economists" represented a serious oversimplification of the problem, as is shown by the responses of members of the editorial board of the *Review*. The most decisive repudiation of Young's views came from H. E. Mills, who protested that his use of the expression "professional quality" was too vague and conveyed "a certain impression of desire for other worldliness." Mills continued:

Economics in America suffered because it was so professional and apart from real life. That time has happily gone because economists have come to study in a scholarly and professional way topics that are timely and have practical interest. College and university teaching deals more and more with concrete subjects in a professional and scholarly way. . . . I do not think we should "cater" to business men members; but on the other hand to separate from them is unwise and the Review may render a great service by showing to the business man the scientific and professional methods of approaching and handling concrete, practical questions. . . . It must be remembered that a large proportion of the trained and able economists of the country are now working upon and primarily interested in matters of practical significance, and hence articles of such character are likely to predominate.<sup>16</sup>

The reactions of other members of the editorial board, while on the whole supporting Mills rather than Young, also reveal the impossibility of drawing any sharp line between the categories "professional and scholarly" on the one hand, and "practical and timely" on the other. Herbert Davenport, for example, endorsed Young's opinion that few businessmen read the *Review*—a view that is confirmed by other correspondence in the Association's files—and added that most businessmen "would find what we call practical articles about as unusable as the very distinctly theoretical ones." But while opposing a policy of "going out for" more theoretical articles, he nevertheless considered that "if good stuff of the theoretical emphasis offers, we might wisely adopt a working presumption in its favour." Henry B. Gardner and Clive Day contended that the so-called "practical" articles not only helped professors to keep up with current questions of the day but had also proved to be of great value in teaching; and for most of the respondents the crucial question was not "balance" but "quality." Frank Fetter, for example, himself an economic theorist, considered that one good theoretical article was worth several concrete and descriptive ones; but he believed that few good theoretical articles were available:

Economic theory should be the product of the ripest and maturest economists, but most of them are interested in specific practical issues. It is the young men, hardly fully fledged, working for their doctor's degrees or little past that point who offer the major part of the manuscripts in theory, is it not, to say nothing of the rank amateur businessman or engineer who has lately discovered that "the earth do move." [*Ibid.*]

There was no deliberate change of editorial policy as a result of this

<sup>16</sup> Mills' comments, together with those of J. E. Coulter, H. Davenport, C. Day, F. A. Fetter, and H. B. Gardner, are preserved in the A.E.A. files.

discussion. Some economists believed that there was a tacit division of the market between the *American Economic Review* and its long-established rival, the *Quarterly Journal of Economics*, and on the basis of this supposition there may have been some tendency for authors to submit their more theoretical articles to the Harvard journal. But whatever the grounds for this belief, criticism of the *Review* was endemic, for it was impossible to please all the members of its heterogenous and widely scattered audience. As late as 1928 Dewey (who continued to be managing editor until 1940) was still patiently explaining that:

The range of interest of our constituency is appalling; and it has been assumed that the *Review* must meet all of these varied interests. No single individual partakes of all the items on the menu of a large hotel. He selects only a few; but the hotel apparently thinks it necessary to provide this wide selection [1, Dewey to Deibler, Mar 30 1928].

From time to time dissatisfied members proposed changes in the policy or composition of the Association, and these proposals clearly reveal the variety of interests involved. It was sometimes suggested that a professional association of nonacademic economists should be formed in affiliation with the American Economic Association, and on one occasion this suggestion was made by a business economist who was on the point of returning to a full-time academic post.<sup>17</sup> In 1915, shortly after the discussion of the *Review's* policy, a similar proposal was advanced by Dr. A. M. Sakolski, who was employed in the valuation office of the Delaware and Hudson Company, Albany, New York. Sakolski wrote to Allyn Young complaining that Roger Babson was taking unfair advantage of his connection with the Association, and suggesting "Isn't it time that a genuine scientias society of American economists should be formed, separate though not in opposition to the American Economic Society [sic]?" [1, n.d. (probably May 1915)]. Subsequently he elaborated his proposal which, he said, had been initiated by Dana Durand, Head of the Bureau of the Census, and his letter reveals the underlying motives:

The membership of a scientific society could be limited to members of the AEA having several years standing in the organization and also possessing the training, the qualifications and the activities of scientific economists. This arrangement would create an alliance without a definite

<sup>17</sup> [1, G. J. Eberle to J. H. Hollander, Jun 6 1921; Eberle to Westerfield, Dec 21 1921]. On Dec 15 1921 the Secretary dispatched a circular to members of the Executive Committee which referred to Eberle's proposal that the Association should establish "certain minimum qualifications for persons who call themselves professional economists" with the aim of protecting "the science and the professors, and to assure clients and prospective clients a certain standard of services." No action was taken.

corporate relationship. . . . Perhaps I feel more keenly about the matter than you, because I lack an academic connection and have no standing in a recognized profession. In spite of my official title, I am, after all, nothing but a clerk and cannot well claim the respect that is given lawyers, engineers and physicians in their duties. Only recently the American Society of Civil Engineers raised the qualifications for membership to their organization. If this policy were followed by the "scientific" group of the AEA it would, in my opinion, shut out the pseudo economists, statisticians, "industrial engineers," efficiency experts, etc. etc. who advertize their membership in the AEA as recognition of their scientific standing. Certainly teachers among economists, and especially those training men to follow this line of work, ought to favor the formation of a strictly professional society [1, Sep 23 1915].

While Sakolski's letter discloses some of the difficulties encountered by the would-be scientific economist in business, Young's sympathetic replies reveal the dilemma of his academic counterpart:

I do feel that the Economic Association has gone too far in catering to the interests of businessmen and to the outside public generally, but I cannot believe that it would be wise to attempt to start another organization [1, May 24 1915].

At one time, Young admitted, he had believed in the desirability of a purely scientific society of economists; but he now considered that if a limited society were to be formed it should be entirely separate from the American Economic Association, since the latter required at least 2,500 members in order to sustain its current activities and could not, therefore, afford to be selective. Recently, Young continued, he had accepted an invitation to be a "Fellow" of the American Statistical Association, supposedly "a small and select body of statisticians of established reputation," but he doubted the advisability of such a category in economics.

A body like the Institute of Actuaries of Great Britain, or like the American Actuarial Society can set up definite standards for membership, and can even set rigorous entrance examinations. In such a Society a fellowship means something, and has deservedly a real professional value. I doubt very much whether in our field it is possible to set up standards so definite and exact that we can create an organization membership which will in itself be a mark of definite standing and attainments. That seems to be the most substantial difficulty in the way of your plans.<sup>18</sup>

<sup>18</sup> [1, Young to Sakolski, Sep 25 1915]. It may be noted that although the Royal Statistical Society and the British Economic Association both had a constitutional procedure for dealing with the expulsion of undesirable members, neither the American Statistical Association nor the American Economic Association had a comparable procedure.

By a curious coincidence, about a month after his correspondence with Sakolski (which apparently bore no tangible organizational fruits), Young received a letter from E. A. Pratt, Chief of the Bureau of Foreign and Domestic Commerce of the Federal Department of Commerce, who represented the opposite point of view. Complaining that the *Review* contained little of interest to businessmen and non-academic readers, Pratt added:

Please do not interpret this to mean that I discount in any way the value of the university professor and what he has to say, but if you expect to draw business men to your organization, it seems to me absolutely necessary that you have them among your officers, that you put them on the program of your annual meetings and that you publish in your economic review things which would be of interest to them, written by men whose names will carry weight among them, and that you do not absolutely worship the strictly academic [1, Nov 8 1915].

In his reply Young granted the justice of some of Pratt's criticisms and explained:

The difficulty is, of course, that the Association is really trying to do two things at once, that is, to stand as the representative scientific organization of the professional economists, and more especially, the academic economists of the country, and at the same time to gather into its membership men from all walks of life who are interested in any way in practical economic problems. . . . The Association was in its beginnings primarily an academic organization, and I am inclined to think that probably it was wiser that it should remain so. If it should break away from its academic moorings, another Association would probably be formed to take its place. At the same time we do not want to sail under false pretences. This explains my reluctance to try to extend our membership among business men except in so far as they themselves understand our work [1, Nov 11 1915].

To Dewey, Young confided:

I am inclined to think that sooner or later we shall have to face the question of just what kind of an association we want to be. My own efforts, as you know, have been devoted to strengthening our hold upon those persons who might be counted upon to support a strictly professional and scientific association. I do not believe that Babson's efforts among business men have done us any good, for few of his nominees remain members for more than one year [1, n.d. (probably Nov 1915)].

Dewey's reply raised a question of central importance:

I have wondered whether it would be better to have two classes of members, one class for the most part composed of academic members who should pay the full fee of five dollars on the basis of professional interest

and loyalty and the other class of cooperative members who would perhaps pay a smaller fee and would connect themselves rather from public interest, very much as they subscribe for their other objects which are regarded to be for social welfare [1, Nov 15 1915].

The fundamental question of professional identity and standards could not, however, be disposed of so easily, and there is no reason to think that any such modification of the existing egalitarian membership policy would have been accepted at an annual meeting. Too many economists objected strongly to the creation of any special fellowship or other membership hierarchy, because it smacked of oligarchy and might lead to the domination of the organization by one of its constituent groups, such as the nonacademic economists or an Eastern academic elite.<sup>19</sup> In the event, a genuine problem of classification according to professional qualifications and expertise was forced upon the economists by the war, when the A.E.A. committee was asked by the Federal Civil Service Commission to examine 900 cards filled in by economists and statisticians who had expressed their willingness to serve the government. As the President of the Commission wrote:

It is understood that your committee will discard the names of persons who do not properly come within the category of economic expert, will classify under proper headings those who are trained along economic lines, and will make such comment in individual cases as you consider advisable [1, President, U. S. Civil Service Commission to Secretary, Oct 8 1917].

Like certain other delicate constitutional matters that arose during the wartime emergency, the question of the classification of economists did not come to a head, largely because of the speedy and successful termination of the war. In 1919 Abram Berglund of the Tariff Commission sent a report on classification to Young, but by this time the question had become somewhat academic.<sup>20</sup> During the 1920's the problem recurred again from time to time, but no action was taken.

<sup>19</sup> Although the danger of a regional split within the A.E.A. had apparently disappeared by the early 1900's, echoes of the earlier difficulties were occasionally heard, especially in connection with the perennial problem of the location of annual meetings. For example, H. Davenport wrote to Young on May 11, 1914, "it has largely been on the basis of these non-professional members that the large majority of eastern men in the Association has existed, and the right or claim built thereon to have most of the Association meetings take place in the East." In a judiciously impartial reply, Young wrote that the 1914 Handbook "will contain for the first time a geographical index of our membership. This should furnish ammunition for one side or the other" [6, pp. 567-70].

<sup>20</sup> [1, Berglund to Young, Dec 24 1919]. Under each of three broad headings, "Economist," "Agricultural Economist," and "Social Economist," there were five subcategories based on age, academic qualifications, and experience.

By 1924, as the result of another systematic membership campaign, a five-fold classification of members was introduced, but like the conventional distinctions between "honorary," "life," and "ordinary" members, this, too, was based exclusively on financial considerations.<sup>21</sup>

#### IV. *Public Responsibility and Nonpartisanship*

In addition to questions arising from the size and character of its membership and its responsibilities to both academic and nonacademic economists, the officers of the Association were always conscious of their relationship to the general public. The original constitution had stated that, "In the study of the industrial and commercial policy of government we take no partisan attitude," and throughout the period under review serious efforts were made to adhere to this general principle, for nonpartisanship was usually held to be the *sine qua non* of a scholarly and scientific approach to economic questions. In practice, however, this policy was more difficult to execute than might be supposed at first sight, for the organization's officers received many inquiries, invitations, and appeals which made it exceedingly difficult for them to maintain their pose of olympian detachment. If they occasionally strayed from the narrow path of righteousness, it was mainly because they were anxious to demonstrate the economist's importance to the business community and to the general public, for as Ely had frankly admitted when he presented his "Statement of Principles" at the inaugural meeting in 1885, "it is not easy to arouse interest in an association which professes nothing" [10]. These deviations were, of course, especially liable to occur under the leadership of activists like Ely, Carver, and Irving Fisher (who was President of the Association for a time during World War I), and the variety of forms which this deviationism assumed are worth noting because they reveal how varied were the temptations and pressures to which the economists were subjected.

Given the persistent differences of opinion as to what the Association should do, it is hardly surprising to find that there were corresponding differences about what it should not do, and the task of interpreting the "nonpartisanship" clause of the constitution was one to gladden the heart of the most devious of constitutional lawyers. Even Young, who so strenuously upheld the cause of scholarship,

<sup>21</sup> [1, For example, Westerfield to T. W. Page, Mar 26 1924 and Page to Westerfield, Dec 24 1924]. Also the circular to members of May 3, 1924, which was followed by a change in Article III of the Constitution. The new categories were "subscribing" and "contributing" members.

See also [1, T. W. Page to Deibler, Dec 7 1925; Managing Director of the Taylor Society to Secretary, June 23 1926].

proudly proclaimed that the Association had "done as much as any one influence to disseminate sound ideas on many economic problems";<sup>22</sup> but he would hardly have agreed with Carver that its "main purpose" was "to run the affairs of the country in an economical and progressive way" [1, Carver to J. E. Woodbridge, Jan 30 1912]. In some cases, of course, there was no difficulty in deciding on the appropriate course of action. The Secretary merely refused to assist those inquirers who wanted to know how to organize birth control education and research, or how to use Fuller's Earth, and he flatly told one correspondent that the A.E.A. took no official position on either the tariff question or the forthcoming presidential election.<sup>23</sup> On other occasions a request that the Association should "probe a little into the alcohol problem" from the standpoint of "pure scholarship" was rejected, and an invitation to join the opposition to a law which would have permitted the importation and dissemination of literature on contraception was refused;<sup>24</sup> but some problems could not be dealt with so easily.

One group of problems arose in connection with the appointment of official representatives to attend conferences or meetings sponsored by other organizations, and here the policies of the Executive Committee demonstrated remarkable inconsistencies. As the Secretary explained to a member on one occasion,

In 1907 . . . [Jeremiah W.] Jenks, then President, by authority of the Executive Committee, appointed delegates to the National Peace Conference. At this very same meeting of the Executive Committee it was voted impossible to cooperate as an Association with the Anti-Child Labor movement. I have also been informed that at a preliminary meeting of those interested in the Conservation of National Resources our Association was represented. If so the representatives were appointed by the President on his own responsibility. At the November meeting last year of the Executive Committee this very Association requested us to appoint representatives to cooperate with that movement. The explicit request for cooperation caused the Committee to decline the request. At that time Professor J. B. Clark argued that if our appointment of representatives were confined to their appearance in the role of interested

<sup>22</sup> [1, Young to A. Belmont, Mar 25 1915]. Young argued that the A.E.A. had made especially valuable contributions to the subjects of money, credit, and banking.

<sup>23</sup> [1, W. S. Reynolds to Secretary, Dec 13 1928; Secretary to Reynolds, Dec 20 1928; A. E. Carman to Secretary, Dec 3 1928; H. Clews to Secretary, Sep 24 1912; Secretary to Clews, Sep 20 and 30 1912]. Mr. Clews, a New York banker, had been asked if he would allow his name to be used on a circular letter to be sent to prospective members.

<sup>24</sup> [1, W. E. Johnson (editor of the *Standard Encyclopedia of the Alcohol Problem*) to Secretary, Jan 16 1905; Secretary of the National Catholic Welfare Conference to Deibler, Nov 18 1925]. In his reply of Nov. 23, 1925, Deibler offered to bring the matter before the Executive Committee but predicted that they would refuse to act.

inquirers merely, we might legitimately appoint delegates. To this view Judge Dill [a Vice-President of the A.E.A.] replied that the appointment of delegates, no matter what the intention, would be construed as sympathetic cooperation. The vote seemed to imply that Judge Dill's opinion was the prevailing one.<sup>25</sup>

Apart from the immediate question whether a delegate might be regarded as a passive observer or an active participant in the proceedings of the sponsoring body, there was the question whether an individual member or group of members could, under any circumstances, be held to commit the Association, *as an Association*, to support or oppose any policy position. In 1928, Fred M. Taylor, then President of the Association, turned down an invitation to become a Vice-President of the Stable Money Association mainly on the grounds that "I have for many years been an almost fanatical supporter of the doctrine that professors as such should keep out of all propaganda work,"<sup>26</sup> and this decision was fully consistent with the action taken by the Executive Committee a year earlier, when it had refused to hold a joint session with the Stable Money Association because the latter "was carrying on a campaign of education or propaganda, rather than constituting a scientific body, like other allied associations."<sup>27</sup> But although Young had informed a critic in 1916 that the A.E.A. was "debarred by its constitution from taking any position on any *disputed* economic problem,"<sup>28</sup> this did not mean, as some cynics might suppose, that the Association was thereby required to remain totally inactive in the field of policy. Among "safe" issues there were such

<sup>25</sup> [1, Acting Secretary of the A.E.A. to Dewey, Jan 23 1909]. (Dewey was then President of the Association.)

<sup>26</sup> [1, F. M. Taylor to N. Lombard, Jan 14 1928]. The leaders of the Stable Money Association included such prominent economists as I. Fisher and J. R. Commons.

<sup>27</sup> [1, Deibler to H. G. Moulton (Director of the Brookings Institute of Economics), Dec 3 1927; T. S. Adams (President of the A.E.A.) to Lombard, Nov 10 1927; Moulton to Adams, Dec 8 1927].

A similar problem arose in connection with other policy-oriented associations, such as the American Association for Labor Legislation, but, as Adams explained to Moulton, the A.A.L.L. had begun as an offshoot of the A.E.A. and its place on the latter's programs had been established by an "old precedent" [1, cf. Ely to Fisher, Jan 24 1919]. An account of the relationships between the A.E.A. and other associations must be deferred until another occasion.

<sup>28</sup> [1, Young to S. H. Pomeroy, Oct 23 1916, italics supplied]. Pomeroy had responded to an invitation to become a member by inquiring "I would ask why you bob up at Election times with your solicitation when your support of levelling wages is so antagonistic to our general welfare. Drop ambiguities and tell me how this country can thrive without a tariff to be made high or low to correspond with the differences in wages in this country and abroad" [1, Pomeroy to Secretary, Oct 18 1916].

On one occasion Young went even further, asserting that "the A.E.A. is prohibited by its constitution from taking any attitude whatsoever on any public question" [1, Young to A. B. Parker, Feb 13 1920].

matters as opposition to a rise in postal rates on the publications of scientific societies<sup>29</sup> and periodic demands for federal aid to American scholars participating in international scholarly and scientific conferences;<sup>30</sup> and during World War I the economists went far beyond the strict constructionist position adopted by Secretary Ray B. Westerfield who, in 1925, replied to a seemingly innocuous Department of Commerce request for an official comment on "the values of simplification in our present industrial and commercial situation" by stating that the A.E.A. was "not interested in propaganda."<sup>31</sup>

During the war, the Association endeavored to do its bit by assisting in the classification of economists who offered to work for the federal government, by inaugurating a scheme designed to coordinate research activities, and by publishing a number of bulletins and special studies of current economic problems. These were but natural responses to the needs of the time, but under the exuberant leadership of Irving Fisher and others, the Association took a number of steps which might, in the long run, have seriously undermined its customary policy of impartiality and detachment. Of these one of the most interesting was Fisher's appeal on behalf of the Thrift Campaign sponsored by the War Savings Committee of the U.S. Treasury, under the chairmanship of Frank A. Vanderlip. According to a circular signed by Fisher which was distributed to all members:

At the Philadelphia meeting a Committee on the Purchasing Power of Money in Relation to the War was appointed. In accordance with the vote at that meeting, this committee, with the authority of the Executive Committee, *but without committing the Association as such*, is publishing a series of bulletins. A copy of the first of these is enclosed. This bulletin was issued through Mr. Vanderlip's Committee, which prepared the introductory statement. It was sent to 20,000 newspapers but was not widely utilized, one reason being that many newspapers could not afford

<sup>29</sup> [1, Editor-in-Chief of *Engineering News* to Secretary, Dec 21 1916]. Young replied on Jan 24 1917, "if a protest will be of any avail at this time I should be glad to enter one for myself *and for the Association* and to ask others to co-operate." (Italics supplied.)

It is amusing to note that the Association's determined effort to be regarded as a scientific society bore unexpected fruit when its members were denied the standard reduction of railroad fares accorded to members of "educational" organizations.

<sup>30</sup> A resolution to this effect was passed at the Minneapolis meeting in 1913 and again at the 1924 annual meeting.

On another occasion the Secretary suggested that although the Association could not "take a stand" on the question of changes in the copyright law, he could insert a statement in the *Review* advising interested members to exert pressure on their Congressman [1, Westerfield to M. L. Rainey, Jan 9 1923].

<sup>31</sup> [1, Secretary, Department of Commerce, to Westerfield, Apr 4 1925; Westerfield to Dept. of Comm., Apr 14 1925]. Westerfield's response was somewhat surprising in view of the Association's earlier cooperation with federal departments, especially in connection with the Census.

to antagonize advertisers of non-essentials. This obstacle emphasizes the very real need of the intelligent and patriotic help of economists.

. . . Any suggestions as to further committees which might be appointed or as to activities in which the Association or its members might engage which would be of value to the country in the present crisis, would be greatly appreciated.<sup>33</sup>

From the constitutional point of view, the crux of the matter was the almost scholastic distinction contained in Fisher's introductory observation: "As you know, the Association, *as such*, does not engage in propaganda. The appeal is to the members individually,"<sup>33</sup> for this rubric could be used to cover a multitude of sins. The best example of the dangers of this distinction is provided by the postwar enthusiasm for the peace movement. Although the executive committee was reluctant to give an open vote in favor of the League of Nations' Resolutions, it recommended the incoming President (H. B. Gardner) to "submit them to each individual member with any recommendation he might care to make."<sup>34</sup> In response to this request, a circular signed by Gardner was dispatched to all members stating:

In accordance with established precedent no action in the name of the Association was taken by the Association or its Executive Committee, but there was a general feeling that the matter was of such great importance that it should be brought to the attention of the individual members of the Association. Fully concurring, as I do, in this feeling, I herewith transmit the resolutions and urge each member of the Association to immediately express his convictions on the question to the congressmen and senators from his state.

While action in the name of the Association is open to objection, there is no question that the membership of the Association should make its influence felt in the decision of a question of such tremendous importance not only for our own nation but for the future of the world.<sup>35</sup>

<sup>33</sup> [1, Fisher to members, Mar 9 1918, italics supplied.]

<sup>34</sup> *Ibid.* (Italics supplied.)

<sup>35</sup> [1, Fisher to Gardner, Jan 21 1919]. On Jan 2 1919, Fisher had explained to Gardner, in a letter informing him of his election to the Presidency of the A.E.A., "it was thought best by the Executive Committee not to pass it, partly because of our precedents and partly because we did not think the resolution would be as effective as individual letters." Gardner could, if he wished, express his own opinion. "In any case, it would not commit the Association as such."

<sup>36</sup> [1, Gardner to members, Feb 11 1919]. The resolutions advocated the establishment of a "League of Nations to enforce Peace and Justice throughout the world," and asserted that "we favor the entrance of the United States into such a League of Nations as may be adequate to safeguard peace." As a sequel to this incident, in January, 1921, the A.E.A. received urgent requests from William Howard Taft, President of the League to Enforce Peace, asking them to send two representatives to a national meeting in Washington, but there is no evidence that this request was granted. Possibly by this time the sense of emergency had passed and strict constitutionalism reigned once more. In 1923 the Asso-

Presumably, when the future of the world was at stake, it was permissible to stretch the A.E.A. constitution to its limit; but it has been well said that of all constitutional arguments the most damnable is that of emergency, and, in taking such liberties with its customary practices, the Association was running a considerable risk. Apparently the gamble was successful, for there were no protests and this proved to be an isolated incident. There were, however, some who doubted the advisability of the activist policy undertaken during the war itself.

One of these cautious spirits was Frank A. Fetter, an erstwhile Secretary and President, who was always sensitive to the scholarly and scientific properties. Even at the height of the wartime enthusiasm he wrote:

I hope that the committee on prices will be restricted to the field which, as I understand it, the Executive Committee meant to authorize it to survey—viz. the problem of price inflation in this war. It will be *ultra vires* and will involve the Association in activities contrary to our constitution if it tries to wander over all creation and to direct the general economic policies of the nation in this whole war [1, Fetter to Young (n.d.), 1918].

The kind of danger that Fetter envisaged was revealed in a statement published by one A.E.A. committee which announced that its work was

... intended to put into popular language some of the simple economic principles or *precepts* connected with the Purchasing Power of Money, and particularly to emphasize the importance of saving money with which to buy Liberty Bonds and pay taxes instead of lending to the Government merely what we borrow of the banks, which procedure is apt indirectly to raise commodity prices.<sup>30</sup>

To those members who feared that such publications would commit the Association, *as an association*, to any given policy recommendations there was, of course, a standard reply, namely that the reports of A.E.A. committees did not constitute official policy positions, but merely the views of the authors. This position was consistently maintained, as for example in the Secretary's report of 1900, which stated rather vaguely that "the Association vouches for [a report] as far,

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ciation refused to allow the American Peace Award to ballot its members in connection with a national referendum on the question of a plan for international peace. [1, Secretary to American Peace Award, Jan 3 1924].

<sup>30</sup> [1, *italics supplied*]. A copy of this statement approved by the Committee on the Purchasing Power of Money is in the 1918 file.

and no further, than for the works of other authors which it issues,"<sup>37</sup> or, as in a secretarial letter of 1926, that

The responsibility for the ideas contained therein rests entirely with the individuals who draft these reports and cannot, in fact, be said to express the attitude of the Association, as an organization. Any expression of opinion contained in such reports must stand on their own merits.<sup>38</sup>

Nevertheless, as Judge Dill remarked in connection with the appointment of representatives as observers of or delegates to other organizations,<sup>39</sup> despite the Association's good intentions it seems likely that most readers attached much more weight to an authorized report signed by a number of leading economists, especially when it was circulated by a government department, than to a regular leading article in the *Review*.

During and after the war, on the crest of a wave of enthusiasm for empirical research, there was a lengthy discussion of various prospects to establish links between the American Economic Association and the Institute for Economic Research (usually known as the Brookings Institute). Initially there was some possibility that the Association might sponsor or even found such an institution;<sup>40</sup> but long after the Institute had been inaugurated with the aid of funds provided by Robert S. Brookings, it was suggested that the A.E.A. should establish its permanent office in the Institute's building in Washington [1, cf. H. Seager (President of the A.E.A.) to Westerfield, Nov 10 1922]. Even in the 1890's, the desirability of such an office and a paid secretary (on the lines of the American Historical Association) had been debated, but although the matter was still under consideration as late as 1928, no action was taken,<sup>41</sup> and the Secretary's office has retained its present location at Northwestern University since 1925. As always, one of the main reasons for the Association's reluctance to identify itself with another body, even a reputable independent research or-

<sup>37</sup> [2, pp. 47-48]. The occasion was the publication of a volume of *Essays in Colonial Finance*, with the aid of funds subscribed by W. E. Dodge, T. Marburg, I. N. Seligman, S. Wood, and the late T. G. Shearman. From time to time other businessmen provided funds for similar purposes.

<sup>38</sup> [1, Secretary to R. C. Reusch, Dec 7 1926]. He explained that the publications of the War Finance Committee (1919) were "the only expression of opinion [on the subject of the issuance of war bonds] that could be regarded as the attitude of the A.E.A."

<sup>39</sup> *Supra*, p. 277.

<sup>40</sup> [1, See the report by E. D. Jones submitted to Young, dated Dec 9 1919. Also W. H. Hamilton to Jones, Nov 18 1919; Fisher to Gardner, Jun 4 1919.]

<sup>41</sup> [1, See, for example, Deibler to T. S. Adams, Jan 7 1928, and Oct 12 1928; Deibler to Dewey, May 8 1928; Dewey to Deibler, May 21 1928; E. C. Day to Deibler, May 28 1928.]

ganization, was the fear that this might in some sense commit the American Economic Association to support of the findings of the Institute's publications. This concern was fully revealed in 1925, when a proposal to launch a large-scale investigation of "the present method of industrial price fixing and price quotation in relation to economic theory" was advanced by H. Parker Willis.<sup>42</sup> This was not the first proposal of its kind; and during the war clear precedents in favor of such a scheme had been established. Nevertheless, many prominent members were opposed to it for a variety of reasons. Allyn Young (then President of the A.E.A.), for example, considered that "there are a hundred other questions of equal importance. I believe it to be particularly unwise that the Association should have any direct connection with the investigation of any one economic problem, no matter how important." Carl R. Plehn held that the Association "should not lend our name and become morally responsible without control," and considered that the wartime undertakings were "exceptional." And even Thomas W. Page, who was President of the Institute for Economic Research and a long-standing member of the Association, expressed his inability to understand "how such an investigation could be undertaken by an independent agency subject to the approval and under the direction of a special committee of the Economic Association. I see no harm in conferring with such agencies as may be deemed competent, but I gravely doubt whether any satisfactory arrangement for cooperation can be made."<sup>43</sup>

As it transpired, no investigation was undertaken, mainly because neither the Institute nor any other suitable research body was able to provide the requisite services on terms within the Association's financial means. Here, as elsewhere, the outcome of long deliberation was dictated as much by fortuitous circumstances as by sensitivity to fundamental questions of principle; but there seems little reason to doubt that the strong opposition to any active A.E.A. intervention in questions of current economic importance was a significant factor in the situation.

<sup>42</sup> [1] This is cited from Willis' motion, as amended by J. R. Commons, and submitted to the Executive Committee in December, 1925.

<sup>43</sup> See Young's handwritten comment on the back of a circular dispatched to members of the Executive Committee [1, Jan 23 1925; also Young to Westerfield, Feb 18 1925; Plehn to Westerfield, Feb 16 1925; also Page to Westerfield, Jan 29 1925]. Plehn also added: "The subject is hot with controversy. Hence any advisory committee should see that its proceedings are of record in such a manner as to clearly show how far it took responsibility, if at all. That 'if at all' means that in my own opinion a *competent* bureau would prefer to assume full responsibility." (Italics in original.) Some members of the Committee either expressed no opinion or raised no objections to the proposal. For the final outcome, see Young's undated circular to members of the Executive Committee [1, (c. Oct 1925) and Deibler to Young, Oct 29 1925].

Finally, under the general heading of policy questions, the conduct and content of the annual meetings of the Association were occasionally the subject of heated controversy. According to the activist view propounded by Carver and others, the selection of topics and speakers assumed almost legislative proportions for, as he informed one U.S. Senator, "all sorts of public measures are discussed, and we try in that way to influence public opinion, but not by officially endorsing a measure by any formal vote [1, Carver to C. S. Page, Oct. 2 1912]. Obviously the program of the annual meeting does afford some guide to the subjects deemed worthy of engaging the members' time and attention, and it is incumbent upon the organizers to secure competent and representative speakers. But in the period under consideration highly topical and controversial matters were sometimes discussed by spokesmen of various interest groups or by economists engaged in studying these questions, and there were inevitably some occasions when critics complained that the organizers had failed to provide a properly balanced treatment of the issues at stake.<sup>44</sup> The planning of the annual meetings, therefore, came to be regarded as yet another matter in which it was vital that the Association should adhere to a policy of strict neutrality.<sup>45</sup>

#### V. *The Attainment of Financial Independence*

The final episode of this story exemplifies the perennial interaction of the forces of continuity and change in historical narrative. Like other scholarly associations in the United States and elsewhere, the war brought a serious setback in the Association's finances owing to a fall in membership income and a sharp rise in costs, and consequently in the early 1920's a third, and what has proved to be a final, membership campaign was undertaken. Once again the funds were provided by a group of businessmen who, as earlier, had been persuaded by a few wealthy academic economists to come to the Association's rescue.<sup>46</sup> Once again a Membership Committee was estab-

<sup>44</sup> See, for example, J. D. Hubbard, a Chicago employer, who appeared on a "Closed and Open Shop" panel discussion in 1904, and subsequently complained bitterly that most speakers were biased against employers: "As a fair and open discussion, the Symposium must be deemed an absolute failure," [1, Hubbard to Taussig, Dec 31 1904]. Similarly, with reference to the representativeness of H. C. Adams' views on public utility corporations see [1, H. T. Newcomb to Dewey, Sep 7 1909].

<sup>45</sup> As in very recent years, there were periodic complaints that the same names appeared too often on the annual programs and that young men and lesser-known (especially mid-western) institutions were underrepresented [1, Cf. Fetter to Taussig, Aug 28 1905; J. H. Raymond to Carver, May 1 1912; Deibler to H. H. Burbank, Oct 12 1925].

<sup>46</sup> [1]. In January, 1919, the Association received \$5,000, the outstanding balance of a sum of \$20,000 which had been provided to support the publication of studies in war finance, and in September, 1922, a special finances subcommittee was set up under the

lished (which included Roger W. Babson), and energetic solicitation among nonacademic as well as academic groups produced a large crop of new recruits.<sup>47</sup> This time, however, the American Economic Association was incorporated in Washington in 1923; a permanent endowment fund was raised with the aid of established members and interested outsiders; and the investment of these funds subsequently formed the basis of the Association's now almost indecently healthy capital position.<sup>48</sup>

There were, of course, favorable external conditions that contributed to this happy solution of the Association's bread and butter problems—especially the contemporary prosperity of business, and the growing number of full-time economists employed by universities, colleges (especially Schools of Business), business enterprises, government agencies, and independent research institutions. By the second half of the 1920's the organized economists had, in effect, succeeded in raising themselves by the businessmen's bootstraps to an enviable position of independence, a position which their predecessors had for so long unsuccessfully striven to attain. Naturally there were still some delicate policy problems to be decided, and, as previous paragraphs have indicated, the variety of needs and interests of the Association's membership meant that its officers were never able to satisfy all of their constituents all of the time.

Nevertheless, in the years before the impact of the Great Depression was felt, the American Economic Association finally managed to consolidate and husband its resources and had at last reached its long-delayed maturity.

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chairmanship of E.R.A. Seligman. In May, 1921 this committee included H. W. Farnam, W. F. Gephart, H. R. Hatfield, J. Hollander, J. T. Holdsworth, E. Johnson, L. C. Marshall, and F. W. Taussig, several of whom had given sums of money to the Association on more than one previous occasion. By the following year \$10,000 had been raised from a group of "thirty gentlemen," and by January, 1923 Seligman was hoping to raise a further \$3,000 a year for three years by means of members' contributions to a permanent endowment fund.

<sup>47</sup> [1]. In January, 1925 it was estimated that the campaign had brought in 629 permanent new members during the period 1922 to 1924. The Secretary's reports show the following total membership figures: 1917—2602; 1920—2866; 1924—3547; 1925—3746; 1927—3507; 1929—3748. For earlier figures see *supra*, note 5.

<sup>48</sup> [1]. On February 29, 1928, the Secretary wrote to H. C. Freeman that the Association now had an accumulated balance of \$30,000. Mr. Freeman accepted an invitation to become a subscribing member.

3. ———, *Secretary's Report, Twenty-fourth Annual Meeting, Am. Econ. Rev., Proc.*, March 1912, 2, 132-34.
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## THE DEPLETION ALLOWANCE IN THE CONTEXT OF CARTELIZATION

By ALFRED E. KAHN\*

The economic commentary on the special tax privileges accorded our extractive industries, most importantly oil and gas, has increased sharply in both volume and rigor in recent years, despite the apparent political immovability of the object of all this attention. Meanwhile a far more extensive literature has accumulated over the last several decades concerning the peculiar market structure and performance of the oil industry, and in particular its combination of state production control and import limitations, which in the name of conservation and national security, respectively, in effect organize the industry into a cartel; the relatively moderate concentration in a domestic market crowded with huge vertically integrated firms; and, more recently, public regulation of the price and interstate sales of natural gas.

In my judgment, the literature and public debate in the first category have for the most part failed to make systematic contact with the second—and conversely. The “industrial organization” studies in general do not give careful analytical consideration to the economic consequences of the depletion allowance (to use a short-cut expression for a more complex phenomenon);<sup>1</sup> and almost without exception, to my knowledge, the “public finance” literature has inadequately considered the implications and merits of the tax arrangements in the light of the comprehensive cartelization of the crude oil market. (For one outstanding exception, see [37].) The present paper attempts to remedy the latter omission.

Paul Davidson has taken several important steps in the direction of bridging this gap, less by actually constructing the necessary links than by incorporating in one article a consideration of (a) the allocative effects of the tax preferences, (b) the impact of production control on the time pattern of resource utilization, and (c) the effect of the tax system on the motivations of vertically integrated companies

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<sup>1</sup>For convenience we will continue to use this rather loose characterization of an inter-related bundle of tax provisions, described more precisely in Section I, below.

with respect to the pricing of crude oil [7]. After a brief description of the relevant tax provisions, the present paper begins by examining the Davidson argument that the benefits are eaten up entirely by economic rents. Then, having rejected the view that the allowance does not significantly affect investment, it summarizes and appraises various arguments about whether the stimulus it imparts to investment contributes positively or negatively to the neutrality of the tax system. These arguments, it will demonstrate, virtually ignore the peculiar structure of this industry and, because of this, often go astray in identifying the effects of the allowance. The article then attempts to demonstrate explicitly the bearing of prorationing and import restrictions on the allocational and national security defenses of these tax provisions.

### I. *The Relevant Tax Provisions*

The most important privileges available to recipients of income from oil and gas production<sup>2</sup> involve three options that must be considered together: (a) to charge against income from any source, in the year in which they are incurred, the major investment expenditures—for dry holes and the intangible costs of drilling and equipping productive wells;<sup>3</sup> (b) to compute depletion expense on either a cost

<sup>2</sup> These include landowners, on whose bonuses and royalties percentage depletion may be taken [17, p. 1482] [30, p. 375] [4, p. 375, n. 63].

<sup>3</sup> In the legal sense, the option of charging off dry-hole costs is not a privilege peculiar to oil; the treatment is analogous to that of research expenditures, for example. On the other hand, to accord this treatment to the intangible costs of productive wells is clearly a special privilege in every sense. In any event, the discrepancy between what the economist and the tax collector define as capital expenditures is clearly extraordinarily large in this industry. The industry's domestic expenditures in 1959 and 1960 for exploration and development, exclusive of overheads, were estimated by the *Joint Association Survey* [58] as follows (in \$'000,000):

	1959	1960
Exploration		
Dry-hole costs	821	774
Lease acquisition	554	626
Geological and geophysical	320	277
Lease rentals	193	193
Other	124	175
Total	<u>2,012</u>	<u>2,045</u>
Development		
Producing wells	1,830	1,651
Equipping leases	483	431
Total	<u>2,313</u>	<u>2,082</u>

The dry-hole costs and most of the rentals are deductible expenses. Those of the geological, geophysical, and other expenses that do not eventuate in acquisition of leases can be similarly written off; the remaining portion, and the lease-acquisition costs themselves, both of which must be capitalized, can be written off as ordinary losses when a lease is

basis or as  $27\frac{1}{2}$  per cent of gross revenue up to 50 per cent of net income from the property;<sup>4</sup> (c) to report as capital gains the proceeds from the sale of producing properties net of whatever costs remain not previously charged off.<sup>5</sup> It is the combination of (a) with (b) that grossly inflates the tendency of the latter to permit companies to recover more than original investment cost free of tax: when the major costs have already been charged against income, the option thereafter to take depletion as a percentage of revenue without regard to costs becomes correspondingly attractive.<sup>6</sup> Conversely, (a) alone, like any accelerated amortization, would permit an alteration only in the timing of tax, amounting to an interest-free loan to the taxpayer; but combining (a) with (b) tears up the promissory note. Similarly the combination of (a) with (c) permits the operator first to reduce his tax by 52 per cent (at the corporate rate) or up to 91 per cent (at the individual rate) of the capital expenditure; then to pay a maximum tax of 25 per cent on the return of these outlays when he sells the property.<sup>7</sup> Moreover, the availability of option (c) represents an alternative, for the explorer, to operating the property himself and availing himself of (b). Any diminution of (b) could therefore be in large measure evaded if route (c) remained open [30, pp. 377-79]. On the other side, the buyer can afford to pay a

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abandoned [17, pp. 1481-82] [4, pp. 375-80, 513-16]. Of the development costs, estimates of the intangible portion—i.e., those expenditures that do not produce salvageable assets—run from 50 to 75 per cent, with 60 per cent perhaps the most reasonable approximation. (Interviews by H. Peter Burckmeyer (unpublished); also [25, p. 1021]). For estimates of the combined importance of these options, see Steiner's survey [46, p. 960]. All of these options are typically taken.

<sup>4</sup> McDonald and Galvin supply lucid descriptions of these options [28, pp. 325-27] [17, pp. 1463-65]. In practice the net income limitation does reduce the percentage depletion actually taken. McDonald estimates effective depletion in the industry is around 22 per cent, and its net benefit as compared with cost depletion around 20 per cent, of gross revenue; in his judgment the figure would be lower if information were available for firms reporting net losses for income tax purposes [28, pp. 328-29]. On the problem of defining the property unit, see n. 18, below.

<sup>5</sup> The last, again, see n. 3, above, is not technically a special privilege unique to extractive industries. But it is peculiarly important in oil.

<sup>6</sup> Moreover, to the extent the investment costs are not chargeable to particular producing properties (e.g., exploratory dry holes), or are charged off before production begins, they reduce the constricting character of the 50 per cent-of-net-income limitation, for net income to this extent tends to approach gross. The President's 1963 tax proposals sought to reduce this opportunity of escaping the 50 per cent limit by requiring that all such deductions attributable to particular producing properties be carried forward and counted as expenses in computing net income after production commences [52, pp. 17-18, 108-10].

<sup>7</sup> The President therefore proposed in his 1963 tax message that the portion of gross receipts equivalent to costs (including depletion) previously charged off against income be taxed at ordinary income tax rates [52, pp. 18, 136-38]. The House Ways and Means Committee first adopted a limited version of this proposal, then rejected it entirely [*Wall Street Journal*, June 18, 1963, p. 4; Aug. 6, 1963, p. 3].

price high enough to remunerate the discoverer for giving up option (b) because he can, as in any other industry, write it off on a cost-of-acquisition basis, regardless of how much of the original cost the discoverer has already charged off under (a).

## II. *The Effect on Supply: A Preliminary View*

### A. *The Long-Run and Economic Rents*

Davidson's argument, it will be recalled, is that the contribution of percentage depletion to investment is slight or nil<sup>8</sup> because its benefits go primarily or entirely into economic rents. And this is so, he avers, because the supply of uncommitted oil leases is completely inelastic. If this reasoning were correct, it would make practically superfluous all further serious consideration of the merits of the allowance.<sup>9</sup> Duncan R. Campbell has, in my opinion, supplied the appropriate refutation—that Davidson's reasoning would be valid only if all potentially oil-bearing lands were homogeneous in quality[6].

If the total supply of potentially oil-bearing leases were both homogeneous and completely inelastic, there would be no rents paid today at all—oil leases would be a free good—since market demand does not intersect any such supply schedule at any point.<sup>10</sup> Most potentially oil-bearing land is in fact not under lease at any rent; and some of the land that is under lease commands very high rentals. Rents are paid because these lands are nonhomogeneous, so that the fixed and inelastic supplies of leases of certain degrees of attractiveness are indeed intersected by derived demand curves (while others are not). And rents on such inframarginal lands rise as the margin of cultivation is extended downward and outward by application of additional real resources to exploration and development. It is precisely *because* and *as* provision of a tax subsidy hastens this process that it increases rents.

Still, Davidson is certainly right to point out that whatever economic case may be made for these tax concessions is weakened to the extent that they merely increase rents. The effects of the subsidy on supply, on the one hand, and rent, on the other, are mutually an-

<sup>8</sup> Compare [6] [7, pp. 86, 104, and 106] [8]. A nil effect would seem to follow from his assertions about the perfect inelasticity in the supply of presently uncommitted lands.

<sup>9</sup> Cf. Steiner: "virtually everyone concedes that present tax concessions do stimulate production and income (and thus induce a higher level of investment and production)." [47, p. 951, n. 6].

<sup>10</sup> According to A. D. Zapp, it is unlikely that more than 20 per cent of the drilling required "for even near-exhaustive exploration" of the area "that has favorable prospects for the discovery of petroleum" has as yet been done in the United States, even excluding Alaska and strata below 20,000 feet [56, pp. H-23-24].

tagonistic—the more it increases the one, the less it increases the other. How far, if at all, it moves the supply curve to the right, rather than increases rents, depends directly, as Stephen L. McDonald has pointed out in a related connection, on the elasticity of supply of oil as determined by the continuity and slope of the “curve of land quality gradations” [30, p. 376]: the more readily available are submarginal prospects of not much poorer quality than those currently at the margin, the more will the subsidy shift the supply of oil to the right, and the less will it increase rents. In McDonald’s judgment this “gradient of alternative site qualities” is gentle; he therefore attaches little importance to the effect of the allowances on rents.<sup>11</sup>

An indirect reflection of the relative importance of these two opposing tendencies is provided in the size of rental payments in this industry. In 1959-60 about 18.5 per cent of its total capital expenditure was merely payment to landowners for the right to probe beneath their soil (see note 3, above). In addition, some 15 per cent of the gross receipts from oil and gas production went in royalties [58]. Appropriately adjusted, these two can be added; a reasonable approximation might be that 25 to 30 per cent of the industry’s gross revenues, hence 32 to 38 per cent of its value-added, represented economic rents.<sup>12</sup> This estimate suggests that the slope in the qualities of

<sup>11</sup> See [30, p. 376] and [29, p. 322, n. 29]. Actually McDonald’s discussion pertains to the relative impacts of the corporation income tax and percentage depletion on rents on the one hand and *price* on the other. He therefore introduces as the second determinant the elasticity of demand for oil and gas. His judgment that this elasticity is low reinforces his view that the allowance does much more to decrease price than to increase rent.

<sup>12</sup> See the estimates, of similar dimensions, by Bain [2, pp. 80-84]. The rough estimate in the text is based principally on computations made for me by Martin Bernstein, of National Economic Research Associates, Inc., from statistical summaries of the detailed reports supplied for 1960 by producing companies in the Permian Basin to the Federal Power Commission (*In the Matter of Area Rate Proceeding, et al. [Permian Basin Area]*, Docket No. AR 61-1, *et al.*, Exhibits 142, 149, 153 and 154). Total net working interest revenue of the reporting companies was about 60 per cent of the industry-wide total estimated by the *Joint Association Survey*. The rents are computed as including (1) royalties, estimated, following the JAS, at 15 per cent of gross revenues; (2) cancelled, surrendered, and abandoned leases (i.e., charged off against income in 1960), with a rough adjustment to take out some capitalized geological and geophysical expense associated with the abandoned leases; (3) amortization of nonproducing leases; (4) lease rentals and options; (5) (cost) depletion charged on producing leases; and (6) a share of net income before federal income tax, proportional to the share that net investment in producing and non-producing leaseholds constituted of total net investment. The total of these items amounted to approximately 30 per cent of estimated gross revenues.

I have chosen to state the figure rather at 25 to 30 per cent because it would appear the above computations must involve counting the costs of leases acquired by one producing company from another, in which event there are included among rents some reimbursements of costs of a nonrent character (e.g., of dry holes and of a return on investment) incurred by the original exploring company.

potentially oil-productive land is far from gentle: so do the average bonuses of \$233 to \$2,267 per acre offered by successful bidders for federal leases in the Gulf of Mexico in auctions held in the 1959-62 period.<sup>13</sup> Clearly the market at any given time places a far higher valuation on *some* intra- than on extra-marginal land. This does not prove that there is a steep slope at the margin itself, within the range affected by the depletion allowance; but the height of total payments suggests that a large part—I lack the information required for greater specificity<sup>14</sup>—of the tax benefit goes to swell rents, and a correspondingly reduced part into expanding productive capacity.

At the same time, as we will argue more fully below, the depletion allowance does make an important contribution to increasing the real exploratory and developmental effort, and thus (to a lesser extent) to increasing productive capacity. We therefore turn, in Section III below, to the question of whether the increase in rents may not be a necessary price for achieving the optimal allocation of resources into this industry.

### B. Short-Run Shifting

Davidson recognizes that his argument about economic rents does not apply (equally) to presently producing properties. The terms of their leases having already been fixed, any additional tax incident to

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The adjustment to the value-added basis is derived from the ratio of value-added to total value of shipments and receipts in 1958 [60, 1961 ed., p. 254].

Thus, it would appear, there is some exaggeration in Davidson's assertion that "the major costs in oil exploration and development are economic rents" [7, p. 103]. The exaggeration apparently resulted from adding together the industry's annual royalties, which averaged about \$1,370 millions in 1959-60 [58], and lease-acquisition costs and rentals, and comparing that total with the industry's total exploratory and developmental outlays (see n. 3, above). This addition leaves out of account the sharp difference in the distribution of the two kinds of payment over time. Royalties are paid out of revenues (which averaged \$9.1 billions in 1959-60, according to the JAS) over the producing life of the properties.

<sup>13</sup> *Petroleum Press Service*, Sept. 1959, p. 363; April 1960, pp. 149-50; also [45, p. 0 38]. Moreover, offshore royalties are fixed at  $\frac{1}{6}$  rather than the typical  $\frac{1}{8}$  of gross revenues (*Oil and Gas Jour.*, April 1, 1963, pp. 78-79). The fact that the principal explanations of the highest bonuses are the nearness of the leases in question to known producing formations—knowledge of what lies beneath the surface may be a more important aspect of a lease's economic quality than its "original and indestructible powers"—and, on federal leases, the prospect of being able to operate as a unit any reservoirs that may be discovered in no way vitiates the observation that the economic quality of such leases is far higher than of submarginal ones.

<sup>14</sup> The preliminary results of a study by Franklin Fisher for Resources for the Future indicate strongly a far greater price elasticity in the supply of exploratory effort than of actual discoveries. These findings, if substantiated, provide specific and direct validation of the inference drawn here. They also illuminate the other side of the coin: the diminishing returns, hence the rising real unit costs, attributable to a subsidy that shifts the margin of cultivation outward.

reduction of the depletion allowance could not (in full measure) be shifted back to their owners.<sup>15</sup> Thus withdrawal of the allowance might, like the imposition of an excise tax, tend to shift the industry's short-run supply curve upward.<sup>16</sup> He deprecates this possibility, however, for two reasons: first, that any resultant tendency for product prices to rise would generate pressures to relax import quotas and state production controls; and, second, that the effect would also be to force the abandonment of some high-cost wells, in consequence of which production allowables for the typically lower-cost, controlled wells could be increased: "thus, the short-run supply curve of the industry could shift outwards rather than upwards" [7, p. 106].

The virtue of this reasoning is precisely that it attempts to analyze the effects of the tax in the context of the industry's cartelization. But, unfortunately, it is much more difficult to predict the reactions of a politically directed cartel than of a profit-maximizing entrepreneur. My own judgment is that, for the following reasons, withdrawal of percentage depletion (a) would not significantly move short-term supply curves to the left, (b) would certainly not move them to the right, (c) would have a more marked effect in curtailing intermediate and long-run capacity, and (d) could well provide the pretext, though hardly an economic justification, for an early price increase.

1. Production from so-called marginal or stripper wells accounts for only about one-fifth of the national total [60, 1961 ed., pp. 33-34]. And most of these are almost certainly not marginal in the economic sense: probably only a very minor proportion would shut down or even appreciably reduce output if the effective price declined from, say, \$3.43 to \$3.00.<sup>17</sup>

<sup>15</sup> I have added the parenthetical qualifications in recognition of the fact that the portion of rent taking the form of royalties is a variable cost (see [7, pp. 90-91]) and can be sloughed off by a failure to continue production or to develop these properties. If, because of a reduction in the depletion allowance, gross revenues (after tax) cease to be large enough for the portion of them remaining with the operator, net of royalty, to cover the relevant incremental costs, the landowner will bear part of the burden.

<sup>16</sup> Davidson's excise tax analogy is extremely illuminating in explaining how an alteration in the effective tax on net income can alter short-term production decisions, something that is at first blush difficult to credit. The reason is that the depletion deduction is computed as a percentage of gross income. So long as operators have enough net income from the property unit, as defined for tax purposes, so that the amount they may take in percentage depletion is not restricted by the fact that it may not exceed 50 per cent of net, percentage depletion in effect makes each added dollar of revenue worth something like 14.3 cents more than it would otherwise be (this being the tax saving, at a 52 per cent rate, on the 27½ per cent of gross revenue it permits to be charged against taxable income). The allowance thus has the same effect as a 14.3 per cent negative excise: an added \$3.00 of revenue from an additional barrel of production becomes worth something like \$3.43.

<sup>17</sup> See n. 16, above. Although these wells are supposed to be defined in terms of the

2. The value of the percentage depletion subsidy is limited to 50 per cent of net income from the property unit. This restriction is most stringent for marginal operations.<sup>18</sup> In consequence, the effect of withdrawing the allowance is likely to be considerably less than is suggested by the \$3.43 to \$3.00 price reduction.

3. Production of the far more important wells subject to prorationing is so restricted by state authorities that they undoubtedly operate far short of the point of rising marginal cost and of its intersection with their individual marginal revenue curves. There is no reason, therefore, why withdrawal of the subsidy would automatically curtail their supply. On the other hand, there is no reason to believe it would expand it appreciably either, and certainly not in such a way as to move the total industry supply curve to the right.<sup>19</sup>

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marginality of their operations, classification in practice runs principally in terms of the low volume of their daily production, and whether or not they are on the pump [57, pp. 73-75, 322, 329]. Neither of these criteria necessarily means imminent shutdown in the event of any given decline in price. On the fuzziness of their alleged marginality, see [9, p. 235]. Uren cites studies of production costs in 1931-34 showing operating costs averaging 48 cents, 41 cents, and 41 cents a barrel, respectively, for so-called stripper wells in the Mid-Continent and Gulf Coast area producing under 3, 6, and 12 b/d; the posted December low prices for 36° Mid-Continent crude in these four years were \$0.77, \$0.69, \$1.00, and \$1.00, respectively. Other estimates which he cites suggest that a price increase of 20 per cent in 1945 would, by extending the economic life of strippers, have added only 1½ per cent, a 40 per cent price increase 6 per cent, a 60 per cent increase 9 per cent to total recoverable national reserves [55, pp. 24 and 499] [60, Centennial Edition, 1959, p. 374].

<sup>18</sup> [17, p. 1462, n. 61]. To the extent operators can combine properties for tax purposes, in particular grouping some that have relatively large net income with others that have relatively high gross, they can and do mitigate this limitation and come closer to taking the full 27½ per cent deduction. It was to prevent this practice that the President proposed, and the House Ways and Means Committee agreed, to require producers to compute their allowable depletion on a lease-by-lease basis. According to the Treasury, most small operators (who operate most of the stripper wells) compute their depletion in this manner already [*Wall Street Journal*, June 18, 1963, p. 4; Aug. 6, 1963, p. 3] [52, pp. 18, 110-12].

<sup>19</sup> Suppose one were to envisage prorationing as in effect organizing the generally lower-cost wells, whose output alone is limited, as a single, profit-maximizing unit, subject to the limitation that the uncontrolled, higher-cost stripper wells will always be produced up to the point where their marginal cost equals price. The presence of the stripper fringe would hold price lower than the single-firm monopoly level: the demand curve for the flush wells would have greater elasticity than the total industry demand because a lower price would cut out some of the marginal, uncontrolled production. The latter tendency would be the greater, the farther to the right (i.e., the larger the market share at any given price) the supply curve of the marginal producers. Withdrawal of the depletion allowance (under the Davidson assumptions) would raise that curve by 14.3 per cent. Thus the effect of eliminating some marginal production, if any, would be to *raise* the profit-maximizing price for the controlled wells; their output would thus increase by less than stripper production had declined.

Of course, in fact, the nature and motivations of the state regulatory authorities can not be so simply characterized.

4. To the extent stripper output declines, it is not at all clear the states would obligingly permit expanded output from flowing wells sufficient to keep prices from rising. On the contrary, withdrawal of the depletion allowance would provide an excellent occasion and pretext for letting prices go up. Such a price increase, it need hardly be emphasized, would not be the automatic consequence of market forces, competitive or otherwise; with supply subject to stringent control, this sort of short-run reaction could occur only as an act of policy.

The upshot of these considerations is that the burden of withdrawal of the percentage depletion privilege could very well be subject to short-run forward shifting. As Richard A. Musgrave has pointed out, a rise in the corporation income tax could signal a price increase by a monopolist or an imperfectly collusive oligopoly, where the price previously achievable did not maximize industry profit [35, pp. 280-82]. Investment target pretax rates of return in these industries might thereafter be correspondingly adjusted to sustain—even in the face of an inelastic supply of capital in the economy at large—the higher pretax margins required for shifting, provided price remained below the entry-precluding level. So in oil, except that, given the large number of sellers and relative ease of entry, the process would have to be somewhat different. There is a sufficient element of administrative discretion in the pricing of crude oil—where changes in posted prices by major buyers are influenced among other considerations by trends in cost and effectuated or preserved by production cutbacks [9, Ch. 8]—to permit upward adjustment to changes in income tax rates. Once this short-run shifting of a reduced depletion allowance occurred, there would be neither danger of its reversal nor need for its support by longer-run processes, since after-tax rates of return would have merely been restored to their previous level. We return to a consideration of the implications of this possibility below.

It is less clear that the depletion allowance itself or its increased value since 1940, in a world of rising income tax rates, could have been shifted forward (in reduced prices) by such short-run processes. The principal effect has been, rather, to stimulate investment (and increase rents); any tendency it may have had to hold down prices would have been principally long- and intermediate-run in character.<sup>20</sup> We turn therefore to the question of whether the rate of investment it has encouraged has been the appropriate one.

<sup>20</sup> The short-run costs of production, even for secondary recovery operations, are preponderantly fixed [9, pp. 67-68]. At that point however where additional investment is required to sustain the rate of flow from reservoirs already discovered and producing, a point being reached almost daily in one place or other, reduction in the effective price of oil would have an effect. Percentage depletion surely shifts these intermediate- as well as long-run supply curves outward.

### III. *The Issue of Allocational Neutrality*

The major objection offered by economists to percentage depletion (to continue use of this shorthand reference to the whole tax package) is that it results in a misallocation of resources. Since it is the rate of return after tax (net of any required provision for risk) that tends, given free entry, to be equated at the margin between various investment outlets, whereas it is the rate of return before tax that approximates the marginal product of capital, these preferences, which tax net income much less heavily in oil than in other industries,<sup>21</sup> must produce an excessive flow of capital into oil [15, pp. 227-29] [21] [22] [46]. The fact that net returns on investment *after* tax in the oil industry may not be unusually high<sup>22</sup> thus constitutes not a defense of the allowance but the clearest possible proof of the misallocation it causes.

An extremely interesting answer to this criticism has been presented (first, to my knowledge) by Richard J. Gonzalez [18, pp. 993-94], then more thoroughly formulated and tested by McDonald [28] [30], based on the fact that the corporation income tax is itself not neutral in its allocational effects. To the extent that tax is shifted forward, it produces a disproportionately great increase in the price, hence a disproportionate discouragement to utilization, of products where (corporate) profits are high in relation to sales.<sup>23</sup> The latter condition, the argument runs, will prevail particularly in industries—like oil—with two characteristics: a low rate of capital turnover and a high normal return on investment because of above-average risk. Therefore, the depletion allowance, which is in effect a flat percentage reduction in tax in relation to sales, tends to offset or correct the misallocation produced by the corporate income tax's bulking large relative to sales in oil.<sup>24</sup>

The McDonald argument was subjected to challenging criticism by Musgrave [34] and Douglas H. Eldridge [14], to which he in turn re-

<sup>21</sup> McDonald points out that the respective ratios of federal income tax to net before-tax income were 14 per cent and 50 per cent in 1958 [30, p. 370].

<sup>22</sup> This is a matter of dispute. Interestingly, sometimes this alleged fact and sometimes the very opposite one are both cited in defense of the depletion allowance, the first in answer to criticisms that the preferences permit excessive profits, the second in support of the view that risks in oil are unusually great and justify some special tax concession. See, e.g., [18, pp. 994, 998-99].

<sup>23</sup> If the corporate tax rate were 50 per cent, full forward shifting would entail price increases by the ratio of profits to sales: industry A, with a price of \$1 and profits of \$0.05 per unit before imposition of the tax, would succeed in raising its price to \$1.05; industry B, with a price originally at \$1 and unit profits of \$0.20, would have its price raised to \$1.20.

<sup>24</sup> In his original article [28], McDonald made an ingenious set of estimates of these two effects, which seemed to show that the one roughly offsets the other. Subsequent consideration, and a comment by Eldridge [14, p. 215], led him to modify this arithmetic conclusion [29, p. 325].

sponded [29]. The discussion here attempts to summarize the conclusions to be drawn from those discussions as a prelude to taking into account, as they did not, the industry's cartelization.

#### A. *Is the Corporation Income Tax Shifted?*

First, Musgrave pointed out, the argument unrealistically assumes that the corporation income tax is wholly shifted forward (but cf. his later findings [24]). In fact, full forward shifting is necessary to the validity only of McDonald's arithmetic demonstration of *how large* a depletion allowance is required to offset the differential impact of the corporation income tax. Musgrave would apparently not deny that some shifting occurs—to the extent (a) the aggregate supply of savings or its flow into investment is responsive to the net rate of return (compare [20, pp. 216, 235-36] and [35, pp. 278-79, 333] [34, p. 208]); (b) the aggregate supply of savings is diminished because of the disproportionate burden of the tax on incomes (corporate and personal) a high proportion of which tends to be saved [13 pp. 231-32] [43, pp. 224-25]; (c) capital can shift between the corporate and noncorporate sectors; (d) there is short-run shifting, as we have already surmised could well occur.<sup>25</sup> The upshot would seem to be that McDonald's observation remains conceptually sound but is possibly subject to some quantitative discount.

A related qualification, pointed out by McDonald himself, is the possibility that the burden of the tax, and the offsetting benefits of the depletion allowance, are to some extent shifted backward to recipients of economic rent. The more this is so, as we have seen, the less the tax raises price, hence distorts resource allocation, and the less purpose the allowance serves as an offset. We have already alluded to McDonald's judgment that this qualification is not of great importance; and we have offered some indirect evidence to the contrary. In any event, the possibility of backward shifting is relevant only to the quantitative importance of the argument, not to its principle. Backward shifting of the initial burden to rentiers occurs only because the margin is retracted from its optimum point. The fact that a return to that optimum may principally restore economic rents and only slightly increase production capacity (if the slope in qualities of oil-bearing lands is steep) or only slightly reduce price (if supply, as defined, is inelastic, or demand elastic) does not destroy the case *on allocational grounds* for returning.

Of course, the Pareto optimum is not the only proper criterion of public policy. It is relevant to consider, additionally, how high a price

<sup>25</sup> The Musgrave-McDonald exchange does not consider the last possibility at all, further illustrating the initial point of this paper, that the "public finance" literature has given inadequate consideration to the peculiar institutional characteristics of this industry.

in the application of scarce resources to less-promising prospects by marginal operators and in unearned increment has to be paid for the results achieved (compare [11, pp. 412-15] and [26, pp. 255-62]). The economist might still prefer the allocational optimum, accompanied by heavy taxes on the rents. But the rents in oil are not only not taxed away but, *mirabile dictu*, themselves enjoy the tax benefits of percentage depletion!<sup>28</sup>

What *does* strike at the principle of the McDonald exposition, and not merely at its quantitative importance, is the possibility that the demand for the industry's products is relatively inelastic, as he himself avers. The more this is so relative to other industries, the less a relative increase in the price of oil and gas, consequent on the forward shifting of the corporation income tax, necessarily implies *anything* with respect to its allocational neutrality. By the same token, however, the allegedly misallocating effect of the depletion allowance itself would equally be exaggerated. This observation is closely linked with the major thesis of the present paper: If this industry were purely competitive, as most discussions of its tax privileges implicitly assume, the allocational impact of these arrangements would, in my judgment, be of relatively minor importance. It is the industry's cartelization that makes them particularly objectionable, as we shall see.

### B. Profits and Risks in Oil

A second set of questions centers on whether returns on investment in oil are really above average; whether, if so, they represent a necessary reward for unusual risk; and whether the risk-premium component of return on sales, if any, need in fact be reflected in a correspondingly above-average increase in the price of oil even if the corporation income tax is fully shifted forward.

On the last point, Musgrave states that since an income tax with perfect loss-offsets reduces risk, "the tax imposed on the component of profits which constitutes return for risk need not be recouped by higher price . . ." [34, p. 206]. McDonald takes as the measure of the industry's required minimum rate of return (in the presence or absence of an income tax) after-tax profits as observed in the period 1949-56. Then, on the assumption that the tax was in fact shifted forward, he is able to estimate by how much the price of oil would, in the absence of percentage depletion, have been increased more than the price of other products in order to restore the observed higher profit rate. But if in

<sup>28</sup> See [30, pp. 375-76, 380] [37, p. 514], both in opposition. It is difficult to find a convincing defense. See [3]. It should be pointed out, however, that these bonuses and royalties do in fact go to the government, where public lands (including the tidelands and outer continental shelf) are involved.

fact imposition of the corporation tax with loss-offsets had reduced the required return, the price of oil would actually have risen less than this computation would show and perhaps no more than the price in other industries.

The theoretical literature concerning the effect of the corporate income tax with its actually limited loss-offsets on risk-taking investment is inconclusive (see [35, pp. 312-26] [29, pp. 319-21]). But it does appear the McDonald reasoning must be severely qualified. As Eldridge points out [14, p. 212], the unusually great dispersion of results of an individual investment expenditure in oil exploration does not inevitably necessitate a higher-than-average return on total investment in the industry. If this dispersion is the measure of risk, there may be an actual preference for the riskier of two investments promising the same average probable return, when total losses are limited to the funds actually invested and possible gains are virtually limitless: the average return on total investment in parimutuel betting is known in advance to be negative. Moreover, the industry has developed a great number and variety of institutions for minimizing risk [33, pp. 392-401]. (See also [23].)

In any event, the impact on the flow of capital into oil relative to other endeavors of the corporation income tax, on the one hand, and of percentage depletion, expensing of intangibles, and the capital gains option, on the other, cannot possibly be regarded as simply offsetting, either qualitatively or quantitatively. The one operates negatively only to the extent the total flow of capital into corporate investment is in fact responsive to its reduced yield, and demand for oil and gas together is elastic; and it probably operates positively by making the government a 50-50 partner in losses.<sup>27</sup> The other provisions have a complex pattern of effects, all of them positive. Even without percentage depletion, they cause losses to be shared by the government to the extent of 52 per cent (so far as corporations alone are concerned), gains to the extent of only 25 to 52 per cent. Moreover, percentage depletion drastically cuts down the 52 per cent maximum and skews the curve of possible returns far to the right. I hazard the strong impression that the comparative impact of the latter tax provisions on investment in oil

<sup>27</sup> The J. Keith Butters, Lawrence E. Thompson, and Lynn L. Bollinger study found that this last consideration was a relatively unimportant one for most investors. The preponderant effect of the personal income tax was to drive them into lower-yield, less-risky investments. However, they found an equally marked, opposite tendency for those investors interested mainly in capital appreciation; high income tax rates and the differential treatment of capital gains clearly drove most of them into investments offering the greatest opportunity for such appreciation, oil being one of those prominently mentioned [5, pp. 34-43]. I am indebted to my colleague, Robert W. Kilpatrick, for these observations.

far exceeds the inhibiting effect (if any) of the corporate income tax, by virtue of its unequal incidence on industries of above-average risk and capital intensity (see also [47]).

The foregoing discussion, following the literature on the subject, is framed in terms of long-run processes only, and under the implicit assumption of pure competition. In fact, however, these processes and their consequences are significantly altered by the actual institutional characteristics of this industry. What if, then, in the absence of percentage depletion, price-posting by vertically integrated, oligopsonistic buyers, fortified by state production controls, could actually have succeeded in the late 1940's in effecting—and during the 1950's in preserving—a short-run forward shift of the higher corporation income tax rates,<sup>28</sup> as we have already suggested could well occur if the depletion allowance were reduced now? In that event, McDonald's argument about the potential impact of the tax on the oil price could have been right, though for the wrong reasons.

But does the possibility of a rise in the price of oil, effected in *this* manner, constitute an equally compelling defense of the industry's tax privileges on allocational grounds? Given relative ease of entry, the net effect of the corporation income tax, shifted forward in this manner, in the face of a relatively inelastic demand, could well have been an absolutely *increased* flow of capital into oil. This would be so if the total supply of capital to corporate enterprise were inelastic and, as I surmise was the case, most other industries were either less successful than oil in short-term shifting or had higher barriers to entry. The higher price could still have been—as in fact price increases were—preserved in the face of this enhanced investment by equivalent curtailment of production allowables and the imposition of import quotas. In these circumstances, the tax preferences would have *compounded* the misallocation, rather than *corrected* it.

The significance of these observations will be clarified in the ensuing discussion of the third major aspect of McDonald's exposition, the low capital turnover in oil.

### *C. Capital Turnover in Oil and the Influence of Cartelization*

One consideration may be fairly quickly dismissed, having in effect already been resolved: The "capital," though measured on the right

<sup>28</sup> The price increases of 1946-48 would probably have occurred in a purely competitive industry as well; and greater increases could have eventuated automatically if productive capacity were smaller, as it would have been in the absence of the depletion allowance. The real contribution of cartelization was to effect additional, more modest increases in 1953 and 1957, and—probably even more important, quantitatively—to keep the progressively growing excess capacity of the 1950's from wiping out the earlier gains [9, Ch. 8].

side of the balance sheet, reflects inclusion on the left side of investment in leaseholds, valued at acquisition costs. Is it appropriate to include those economic rents, which are price-determined rather than price-determining, in ascertaining the industry's required marginal rate of return on sales? The answer is that the rents paid for superior land are merely the equivalent and consequence of higher nonrent costs at the margin. If the *average* capital turnover figure is therefore reduced relative to the *marginal* by the former's inclusion of lease acquisition costs, this is so only to the extent that it is higher than the latter with respect to other costs. The necessity of earning a return on capitalized rental payments for superior land is a reflection of the process of equating marginal costs and benefits.

A more serious question concerns the implications of the heavy and growing excess productive capacity with which the industry has been saddled since the early 1950's. One evidence of this is the progressive and drastic reduction in the number of days' production permitted controlled wells in Texas, which has borne the brunt of output restriction [9, pp. 156-63] [62, pp. 19-23]—from an average of 345 days in 1947-48 to 97 in 1962.<sup>29</sup> A comparison of the authoritative capacity estimates of the National Petroleum Council with average annual production figures charts the growth of this excess: from 1.0 million barrels a day in 1950-51 to 3.8 million in 1959-60.<sup>30</sup> As Table 1 indicates, production ran around 84 per cent of capacity in the early 1950's<sup>31</sup> then declined steadily to around 64 per cent at the end of the decade. We will consider the cause of this phenomenon at a later point; let us

<sup>29</sup> The annual figures are as follows:

1947	324 days	1951	276	1955	194	1959	123
1948	366	1952	261	1956	190	1960	104
1949	238	1953	236	1957	171	1961	101
1950	230	1954	194	1958	122	1962	97

Sources: *Second Report of the Attorney General* pursuant to Section 2 of the Joint Resolution of July 28, 1955, Consenting to an Interstate Compact to Conserve Oil and Gas, Sept. 1, 1957, p. 86; [62, May 15, 1963, p. 24]; The Railroad Commission of Texas, *Annual Report* of the Oil and Gas Division, 1960, p. 11. Texas' share in national proved reserves shows no decline over these years, while its share of national output has fallen sharply.

<sup>30</sup> The Independent Petroleum Association of America publishes annual estimates of production capacity (see [60]) running slightly lower than those of the NPC, evidently because the IPAA's estimate is of production that could be sustained for 6 to 12 months without further development. Its figure for 1/1/60 is 9,708,000 b/d, which yields an output to capacity figure in 1960 of 69.8 per cent, corresponding to the 64 per cent shown in the table. The trends are similar to those shown in the table, with output in the 1960-62 period continuing to run at just about 70 per cent of capacity.

<sup>31</sup> Actually, as the changing number of days of permitted production in Texas suggest, the 1950-51 average conceals a drop in 1949-50 and recovery in 1951-52.

TABLE 1—U.S. CRUDE OIL PRODUCTIVE CAPACITY, PROVED RESERVES, AND PRODUCTION

	1/1/51	1/1/53	7/1/54	1/1/57	1/1/60
<u>Productive capacity</u>					
(1) (000 b/d) <u>on annual basis</u>	6,727	7,465	8,331	9,867	10,585
(2) (000,000 bbls.) <u>Average production<sup>a</sup></u>	2,455	2,725	3,041	3,601	3,864
(3) (000 b/d)	5,696	6,250	6,184	6,992	6,778
(4) <u>Excess capacity (1-3)</u> (000 b/d)	1,031	1,215	2,147	2,875	3,807
<u>Production as % of capacity</u>					
(5) (3/1)	84.7	83.7	74.2	70.9	64.0
<u>Proved reserves</u>					
(6) (000,000 bbls.) <u>Annual productive capacity</u>	25,268	27,961	29,253 <sup>b</sup>	30,435	31,719
(7) <u>as % of reserves</u>	9.7	9.7	10.4	11.8	12.2

<sup>a</sup> Except for 1954, these are averages for the two years surrounding the date for which the capacity estimate is provided. The first figure, for example, is the 1950-51 average. The 1954 figure is for that single year.

<sup>b</sup> Average of beginning and end-of-year estimates.

Sources: Capacity figures [36, Table V]. Production and proved reserves from [61].

assume as proved for the moment (though it is an oversimplification) that it is the consequence of production controls—the familiar outcome of cartelization in an industry where entry is easy.

The burden of this excess capacity reduces the industry's capital turnover. If, despite this, the return on *investment* remains adequate, the industry's rate of return on *sales*—hence McDonald's assumed distorting effect of the corporation income tax and the depletion allowance required to offset it—is unnecessarily inflated.<sup>32</sup>

Does this mounting excess capacity weaken the case for percentage depletion—or strengthen it? In a sense, as McDonald's arithmetic demonstrates, it strengthens it. If we must take production control as given, we must also accept the cost of the excess capacity it imposes. The tax privileges keep investors contented with a price lower than it would otherwise have to be. The industry's rapidly mounting excess capacity, which was certainly attributable in part to the enhanced real value of the tax preferences after World War II, in a context of higher general income tax rates, probably did make it more difficult to raise prices as unit costs seemed to mount and profit margins to decline in the 1950's: the power of the cartel over price was not unlimited; cut-backs in Texas were partially offset by expanding output in states not

<sup>32</sup> This idea was first suggested to me by my graduate student, Dane J. Cox.

practicing similar restraint and by rising imports. From this perspective, then—taking production control as an unalterable fact of life—one might well argue on allocational grounds for an allowance even more generous than it would otherwise have to be, in order to offset the price-boosting effect of the cartel.

But this view makes sense only to the extent that the subsidy does indeed affect quantities produced and consumed. The economic purpose served by increasing the flow of capital into an industry is to reduce the price and promote the utilization of its products. The allocational case for the depletion allowance is, thus, seriously weakened when a large proportion of the capacity thus created is merely shut in and prices are prevented from falling. While the tax allowance is tending, through its effect on investment, to reduce price relative to cost, production control thwarts that tendency and pushes up costs relative to price. When the higher costs, to which the subsidies themselves contribute, are then proffered in partial justification of those same subsidies, the argument completes the circle, and it becomes reasonable to ask whether it may not be time to break out.<sup>38</sup> Before attempting to

<sup>38</sup> The depletion controversy provides numerous intriguing illustrations of the principle of self-justifying irrationality—the tendency for irrational interferences with the free market to produce results which are then cited in justification of the original interventions. For example, reference is frequently made, in defense of the allowance, to the high and allegedly rising costs and risks of exploration. Yet the offer of the subsidy to an industry characterized by statically increasing costs is itself in some measure responsible for extending the margin of cultivation, raising real costs (including risks taken) at the margin, and rents within the margin. See, e.g., [37, p. 509].

Again, Gonzales defends the 27½ per cent rate on the ground that it usually produces tax savings less than those of cost-based depletion, so far as purchasers of proved oil properties are concerned. Oil in the ground sells for \$1 a barrel or more; this could be charged off on a cost basis, whereas, at a market price of \$3, percentage depletion would come at most to only \$0.825 [18, pp. 996-97]. What this ignores is that the \$1-a-barrel price is not independent of percentage depletion. For one thing, the availability of that option to the *seller* is an important determinant of his reservation price—a fact which Gonzales in effect concedes when he points out, in another defense of the 27½ per cent, that finders of oil are constantly weighing the alternative benefits of operating the properties themselves, and enjoying the 27½ per cent deduction, or selling out for capital gains: “any reduction of percentage depletion would swing the balance more heavily in favor of sales of reserves in the ground” [18, p. 997]. Moreover, the price buyers are willing to pay is itself a function of the cost of finding oil on their own account, and this, too, we have just pointed out, is influenced by the subsidy.

Another argument frequently encountered is that the importance of cheap fuel for economic growth justifies retaining these special inducements. The argument is often made but never rigorously developed. All sorts of possible applications of resources might contribute importantly to growth; there remains the inescapable problem of choosing among them. Until parents are permitted to expense such intangibles as their children's tuition costs, and to charge off depletion—even on a cost basis—for investment in education, it is an open question whether the particular subsidy to the consumption of gasoline by college students in sports cars is on balance justified.

A final illustration—perhaps better characterized in this context as the principle that one irrationality deserves another—is the argument that one cannot condemn the alloca-

answer, however, we will have to consider whether that circle may not also be encompassing national interests other than optimal resource allocation.

#### IV. *Excess Capacity, Cartelization, and National Security*

From time to time the view has been attributed to military authorities that national security requires we keep some one to two million barrels a day of production capacity in reserve for national emergencies. The goal, it should be noted, is capacity that is not just excessive, by ordinary economic standards, but also unutilized. (Industries like agriculture, bituminous coal, and textiles have been periodically or chronically plagued by excess capacity; but one of their main difficulties has been an inability to keep the excess *idle*. See, e.g., [42, pp. 736-39].) This view in effect constitutes a strong defense of the very combination of institutions here under examination.

It is production control, not the depletion allowance, that is principally responsible for the oil industry's holding its excess capacity idle. Certainly it would not remain idle if we were to return to the law of capture, either without restriction or within the limits of maximum efficient rates of production (MER). And if we adopted the only rational system of organization—unit operation of reservoirs—excess capacity of the kind that our present, irrational method of exploitation fosters would not come into being in the first place, the subsequent discussion will more fully show.<sup>34</sup>

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tional consequences of the depletion allowance without also considering the heavy taxes that directly and indirectly discourage the consumption of gasoline. See, e.g. [18, pp. 999-1000]. A survey of the relevant data and literature leaves the strong impression that we can not yet say with assurance whether the motorist is overtaxed, and perhaps we never will. Does a verdict of unproved for the entire package then vindicate percentage depletion? In principle, yes—partly. But, first of all, taxes on one final product, even the most important one, can never correctly offset or be offset by subsidies on the raw material. Second, as a practical matter, it seems intolerable to have to decide about everything before deciding about anything. The rule that one alleged wrong deserves another can lead to a stagnation of policy-making. Finally, if our argument is correct, that the depletion allowance leads more to an accumulation of idle capacity than to lower prices and enhanced utilization, the subsidy could not in any event offset the allocational effects of overtaxing the motorist. The two problems can therefore be confronted separately.

<sup>34</sup> The present system of production control is certainly far superior to the preceding one, competitive exploitation *under the law of capture*. But it is important to understand wherein the improvement lay, and that is the reason for emphasizing the qualifying phrase. The critical flaw is not always explicitly identified; the impression is therefore sometimes left that competition itself is inherently infeasible in oil.

Competition *as a force in the market* does not require that every individual worker be "in business for himself" *within* the individual producing establishment. On the contrary, it assumes a centralized control based on property rights coterminous with the integrated unit of production. So in the case of oil: compulsory unitization, by eliminating the insanity of competitive exploitation of the individual reservoir, would, as Davidson

This conclusion requires some qualification: the tax privileges are not entirely blameless for the industry's system of control, the wide gap it maintains between price and costs of efficient production, and hence for both the excess capacity and its enforced idleness. Percentage depletion creates an artificial incentive on the part of vertically integrated refiners to take their profits at the production end of the business, hence probably makes them more ardent supporters than they would otherwise be of the cartel-sustained price of crude oil.<sup>35</sup> The large purchasers

shows, make it possible for each physical establishment to be run rationally while escaping the severe defects of prorationing to market demand. See also [32, pp. 110-21] [55, pp. 175-83, 572]. There has been no demonstration that competition in the market place would not then perform its social role of coordinating the behavior of these individual, profit-maximizing units. See [9, Ch. 10] [44, Ch. 6], a pioneering discussion. Cf. [1, pp. 55-58].

What of the alleged threat of destructive competition in the presence of heavy fixed costs in oil? In pointing out that depreciation and depletion are typically charged on a unit-of-production basis—i.e., only as each barrel of oil or mcf of gas is produced—rather than automatically with the passage of time, de Chazeau and I were concerned to deprecate this alleged danger, observing that, in the absence of a rule of capture, market competition would be subject to the ability of each (unitized) producer to withhold from producing today what he feels might be more valuable tomorrow [9, p. 72, n. 23 and pp. 71-72, 239, n. 17, 243]. We erred at one point, as Davidson shows [7, pp. 87-88, n. 5], in characterizing depreciation and depletion on this account as variable in the economic sense, rather than fixed—i.e., in confusing these book charges with user costs. The capital outlays these accounting entries seek to recoup are indeed sunk, and these charges *as such* do not enter into the decision whether or not to produce. What does however remain valid is the basic point that the low ratio of variable to total *production* costs would not in itself make competition destructive, absent the rule of capture; that, given unitization, it would be only if market price covered also (discounted) marginal user costs that production would actually be undertaken and the book depreciation and depletion charges be actually recorded. See [7, pp. 91-94, 97-98].

<sup>35</sup> See [33, p. 155] [9, pp. 221-29] [7, pp. 98-100] [27, pp. 102-6]; also [2, pp. 106-18], supplying additional reasons for the major companies' interest in a high and stable price of crude oil.

McDonald has challenged the relevancy of this motivation: given the freedom of entry into production, he avers, "it is a motivation without power" [29, p. 317, n. 18]. The principal answers, I believe, are the following: (1) Until the imposition of import quotas, in 1959, the world oil price was tied to the U.S.; conditions of entry and independence of action in foreign oil falling far short of effectively competitive, a rigged U.S. price therefore meant greater monopoly profits than otherwise for the international companies. (2) Free entry tends to bring down to "normal" levels the profits of the "representative producer." It does not eradicate the supernormal returns of the successful operators, and specifically of the "representative majors." The discrepancy in profitability between successful and unsuccessful firms is probably unusually great in this business. See, e.g., the comparative profits of firms showing net income and of all reporting firms in [14, p. 215]. On the enduring advantages of large producers, see [31, pp. 550-51, 556-57]. This is another way of saying that entry is not really so utterly free in oil as is suggested by the fact that new ventures are constantly being formed—attracted in large measure by the tax arrangements that make it possible for investments to be privately successful even when total outlays exceed total receipts (see [37, p. 510] [22, pp. 18-19]). A high price that induces a lot of wildcatters to drill dry holes will mean higher profits than a lower price for the big company that has tied up the more promising acreage and con-

of oil, who post the field price, are typically reported as arguing on balance on the side of more stringent cutbacks of production allowables at the monthly meetings of the Texas Railroad Commission;<sup>86</sup> and this pressure does in fact amount to a call for a higher price, hence (given an inelastic demand) a greater stimulus to investment relative to the permitted level of production than if the market were more competitively organized. But clearly the price policies of the integrated companies would be far less effective in the absence of prorationing and import quotas; to these government controls must be attributed the principal responsibility.

Apart then from its possible contribution to the organization and functioning of the cartel, the depletion allowance does not itself encourage holding discovered oil in reserve. On the contrary, its benefits come only as the oil is actually produced.<sup>87</sup> The contribution of the tax privileges to the alleged national security goal, rather, is to make it possible for investors to tolerate, and by their continued investment to maintain, a larger capacity, idle or otherwise, at any given price, than they otherwise could or would.

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trols the wet holes. (3) Even if a rigged price could not guarantee supernormal profits, the major refiners might still prefer a higher price *cum* excess capacity to a lower with less crude-oil-producing capacity, in view of their long-run interest in continuity and adequacy of supplies. Meanwhile they can and do exert their influence to induce state authorities to curtail by direct, well-spacing regulation the kinds of competitive entry and development most costly to them. (4) To the extent supernormal profits in production are eroded away by secret concessions from posted prices, possibly reflected in turn in product price reductions, the integrated company can still enjoy the tax advantage of transferring crude oil to the refining department at the posted price, taking its nominal profits at the production end.

At the same time, McDonald's observation (which he has most helpfully elaborated in private correspondence) constitutes a useful reminder of what the majors have doubtless been discovering for themselves: that they cannot raise prices indefinitely without attracting entry and forcing costly production cutbacks on themselves. This discovery corroborates the consideration raised in n. 19, above: that the presence and possible entry of independents increase the elasticity of demand and reduce the profit-maximizing price of the owners of low-cost wells. The major companies could escape this burden and increase their monopoly profits so long as they could produce abroad and bring in low-cost foreign oil without restriction under the U.S. price umbrella. But this door too has now been partly closed, and the foreign price level cut loose by import quotas from the domestic. In the circumstances, Paul H. Frankel raises the interesting question "to what extent is proration as we know it now still as acceptable to the biggest companies, as it undoubtedly was 30 years ago?" [16, p. 67].

<sup>86</sup> See the monthly reports of these meetings in the *Wall Street Journal*.

<sup>87</sup> See [37, pp. 510-11] [19, pp. 433-34]. Nelson suggests that the stimulus imparted by percentage depletion is almost entirely to development rather than exploration. I do not follow his reasoning. True, the producer reaps no tax benefits until production commences; but it is equally true of all investment and regardless of the tax system that profits wait upon revenue. Whatever increases the prospective flow of net revenues after tax surely encourages both longer- and shorter-term expenditures to expand output.

But what precisely is the nature of this excess capacity that has grown so sharply since 1950-51? The table in note 29 and Table 1, above, suggest that it has certain peculiar characteristics. It is apparently *not* an excess of proved reserves: the ratio of proved reserves to annual production (the so-called Life Index) has shown no trend whatever since World War II.<sup>38</sup> Thus "production capacity" seems to have increased steadily relative to proved reserves—from 9.7 per cent at the outset of 1951 to 12.2 per cent nine years later, as the table shows, and higher since then.<sup>39</sup>

In short, the excess capacity of recent years apparently consists entirely of an ability to take out of the ground more rapidly than before oil that has already been discovered.<sup>40</sup> This trend is the consequence of many factors, including improved well-completion and secondary recovery techniques. But one prominent factor has been intensive developmental drilling of known or already discovered reservoirs. Actually, developmental drilling did not increase relative to exploratory over the entire postwar period. Under the influence of various short-term stimuli—the enhanced value of the tax preferences because of sharply rising income tax rates, the rapidly growing demand for oil and

<sup>38</sup> The average of the annual ratios in the 1955-62 period was slightly over 12.4 years; for 1946-54, it was not quite 12.5 (computations from [61], comparing yearly production with end-of-year reserves). In 1951, for example, when allowables were 276 days and output at some 90 per cent of capacity, the Life Index was at 12.4 years; in 1962, when controlled wells in Texas were permitted a puny 97 days, and excess capacity nationally was estimated at 3.5 to 4 million b/d (and output averaged 7 million), it was 12.3.

<sup>39</sup> The IPAA production capacity estimates (see n. 30, above) were 11.2, 11.4, 11.6 and 11.8 per cent of proved reserves at the outset of 1960, 1961, 1962, and 1963, respectively.

<sup>40</sup> The foregoing statistics understate the extent to which this is so. Estimates of proved reserves are recomputed annually (and typically upward) as a result of two processes: new discoveries, consequent on exploration, estimated as proved at the end of the year of discovery; and revisions and extensions (typically positive) of previously estimated discoveries, principally as developmental drilling demonstrates how much was in fact discovered in preceding years.

Extensions and revisions have accounted for by far the greater proportion of total additions to proved reserves and have showed some tendency to increase in relative importance since World War II: 82.7 per cent of the total in 1946-50, 84.3 per cent in 1951-56, 86.8 per cent in 1957-62. These in turn represent an increase over the previous decade: the ratios were 78.6 per cent in 1937-40 and 80.0 per cent in 1941-45. Computations from [61].

Thus the development of known reservoirs has been reflected not only in growing estimated productive capacity but also in growing estimates of proved reserves, via revisions and extensions. So in the ratios shown above, rising from 9.7 to 12.2 per cent, developmental drilling has been increasing the denominator (via extensions and revisions) as well as the numerator, hence underestimating the extent to which we have merely been more intensively cultivating and exploiting fields already discovered rather than finding new ones.

gas, and general anticipations of inflation—exploratory drilling increased very sharply during and after World War II relative to the total (see [60, Centennial edition, 1959, pp. 16, 22]). But then, as permitted days of production were sharply curtailed, pay-out periods painfully extended, and the burden of excess capacity mounted, it was exploratory drilling that was disproportionately discouraged: the number of wildcats drilled in 1962 was 31 per cent while all other (primarily developmental) well completions were only 18 per cent below their respective 1956 peaks.<sup>41</sup> Over the entire period, development of known reservoirs, including secondary recovery, has tended to gain relative to new discovery.<sup>42</sup>

The economic explanation of these respective trends in exploration and development seems to lie in two sets of causes. The first, a combination of market imperfections with aspects of the cobweb and acceleration phenomena, is fairly obvious: the lead time between the commencement of exploratory activity and of production typically involving several years, exploration overresponded both to the enhanced incentives of 1940-53 and—in the opposite direction—to the industry's less buoyant prospects after 1956.

The second cause was prorationing. A cartel that, by controlling production, holds price far above the short- and long-run marginal costs of the more efficient producers but does not effectively restrict entry will inevitably produce overinvestment. The burden of this waste falls differentially on exploratory as against developmental drilling. For the explorer its only effect is to promise a long-delayed pay-out if he does in fact strike oil: the cutbacks in output fall mainly on the lower-cost and generally the newer wells. For the individual producer sitting atop a known reservoir, in contrast, the drilling of another developmental well promises an almost certain (only one in four to one in five development wells proves to be dry) additional ticket of admission to the cartel; it guarantees an additional quota, at the rigged price, hence the

<sup>41</sup> Calculated from data reported in the *Annual Review-Forecast* issues of *The Oil and Gas Journal* (late in January of each year).

<sup>42</sup> See also n. 40, above. Further evidence of the same trend has been the sharp and continuous increase in the relative importance of stripper and secondary recovery production, from 15.2 to 22.3 per cent of the national total between 1951 and 1960—a trend only partially explained by the progressive cutbacks of allowables on more productive wells. See [48, p. 18].

Again, between the 1950-51 average and 1961, production from other than stripper wells rose 15.8 per cent, nonstripper proved reserves 22.1 per cent, and the number of producing nonstripper wells by no less than 33.2 per cent. These divergencies in trend appeared entirely after 1955-56. All computations, from [60] and [48], were made by subtracting the stripper data (using only "primary" stripper reserves) from the national totals. I am indebted to Herman Roseman for helping me work out the logic of this comparison.

possibility of a faster pay-out. So it pays a leaseholder on proved properties to drill more wells than he would in the absence of production controls or if reservoirs were unitized.<sup>43</sup> The exemption of secondary recovery operations from production control has the same effect. For all producers as a group the process is of course self-defeating—additional wells and quotas merely necessitate additional cutbacks of allowables; but no one of them can afford to hold back. The result, since developmental drilling (and secondary recovery) increases the potential recovery rate, and since marginal, high-cost wells that would otherwise be forced to close down are exempt from output limitations and sheltered by the rigged price, is to widen the margin of idle capacity.

The tax laws, too, in a sense discriminate against exploratory and in favor of developmental activity. The costs of drilling and equipping wells, most of which can be charged currently against income, constitute the major part of developmental outlays. On the other hand, a large proportion of exploratory costs—notably the outlays for geological and geophysical work and lease acquisition—must be capitalized and amortized, so long as the leases in question are not abandoned. Industry spokesmen have pointed out this anomaly, urging that the expensing privilege be more fully extended to exploratory outlays as well [25, pp. 1019-21]. The foregoing discussion suggests the advisability of reversing the discrimination. The various defenses of these special tax arrangements for oil—notably risk and low capital turnover—apply, if they apply at all, principally to exploration. The “intangible” drilling costs of developmental wells, in contrast, are really the costs of acquiring a perfectly tangible asset; how risky they really are is suggested by the widespread recourse to three- to ten-year bank loans to finance them (see, e.g., [49]); and their contribution to national productive capacity consists essentially in making it possible, at wastefully excessive cost, to take known reserves out of the ground at a more rapid rate for a relatively short time [9, pp. 160, 232-33] [36, pp. 27-28].

So our reserve productive capacity is a short-term phenomenon. The more enduring measure of capacity is proved reserves, and these not only show no rise relative to production but have (because of rising imports) declined relative to consumption since World War II. Industry spokesmen point to these facts as demonstrating that the incentives conferred by the depletion allowance ought in no way to be dimin-

<sup>43</sup> See [32, pp. 110-21] [9, pp. 236-39] [33, pp. 111-12, 381-82] [16, pp. 86-90]. Also the testimony of William J. Murray, then Chairman of the Texas Railroad Commission, in U.S. House of Representatives, Select Comm. on Small Business, *Small Business Problems Created by Petroleum Imports*, Nov. 1961, p. 113.

This is not to minimize the progress made recently by both Louisiana and Texas toward improved well-spacing regulation and giving increased weight to acreage in assigning allowables. An enormous encouragement to overdrilling remains. See [59] [10].

ished;<sup>44</sup> it would be entirely consistent with their logic and these facts to argue for making the tax incentives even more generous.

Before contemplating this piling of irrationality on irrationality, we should consider whether the case may not have been overproved. The so-called Life Index was 12.7 years in the 1920's, 13.8 in the 1930's, and 12.5 in 1946-62, with no trend apparent in the latter period (computations from [60] and [61]). Of all the oil that is underground, the amount actually "proved" at any given time is only a fraction; it is far from a full reflection even of all the oil that has already been discovered and will prove to be recoverable even with currently available techniques, let alone the far greater supplies—from oil reservoirs discovered and as yet undiscovered, from the vast oil shales and tar sands—that future technological progress and growing demand will make it economical to produce [39, pp. 7-63]. It is expensive and wasteful to do the drilling to prove out in advance more reserves than are needed as a kind of on-the-shelf inventory (see [38, tr. pp. 6336-47]). If the industry has tended, decade in and decade out, to keep an inventory about 12½ times current sales, it is questionable whether it even can, let alone ought to, be induced to hold more.

The mere fact that we can open the valves in case of national emergency and increase the flow of oil for a period of time measured in years, without any need to increase drilling, certainly must in a sense contribute to national security. But this fact cannot be appraised in isolation. The 10,585,000 b/d we could produce at the outset of 1960 were more than we could refine: total refinery capacity, some of it obsolete and inoperable, was only 9,800,000 b/d on that date [51]. Other surface facilities for storing, handling, and transporting the oil similarly did not match our ability to produce it [36, p. 5]. For all-out nuclear war the reserve is surely useless. For a longer war of attrition, the question must be posed and answered whether this is the kind of reserve we want or whether instead we may not prefer to have more oil discovered and requiring only development.<sup>45</sup> Or, to what extent, alter-

<sup>44</sup> See, e.g., *The Petroleum Situation*, Chase Manhattan Bank, April 1963, p. 3. Their argument provides another instance of self-justifying irrationality. Noting that the ratio of proved reserves to production has been roughly constant over the decades, Gonzalez asserts: "One of the best reasons for maintaining percentage depletion at existing rates is that the system has worked to encourage development of new resources at about the rate required to meet the needs of our expanding economy" [18, p. 998].

The subsidies encourage *both* exploration and production. That the two have grown roughly *pari passu*—in general, the more we produce the more we look for and find—is no indication that the rate of growth of both and their absolute levels today are the economically desirable ones.

<sup>45</sup> Jerome J. O'Brien, Director of the Department of Interior's Office of Oil and Gas, calls for a 70 per cent (7,000 wells) increase in exploratory drilling, "to maintain our reserves," "paralleled by a reduction of at least 7,000 unnecessary development wells" [40, p. 5].

natively, compressible normal civilian consumption might not serve as our principal reserve.

The choice among these and other alternatives cannot be made intelligently without considering their respective costs; and the costs of the present policies are immense. Industry representatives estimate that on the order of \$500,000,000 annually are spent drilling unnecessary developmental wells.<sup>46</sup> And this entirely apart from the additional wastes involved in denying ourselves the foreign oil that can be delivered on the East Coast at a cost \$1.25 to \$1.30 a barrel less than domestically produced crude—total output of which was over 2.5 billion barrels in 1962.<sup>47</sup> Only in terms of comparative costs can we choose between maintaining the present shut-in capacity and being prepared instead to draw upon another reserve, as yet unmentioned: the possibility of permitting production in excess of MER's, as happened for a time during World War II. It is difficult to believe that the costs of the latter recourse—which would be incurred in the future only if and as necessary and would consist of some sacrifice of ultimate primary recovery—could (appropriately discounted) possibly exceed our equivalent current expenditure on unnecessary wells, which have the principal effect of putting us in a position to use up more rapidly what has already been discovered.

### V. Conclusion

The depletion allowance does sustain real investment in oil exploration and production, though much of its benefit is dissipated in raising economic rents and in the higher real costs of the marginal operations it encourages. But production controls have seriously attenuated the connected between investment and the level of capacity, on the one hand, and the level of output and price, on the other—not surprisingly, since both industry spokesmen and the Texas Railroad Commission regard the demand for oil and gas together as highly inelastic. In these

<sup>46</sup> See M. J. Rathbone, then President of Standard of New Jersey [41, p. 5], Lyon F. Terry, of Lehman Brothers, formerly Vice President of the Chase Manhattan Bank [50, pp. 9-10] [32, p. 116] and n. 43, above.

What is superfluous or unnecessary is seldom rigorously defined in these estimates. But all carry the implication that this is drilling that does not increase ultimate recovery and would not be done if the reservoirs were unitized; hence the expenditures are justified on grounds of neither efficiency nor economy.

It perhaps goes without saying that this waste is not the fault of any individual or firm, but the consequence of a violently irrational industrial organization and control.

<sup>47</sup> It is not suggested that all our needs could be supplied from abroad at such a cost saving or that it would be desirable to do so. On the other hand, the fact that Middle Eastern reserves, already discovered and proved out, are six times ours and are produced at the rate of only about 1 per cent a year while ours are at 8 per cent is suggestive of the extremely high potential elasticity of foreign supply.

circumstances, the incremental investment encouraged by the depletion allowance has taken the form largely of idle short-term production capacity; the allowance makes investors willing and able to carry a larger cost burden of this kind than they otherwise could or would. Its withdrawal, by reducing after-tax rates of return, would curtail investment until the reduced burden of idle capacity had restored returns to their equilibrium level.

What of the possibility that the industry might instead shift its added tax burden forward, as we have concluded it well might, despite its heavy excess capacity? If it did so, the price we are paying for the system of production controls and the system of import limitations that reinforces it would be no greater than before; it would merely be clear for all to see, instead of concealed by the tax subsidy. Perhaps this development would increase the likelihood of a more rational public policy along these fronts as well. Absent such reform, the excess capacity would remain, its burden carried more by the consumer and less by the general taxpayer.

The question then is whether the excess capacity is nonetheless worth having in the interest of national security and worth its cost. It would be an amazing historical coincidence if it were. The complex of regulations and incentives surrounding this industry could not by the remotest stretch of the imagination be described as having been evolved in accordance with some prior military determination; and of course the cost is not incorporated in the defense budget.<sup>48</sup>

These costs are of an entirely different character, as well as order of magnitude, from those of any misallocations of resources to which percentage depletion might (or, following McDonald, might not) give rise in a purely competitive situation. In such a context, the net loss would merely be the difference in the contribution to consumer satisfaction from marginal bundles of inputs applied to oil and gas rather than to other goods and services. But in the context of cartelization, most if not all of the additional short-term production capacity that the tax allowances encourage investors to bring into being is both unused and, except for the claimed contribution to national security, unnecessary; the marginal social product of the investment is zero.

It would seem difficult for an economist to justify the retention of arrangements that conflict so violently—I think most disinterested observers would agree—with common notions of equity and make investors willing to commit so egregious a waste of resources.

<sup>48</sup> "If there is one thing clear about our domestic crude oil policy, it is that it has never been frankly explained to the citizens who pay the price, nor fully debated by the Congress that has enacted, piecemeal, the legislation under which production is restrained, imports restricted, and exploration tax-subsidized" [12, p. 299].

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## QUANTITATIVE ANALYSIS OF THE COLLECTIVE FARM MARKET

By JERZY F. KARCZ\*

In the course of the campaign to eliminate private trade in the USSR during the early stages of rapid industrialization, an effort was made to channel the entire marketable surplus of farm products through state distribution outlets. These proved unable to handle the task with the means placed at their disposal, and the continuing pressure of rising demand upon declining output resulted in the emergence of a *de jure* black market. Faced with this situation, the Soviet leadership found it convenient in 1932 to resurrect the ancient, and but recently and officially discarded, institution of the so-called bazaar trade (or farmers' markets). These markets were re-established under a new name and subjected to strict government controls [25]. Ever since, the collective farm market (hereafter briefly referred to as "the market") has continued to function as the only market in the otherwise tightly planned economy where prices are established by the interplay of demand and supply.

During the last decade, the importance of the market in the Soviet distribution system has declined, but its position as a source of urban food supplies is still very respectable. According to an official statistical handbook, it accounted for 14.1 per cent of "sales of comparable food products" in 1961 [30, p. 632]. All averages, however, can be made to cover a variety of sins, and it appears that this one is no exception. Around 1960-61, the share of the market in "total sales to population" was said to exceed 50 per cent for a number of products [1, p. 54]. In 1957, when the corresponding handbook percentage was 18.2, the share of the market in total physical volume of sales to population was 63 per cent for potatoes, 48 per cent for eggs, 35 per cent for vegetables, and 23 per cent for meat products [6, p. 15].<sup>1</sup>

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<sup>1</sup>In 1957, the share of the market in all purchases by families of those employed in industry was: 28 per cent for meat, 26 per cent for fresh milk, 54 per cent for eggs, 67 per cent for potatoes, and 39 per cent for vegetables [21, p. 8].

Collective farm market continues to provide a substantial proportion of total money income of the agricultural population, especially in the collective farm sector. Given our present state of knowledge on this subject, it is not possible to obtain the exact share of market sales in this income, but some impression of its importance may be gained from the following figures. In 1957, total distributions by collective farms as labor day payments to their own members have been estimated at 41-48 billion (old) rubles [16, p. 276] [17, p. 318], while market sales by collective farm members (including rather insignificant sales by other Soviet citizens who also trade on the market) were 40.5 billion rubles [6, p. 89].

The knowledge of quantities of various farm products marketed through collective farm market, as well as of prices prevailing in such transactions, is essential to Western analysts for many purposes, including the estimation of national income accounts, calculation of average farm prices, construction of food balance sheets, and the like. Unfortunately, data of this sort have not been published recently. Under the circumstances, specialists on the Soviet economy (including the writer) have been forced to make their own estimates, based *inter alia* on official Soviet price and quantity indices for the total "extra-village collective farm market trade" (to be defined presently). The validity of these estimates, however, has been beclouded by uncertainty as to the coverage of some underlying Soviet data as well as by some confusion as to the relationship of collective farm market prices to either state retail prices or to prices realized in the so-called commission trade.<sup>2</sup>

It is the purpose of this article to discuss the nature of market statistics with the aim of clarifying some of the issues of their interpretation and to present a series of estimates of "extra-village" market prices as well as of quantities sold on this market during the years 1940, 1950-62. My estimates are based partly on information provided by a rather inaccessible but obviously very well-informed source [6] and in part on the official data of the Central Statistical Administration. Except for a few rather insignificant departures, the estimates obtained are consistent with the official Soviet indices referred to above. For reasons of space, the description of methods has been reduced to a minimum; details are set forth in a mimeographed appendix obtainable from the writer on request.

<sup>2</sup> The term "commission trade" describes sales on the market through the intermediary of cooperatives who collected a commission from 1953 through 1960. In 1961, this trade was replaced in part by the urban trade of the cooperatives, who now buy from producers at local market prices and sell at the market prices ruling in the city where the sale to the consumer takes place.

The article deals exclusively with the Soviet collective farm market, but it is believed that many of the problems discussed here are also relevant for the study of similar "farmers' markets" which survive in other countries of the Sino-Soviet bloc. Since marketing without middlemen also plays a substantial role in underdeveloped countries generally, the issues raised by the Soviet experience would seem to have an even wider applicability.

## I

### A. Terminology

The term "collective farm market trade" (*kollektivnaya torgovlia*) has been used in the USSR to describe a variety of economic phenomena.<sup>3</sup> The most inclusive definition of this trade refers to all sales of farm products (including sugar) by collective farms and their members to buyers other than state procurement agencies. This definition covers, therefore, not only sales at market-determined prices (or prices agreed upon by buyers and sellers) within or without the agricultural sector, but also sales of farm products, at prices controlled by the state, to institutions other than state procurement agencies. A more restrictive concept eliminates sales to institutions (known also as decentralized procurements) but still includes the "intra-rural" turnover or sales within the agricultural sector. Another definition covers the so-called "extra-village collective farm market," defined in official sources as "sales of farm products by collective farms, collective farmers and other individuals who own a subsidiary farm plot to non-agricultural population and to institutions at prices determined on the market" [28, p. 708]. This concept, to which the published Soviet indices as well as the official data on the value of sales refer, includes therefore sales to rural but nonagricultural population. There are, however, even more restrictive definitions of "collective farm market trade." The term has been used on occasions to refer to urban trade only, to sales within markets of 251 cities where basic statistical information is collected, or to even smaller samples of 101 or 71 large cities.

Unfortunately, Soviet economists have never been exposed in a systematic fashion to Professor Fritz Machlup's strictures on pitfalls resulting from confusing use of terminology. Generally speaking, they use the term "collective farm market" very loosely. Thus, although

<sup>3</sup> For a more complete discussion of this aspect see [7, pp. 97-101] and [10, Appendix C]. Dr. Naum Jasny seems to imply that differences in definitions might partly be accounted for by the use of different Russian terms, such as "markets" instead of "market." This might well be true at times. But many Soviet economists use the various Russian terms interchangeably [1, p. 44] and this appears to be the more general practice.

the late Professor M. M. Lifits (who had also served as the deputy minister of trade for the USSR) once spoke explicitly of "urban collective farm markets" [13, p. 285-86], a closer examination of the evidence made it clear that he was actually describing developments on markets of the 251 sample cities alone [31, p. 185].<sup>4</sup>

### B. *Soviet Data on the Market*

It will be useful to begin with a brief description of Soviet data-collecting methods, as this will enable us better to appreciate the reliability of the various Soviet figures. Fortunately, a fairly full description of these processes has just been published by an outstanding authority on the subject [1].<sup>5</sup>

For reasons that will become clear immediately, Soviet authorities are unable to determine the quantities sold on the market (however defined) or prices ruling in market transaction through enumeration. Market sales take place in the USSR on some eight thousand urban and rural markets set aside for this purpose by local authorities. Many transactions that would fall within the scope of extra-village market sales take place outside the boundaries of these markets, either through home delivery, through street sales (nominally illegal), or through other informal arrangements [10, pp. 131-32] [22, p. 32].

Data on the volume of market sales of 72 products are gathered daily on all markets located in 251 Soviet cities by enumerators employed by market administrations. In addition to sight estimates, the enumerators (whose professional and educational qualifications have been described as "not high, as a rule" [1, p. 29]) also utilize information provided by the would-be seller at the time of his entry on the territory of the market [1, p. 26].

Price information is gathered by personnel of state statistical agencies for 73 products (including sugar distributed to farmers involved directly in beet production at discount prices) on the 25th day of each month *only*, and for the most part only on the main market(s) of each of the 251 sample cities. In addition, price registration for 17 products takes place on the Sunday closest to the 25th day of the month on markets located in all *raion* centers (county seats), which may or may not be classified as urban. An attempt is made to register actual (as

<sup>4</sup>In [8, p. 449] Dr. Naum Jasny assumes that "every statement of the market trade pertaining to the present and the past [refers] to the total trade as *now* [italics in original] officially defined as ex[tra] village trade, unless the opposite is definitely obvious from the source." It would have been far better to conclude that such an assumption is permissible only if it is supported by the sources explicitly.

<sup>5</sup>I. K. Beliaevskii wrote a dissertation to satisfy requirements for the *kandidat* degree in economics on this subject, as well as several other articles. As his recent book includes all the material needed here, reference is made to [1] only in order to economize space.

opposed to "bid" or "ask") prices at the time of the most active trade. In all cases, the prices registered are to refer to first-grade products only (and this is said to lead later to "some overstatement of the value of trade turnover") [1, pp. 18, 26-28].

With two exceptions (potatoes and vegetables during the summer months), price quotations obtained in this fashion are converted into average monthly prices by means of unweighted averaging of prices registered during the current and the preceding month. Thus, the June price is taken to be the unweighted average of quotations for May 25 and June 25. In further calculations, though, average monthly prices are weighted by quantities sold. Some difficulties arise in the calculation of the average price for a territorial unit, such as a republic. Since no quantity data are collected in the *raion* centers, the average republic price is derived as an unweighted average of prices collected on all of its markets [1, pp. 57-58].

The sample of 251 cities referred to above is far from representative. This has been made clear by the results of a general survey conducted during the week of April 10-16, 1957 on 3,888 markets of 2,813 cities and 1,809 markets of 1,787 rural *raion* centers. The sample cities include 58.1 per cent of total population of cities and urban settlements and, during the week referred to, accounted for 61.2 per cent of the total value of trade (defined presumably in this instance as trade on markets of surveyed and sample cities taken together) [1, pp. 37, 47]. But the percentage of urban population accounted for by towns of less than 10,000 inhabitants in the 251 sample cities is only 0.1 per cent, whereas the corresponding percentage in the universe is 8.7 per cent. The corresponding figures for cities of 10,000-20,000 and 20,000-50,000 inhabitants are, respectively: 0.5 and 10.3 per cent; 4.3 and 15.6 per cent [1, p. 30]. As a result, the sample of 251 cities cannot be described as truly stratified. In the words of the leading Soviet authority on market statistics [1, p. 37]:

... one can agree that the existing [data collecting] procedures aim at the description of collective farm market trade in large cities. But the question then arises: why does [the sample] include a very insignificant number of smaller cities? Data collected there cannot be representative and do not exert a significant influence on averages and relative indicators of the sample.

Such, briefly, is the basic raw material gathered by Soviet authorities for the purpose of calculating price and quantity data on the market. As the reader will have noticed, the information obtained leaves much to be desired as to representativeness though not neces-

sarily as to comparability.<sup>6</sup> Among the outstanding shortcomings are: lack of quantity data for the *raion* centers (where price data are gathered), the practice of obtaining prices for first-grade products only, and the use of unweighted averages for the purpose of calculating average monthly prices. In all fairness to Soviet statistical agencies, it should be added here that the nature of the observed phenomena does not render their task a smooth and easy one, even though the first shortcoming could easily be remedied without much additional expenditure of real resources.

### C. Soviet Estimating Procedures

Our understanding of data on the extra-village market, published annually in official statistical handbooks, would be vastly improved if the information on procedures used in the compilation of the series were made freely available. Regrettably, this is not the case. We have known for some time which index-number formulas are used in the calculation of price and quantity indices,<sup>7</sup> and we know what procedures are used in the aggregation of data collected in 251 sample cities. It is also known that the Central Statistical Administration makes no calculations describing the urban (as opposed to the extra-village) market trade. But Soviet sources are silent on the exact methods of generalizing on the basis of the sample.

Strictly speaking, this remains an unanswered question. But two Soviet sources [1, p. 44] [19, pp. 32-33] provide enough clues to conclude that, in addition to data on trade in sample cities, the statistical agencies make substantial use of information provided by annual reports of collective farms and those institutions which purchase farm products on the markets as such [33, pp. 63-64] or directly from producers at market-determined, or mutually agreeable, prices. Furthermore, data derived from sample budgets of collective farmers as well as those of workers and employees are employed extensively in this connection.

The reliability of these data leaves much to be desired. The sample of workers' budgets was described in 1957 as "not sufficiently representative of urban population" generally [23, p. 31]. Until 1959, no

<sup>6</sup> Except insofar as the size of the sample changed over time. During the 'thirties, price information was gathered at ten-day intervals rather than once a month, as is the case today.

<sup>7</sup> The justification for the use of the Paasche formula for price indices may be of interest. According to [1, p. 73] this is "inherently" superior to the Laspeyres formula in this context as it immediately reveals the "savings" realized by Soviet population as a result of price declines. As far as the collective farm market is concerned the argument is obviously fallacious. Needless to say, it was advanced before the increase in state retail prices of meat (30 per cent) and butter (25 per cent) [26].

sample budgets were collected from agricultural families other than those of collective farmers, and sales by such families were assumed to take place at rates determined from budgets of collective farmers [19, p. 33]. We cannot be sure that financial reports of institutions buying on the market are fully reliable, as these may show a bias towards underreporting, especially with respect to prices. Finally, corrections are also made (we do not know upon what basis) for sales taking place outside of the territory of the markets.

Under the circumstances, one statement can be made with confidence: with the exception of data referring to 251 sample cities, the volume data on extra-village market sales are Soviet estimates, the reliability of which varies inversely with the increase in scope of collective farm market trade.<sup>8</sup> The information on prices obviously falls into the same category but, for reasons already noted, its reliability is even more questionable. In spite of all this, however, various Soviet official data represent the best information that is available to us on this important subject, even though one is tempted occasionally to classify it somewhat facetiously among the "best Soviet informed guesses."

#### *D. Conclusions on Price Comparisons*

The foregoing description also throws some light on the important relationship of state retail to collective farm market prices. Beliaevskii states that one of the reasons why first-grade-product prices are collected is the desire to preserve reliability and comparability of quotations over time [1, p. 18]. In stating emphatically that the comparisons of state retail to collective farm market prices are limited to 101 largest cities (all of which form a part of the larger sample of 251), he also implies that comparisons of this sort are made for first-grade products only. Until and including 1960, such comparisons were published regularly in official statistical handbooks, e.g., [29, p. 718], but others were also made by various Soviet economists who, to my knowledge, are never sufficiently explicit about the nature of the comparison or the coverage of the underlying data. On the other hand, we know that Soviet statisticians, when calculating indices of real wages, shy away from the calculation of average retail prices, i.e., averages of state retail, cooperative trade, and collective farm market prices. The explanation given is "the difficulty due to the complexity of determining 'average purchase prices' for any one product" [4, p. 194]. It would follow that it is equally difficult to compare average state retail prices to average collective farm market prices for the

<sup>8</sup> We cannot, however, be sure that data which describe extra-village turnover only are more reliable than those which include the "intra-rural" turnover as well.

USSR as a whole, and that such comparisons are not made as a matter of statistical routine. Hence, it must be assumed that all such comparisons refer to prices of first-grade products only and that they are limited to a sample of 101 or, perhaps at times, 251 cities. It then follows that any effort to draw generalized inferences about the relationship of state retail to collective farm market prices (as is done, e.g., in [2]) must necessarily assume that trends in prices for first-quality products are representative of trends in average prices for the same products and, furthermore, that whatever is true of the limited (or otherwise undefined) sample is also true of the USSR as a whole. The first assumption appears to be quite plausible at first glance but need not always turn out to be so. The second, however, is definitely untrue. As we already know, even the larger sample of 251 cities is not adequately representative of collective farm market trade, defined in any way but that of trade in the same 251 cities. This conclusion was reached by some Soviet economists even before the April, 1957 survey of most urban collective farm markets [20, p. 10-11]. Thus, those who wish to draw such inferences must necessarily proceed at their own peril.

## II

### A. *Methodology*

Efforts to estimate the volume of extra-village market sales are inextricably linked with the problem of determining the composition of market output of the various farm products. Market output, or marketings, is defined officially as including "farm products delivered and sold to the state, to co-operatives, and also sold on the market to non-agricultural population" [29, p. 883]. The last category, therefore, is identical with the definition of extra-village market sales.

The composition of market output has been described recently as follows [12, p. 160]: (i) state procurement; (ii) collective farm market sales to nonagricultural population and institutions (i.e., extra-village turnover); (iii) decentralized procurement, or direct purchases from producers by agencies involved in retail or wholesale trade and catering; (iv) deliveries of institutional farms to their parent organizations; and (v) distribution of farm products by collective farms to hired but basically nonagricultural labor as partial payment in kind.<sup>9</sup>

<sup>9</sup> [12, p. 160] also lists the following components of market output: distribution of farm products in kind to state and institutional farm workers and allocation of farm products by state and collective farms to their own canteens. While such ways of disposing of the product are included in the definition of market output which includes "intra-village" turnover, they cannot be included in the officially published data defined to exclude sales or distributions to agricultural population.

Of all the foregoing, data are available only on the volume of marketings and state procurement. The latter are stated partly in terms of physical and partly in terms of the so-called accounting weight, i.e., grain of standard moisture content, etc.<sup>10</sup> It follows that by subtracting from the total volume of marketings the sum of state procurement and estimated market trade, one obtains a residual which ought to be large enough to cover reasonable estimates of items (iii) through (v) above, as well as the difference between the physical and the accounting weight of state procurement, whenever applicable. My estimates of the volume of extra-village market sales proceed in accordance with this criterion.

The procedures employed in this study can be summarized as follows (a full description is given in the Appendix): the basic source for the estimates [6, p. 15] shows data on the percentage breakdown of value of extra-village market trade in 1950 and 1957 for seven major product groups. This information is said to be based on materials of the Central Statistical Administration. Since the total value of this trade is known for both years, ruble data for each of these product groups can be determined easily.

For 1957, the same source [6, p. 87] also shows the percentage of commission trade in the total volume of extra-village market trade (including commission sales) for six of the seven product groups. Since the volume of commission sales for 1957 is known or can be estimated within a small margin of error [28, p. 653], the volume of total market trade can easily be obtained for the main six product groups. Prices ruling in the extra-village trade in 1957 can be calculated by dividing value by volume data. Substituting the 1957 price and quantity figures into the appropriate Soviet indices of extra-village market trade for each of these six product groups, I obtain data shown in Table 1. These serve henceforth as my estimates of the relevant magnitudes.

### *B. The Estimates*

The estimates are internally consistent with other Soviet evidence, provided by source [6] or by official indices and discussed further in the Appendix. As a further check, price and quantity indices, using formulas applied in such computations in the USSR, have been calculated with the aid of data shown in Table 1. The six major product groups for which I show estimates accounted for 79 per cent of the total value of extra-village trade in 1950; the corresponding figure for 1957, including commission sales, is 78 per cent. On a priori

<sup>10</sup> On this vexing subject, see [9].

TABLE 1—ESTIMATED VOLUME OF SALES AND PRICES, EXTRA-VILLAGE COLLECTIVE FARM MARKET,<sup>a</sup>  
SELECTED PRODUCT GROUPS, USSR, 1940, 1950-62

Product Group	1940	1950	1951	1952	1953	1954	1955	1956	1957	1958	1959	1960	1961	1962
A. Quantities Sold (Million tons or billion eggs)														
1. Grains	0.81	3.29	3.91	4.28	3.85	2.75	2.33	2.85	2.77	2.52	2.40	2.19	1.86	1.67
2. Potatoes	2.19	6.46	6.13	6.20	6.26	6.50	6.53	6.62	6.32	6.46	6.31	5.69	5.72	5.26
3. Vegetables	1.49	1.56	1.61	1.73	1.97	1.95	1.94	1.83	1.83	1.98	1.65	1.58	1.39	1.42
4. Milk <sup>b</sup>	2.09	2.42	2.36	2.40	2.47	2.43	2.86	2.51	2.26	2.07	1.94	1.86	1.76	1.84
5. Meat+Fat	0.69	0.79	0.67	0.69	0.73	0.77	0.78	0.81	0.84	0.82	0.75	0.69	0.83	0.98
6. Eggs	1.20	1.37	1.73	2.33	2.80	3.08	3.14	2.86	3.03	3.06	3.11	3.17	3.14	3.20
B. Prices <sup>c</sup> (new rubles per ton or thousand eggs)														
1. Grains	250	330	300	270	240	280	290	230	230	230	200	210	230	230
2. Potatoes	140	110	120	140	130	160	150	130	110	140	130	140	140	160
3. Vegetables	150	190	250	210	200	200	180	150	180	190	220	180	200	210
4. Milk <sup>b</sup>	250	230	240	250	240	280	270	250	220	230	240	240	270	280
5. Meat+Fat	1,430	1,330	1,450	1,560	1,430	1,500	1,590	1,400	1,420	1,460	1,370	1,470	1,630	1,730
6. Eggs	102	106	101	104	97	103	100	101	100	104	103	105	107	113

<sup>a</sup> Including commission trade.

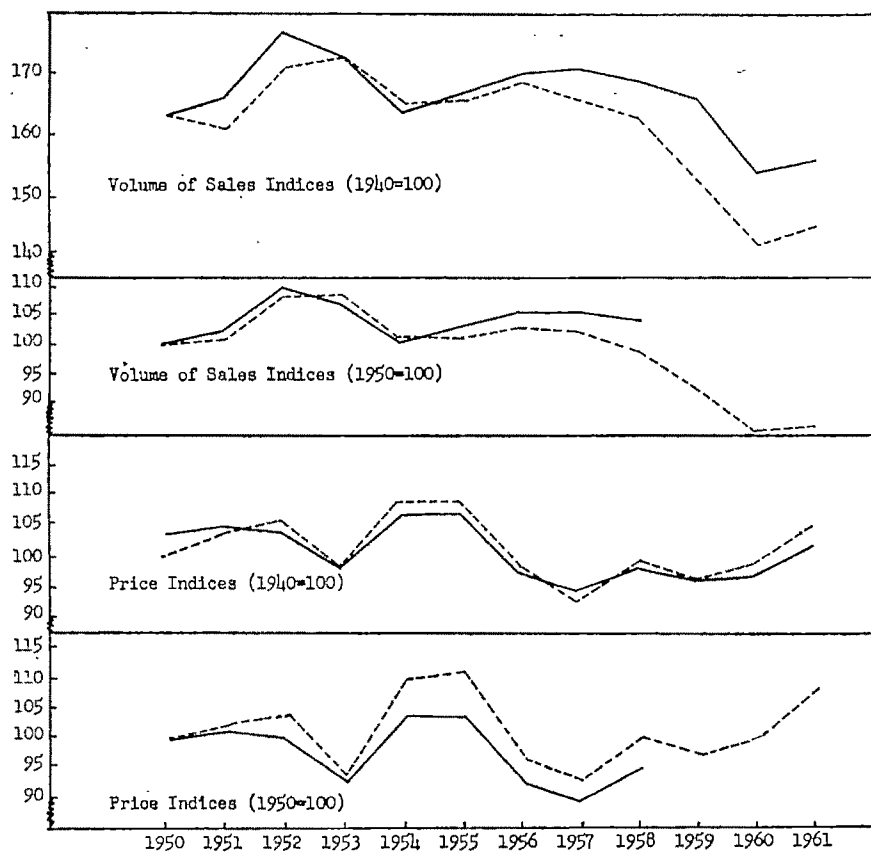
<sup>b</sup> Including milk products in terms of milk equivalent.

<sup>c</sup> Prices have been rounded to the nearest 10 rubles per ton or one ruble per thousand eggs. Unrounded prices in terms of old rubles are shown in the sources.

Sources: Appendix Tables I through VI.

grounds, therefore, one expects a reasonably close correspondence between the indices so computed and the official Soviet indices for the total extra-village market. Comparisons of this sort are presented in Chart 1. As will be seen there, my price indices move reasonably close to the official Soviet indices, so that no additional comment is called for here. This is also true of the quantity index based on 1950 = 100.

CHART 1. OFFICIAL AND CALCULATED INDICES OF EXTRA-VILLAGE  
COLLECTIVE FARM MARKET, USSR



Note: ——— official indices  
----- calculated indices

Source: Appendix Tables XI-XIV.

As for the quantity index based on 1940, the calculated index follows generally the trends of the official index, but beginning with 1956 it does so at a somewhat lower level. The explanation for this behavior must take into account the remoteness of the 1940 base year,

as well as the behavior of the Soviet index for fruit, which is also available in the official handbooks. Fruit forms a significant part of the product groups for which I show no estimates, and which include livestock, mushrooms, honey, sugar, berries, melons and the like. The Soviet fruit index, which generally moves at a lower level than the index for all sales until 1954, begins to show much greater increases in the years 1955, 1958-59. This helps to account for the discrepancy referred to above. In 1960 and 1961, however, the fruit index shows very pronounced declines, which should bring the Soviet index closer to my calculated index. This does not occur and the explanation must be sought partly in the remoteness of the base year and partly in the possible trends for other omitted product groups.

Data presented in Chart 1 offer, I believe, additional confidence in the estimates presented in Table 1. Errors due to conversion and rounding are not believed to exceed 5 per cent.

### III

Our remaining task is that of evaluating the estimates in the light of information provided by other Soviet sources which might be used in the making of similar estimates as well as to deal with some objections that might be raised by one or more *advocati diaboli*.

#### A. Evaluation of Volume Estimates

As far as the order of magnitude is concerned, my volume estimates for 1950, 1954, and 1958 are consistent with partial information provided by two independent Soviet sources [3, p. 50] [5, pp. 236-37]. On the other hand, my estimates for 1956 and 1957 are not in agreement with statements made by two other Soviet sources [14, pp. 120-1] [27, p. 7]. None of the latter, however, defines the term "collective farm market trade" with any degree of precision, nor is the source of their data stated explicitly. By contrast, my estimates refer uniformly to the concept of extra-village market and are based on information which is ultimately homogeneous with respect to its source (Central Statistical Administration). Consequently, I see no reason for concern over this incongruity.

On the other hand, the very order of magnitude of my volume estimates might be considered to be excessively high. Those who might be tempted to reason in this fashion would do so presumably on the basis of information provided for the 251 sample cities in the official handbooks.<sup>11</sup> The unrepresentativeness of this sample, especially with respect to the concept of the extra-village market trade, has already been commented upon. My high estimates essentially mean that the

<sup>11</sup> See Appendix, Table X.

market plays a much less significant role in supplying the population of larger cities with purchased foodstuffs than it does in providing them for inhabitants of smaller towns as well as for members of rural but nonagricultural population. This is in agreement with statements of other Soviet sources as well as observations of Western travelers.<sup>12</sup>

Moreover, my high estimates of the volume of market sales fit quite well with data on the share of the market in total "sales to population" (cf. page 315 above). On the other hand, estimates of much lower order of magnitude would have to face a difficult task of reconciling these two sets of data. First, one would be hard put to explain what the state does with the procured output (even though we all agree that there is considerable waste at this stage). Second, low estimates for extra-village market sales open another Pandora's box with respect to the composition of market output, as they would leave uncomfortably large totals for decentralized procurement and deliveries of institutional farms. On all these grounds I consider my estimates to be of the right order of magnitude.

#### *B. Market Prices and Commission Trade Prices*

The reliability of my price estimates remains to be examined. These might first be compared with prices prevailing in the commission trade (and shown in the Appendix, Table XV). Such a comparison reveals a number of instances where my estimated prices are lower than the commission trade prices, especially (though not exclusively) for grains and potatoes. In view of many Soviet statements to the contrary, this might at first glance cast some doubt about the results of my estimates. For a number of important and relevant reasons, however, comparisons of this sort involve substantial pitfalls of interpretation.

To begin with, many if not all of the Soviet statements just referred to are based explicitly or implicitly on official tabulations of prices ruling in the sample of 101 largest cities. This is even less representative of average market prices than the larger sample of 251 cities.

To the extent that such statements do not refer to such samples (I am not aware of a single statement that could not be construed to do so by implication), they must refer to products of the same quality, sold in the same locality (or on the same market) at the same time.

<sup>12</sup> This is evident from statements that prices are much lower in smaller cities as made, e.g., in [1, p. 59] or [15, p. 13]. According to [22, p. 15], the share of the market in total retail sales in the Ukrainian *oblast'* center Poltava (1959 population: 143 thousand) was: 65 per cent for meat, 92 per cent for vegetable oil, 58 per cent for eggs, 11 per cent for fresh milk, 75 per cent for vegetables, and 79 per cent for potatoes. Data refer to 1958.

Otherwise, they lose much if not all of their meaning. Now, there are many reasons to believe that the total commission trade market and the extra-village market (excluding commission sales) are in fact close to being two separate markets, which overlap primarily in larger cities only. To begin with, the improvement of supply of larger cities (through acquisition in relatively inaccessible rural areas) was the main reason for the introduction of the commission trade in 1953, and it is known that this trade often includes interregional shipments.<sup>13</sup> Secondly, commission trade does in fact appear to be concentrated in the larger, though not necessarily the largest, cities, while collective farm market trade proper takes place primarily in the smaller towns.<sup>14</sup> Thirdly, the commission trade is still relatively insignificant compared to the total extra-village trade (cf. Table 1 and Appendix, Table XV).

To the extent that the two markets are, by and large, separate entities, comparisons of prices for the commission trade taken as a whole and the entire extra-village market depend to a very large extent upon the choice of weights. There is first the variation of prices due to the difference in the location of sales between the small and the large city. Second, there is the issue raised by the fact that Table 1 shows estimates for product groups defined rather broadly (e.g., my grain group is composed not only of the different varieties of grains but also of the grain equivalent of all kinds of flour, baked products, as well as groats and legumes). There is no a priori reason to expect that the product mix *within* each product group is the same for the commission trade as for the total extra-village trade (even though the latter includes the former as far as my estimates are concerned). In fact, for some product groups, the difference in product mix may be very pronounced, as the shares of feed grains and fresh milk might be expected to vary inversely with the size of the city where the transaction takes place. Finally, there is the problem raised by the pronounced seasonal variation in collective farm market sales and prices. Since product mixes for the two markets differ, the appropriate seasonal indices might also show pronounced differences.

For all these reasons, therefore, it is to be expected that what might be true of price relationships holding for identical products, sold in the same locality and at the same time, need not at all be true of the

<sup>13</sup> In 1954, potatoes acquired by cooperatives for commission sales in the central black-soil region were shipped for sale to Rostov, Krasnodar, and even to the Turkmen Republic in Central Asia [32].

<sup>14</sup> According to [32] the share of commission trade in total supply of the population of the Estonian capital of Tallin was: 23.4 per cent for potatoes, 46 per cent for vegetables, 8.5 per cent for meat, and 11.5 per cent for fresh milk.

same relationships as far as average prices for the commission trade and the extra-village market are concerned. That this is so might best be illustrated by the following arithmetical example, bearing on only one of the problems discussed above. Assuming that the relevant magnitudes are as shown below (we are comparing, say, milk data for a hypothetical republic), the average collective farm market price of 244.40 rubles per ton is less than the commission trade price of 253.70 rubles per ton, even though individual market prices are uniformly higher than the commission trade prices:

	Extra-village market		Commission trade	
	rubles/ton	% of sales	rubles/ton	% of sales
Large cities	266	40	256	95
Small towns	230	60	210	5

Lest I be misunderstood, I wish to make it clear that a *broad* relationship between average commission trade and average collective farm market prices is to be expected, even though the comparisons are subject to all the pitfalls enumerated earlier. For example, I would be disturbed if the commission trade prices exceeded my corresponding estimates by a factor of 2 or so. Since this is not the case, we need say no more on this account.

### C. Market Prices and State Retail Prices

There remains the problem of the relationship of my estimates to state retail prices. We are told time and time again that collective farm market prices are significantly higher than state retail prices. We have already discussed the fact that general Soviet statements of this sort are based upon limited and unrepresentative samples. But similar facts have also been observed by Western observers (including the writer), and it is obvious that some broad correspondence between my price estimates and state retail prices should also exist, even though much of what has just been said with respect to comparisons involving commission trade prices also applies here.

No Western calculations of average state retail prices for the six product groups for which I present estimates exist at the time of writing. Some data on state retail prices for roughly comparable products are presented in Table 2 for the median retail price zone II as well as for the lowest retail price zone I. As it turns out, my price estimates for extra-village market are generally higher than state retail prices (with a single exception of milk for 1958, where the departure from this rule is not too significant).

This, however, is true of averages. In larger cities, market prices are considerably higher than state retail prices; my estimates are

therefore consistent with the proposition that in many smaller towns market prices are at times below state retail prices. Is this state of affairs theoretically possible, especially in the light of the weighty argument advanced by Professor Nicholas Spulber who concludes: ". . . two prices cannot, however, prevail for the same commodity when the state price is higher [than the market price]. In this case these [state] shops simply price their commodity out of the market" [24, pp. 98-99]?

Strange as it may seem, Spulber's analysis is tacitly based upon assumptions usually associated with the working of perfect com-

TABLE 2—STATE RETAIL PRICES OF SELECTED FOODS, SELECTED AREAS  
OF THE USSR, SELECTED YEARS  
(new rubles per ton)

Product	Area	1953	1958	1959	1960-61
<b>A. Grains:</b>					
1. Wheat	Zone II	n.a.	215	215	215
2. Rye	Zone II	n.a.	200	200	200
3. Barley	Zone II	n.a.	124	124	124
4. Oats	Zone II	n.a.	104	104	104
5. Corn	Zone II	n.a.	114	114	114
<b>B. Potatoes:</b>					
6.	Moscow (zone II)	75 <sup>a</sup>	n.a.	90 <sup>b</sup> -100 <sup>c</sup>	100 <sup>d</sup>
7.	Leningrad (zone II)	n.a.	100 <sup>c</sup>	100 <sup>b</sup>	n.a.
<b>C. Milk:<sup>e</sup></b>					
8.	Moscow	213	n.a.	213	281 <sup>d</sup>
9.	Zone II	n.a.	242 <sup>f</sup>	n.a.	n.a.
10.	Kazan' (zone I)	n.a.	n.a.	n.a.	174-233 <sup>f</sup>
<b>D. Meat:</b>					
11. Beef	Moscow	1,260 <sup>a</sup>	n.a.	1,200 <sup>c</sup>	n.a.
12. Beef	Zone II	n.a.	n.a.	n.a.	1,110 <sup>c</sup>
13. Pork	Zone II	n.a.	n.a.	n.a.	1,560 <sup>c</sup>
<b>E. Eggs: (rubles per thousand)</b>					
14.	Moscow	69	n.a.	80 <sup>c</sup>	80 <sup>c</sup>
15.	Zone II	n.a.	80 <sup>f</sup>	n.a.	n.a.
16.	Kazan' (zone I)	n.a.	n.a.	n.a.	50-70 <sup>f</sup>

Notes: n.a. = not available.

<sup>a</sup> April 1, 1953.

<sup>b</sup> After September 1.

<sup>c</sup> August 15, 1959.

<sup>d</sup> June 15, 1962.

<sup>e</sup> Converted to price per ton from price per liter with a conversion coefficient of 0.969.

<sup>f</sup> Seasonal average or seasonal range.

<sup>g</sup> Grade average to cover entire carcass.

Sources: See Appendix, p. 37.

petition (complete mobility, perfect knowledge, lack of marketing costs). But he also assumes that state shops *always* have something to sell; otherwise their prices lose much of their leverage effect, although they can still exert some influence on the level of market prices (for example by determining the initial level of asked prices). Quite often, however, state retail shops run out of some foods even in the larger cities,<sup>15</sup> and I already stated my belief that the market provides the bulk of purchased supplies in smaller towns.

The seasonality of agricultural production, combined with the lack of adequate storage facilities in the hands of small producers, results in some very pronounced seasonal variation in supply on the extra-village market. That this is also reflected in pronounced price variations suggests that market prices tend to be affected much more by fluctuations in supply than by those in demand. In terms of the famous analogy, it is the supply blade of the scissors that tends to move more often than the demand blade (cf. also [18, pp. 30-31]). Additional factors, reinforcing this tendency, are to be found in the high transport costs associated with the collective farm market. Once these had been incurred, the seller would ordinarily attempt to spread them over a large volume of sales without paying too much attention to the price towards the end of the day. Taken by themselves, supply factors might be expected to push market prices below the level of (largely fictitious or paper) state retail prices in the smaller town during the periods of peak seasonal trade when much of the turnover materializes.

It is to be expected, of course, that a floor exists below which market prices would not normally decline regardless of the seasonal trends in supply, but it is not easy to determine the exact level at which it would be established. Theoretical considerations point to the level of prices realized by the farmers in state procurement, increased by the amount of marketing costs attributable to a unit of product. In the USSR, or elsewhere for that matter, a peasant farmer cannot always be expected to engage in such a refined calculation, but this need not deprive him of the title of rational man. For clearly some utility may be derived from a trip to town, however small.

Something remains to be said for the demand side. If there are no opportunities to buy at state retail stores, the volume of demand directed at the collective farm market might be expected to be larger, thus nullifying to some extent the influences working on the supply

<sup>15</sup> On March 11, 1962 I observed a collective farm market price of 0.50 rubles/liter in the city of Iaroslavl' (1959 population: 407 thousand). A Soviet citizen queried on the level of state retail price for milk replied: "0.29 rubles/liter, but there isn't any." As it turned out, there was but little milk in one state store that morning.

side. But people who live in smaller Soviet towns rely to a considerable extent on their own household plots for food supplies. While more recent data are lacking, the private nonagricultural household plot accounted for the following percentages of total per capita consumption in the nonagricultural sector in 1956 [11, p. 33]:

Meat	14.1 per cent
Milk	20.3 per cent
Eggs	37.7 per cent
Potatoes	31.1 per cent
Vegetables	18.1 per cent

These are averages for the USSR as a whole; in smaller towns the percentages are likely to be much greater. Under the circumstances, I do not believe that demand shifts can, on the average, outweigh factors acting on the supply side; it is the latter that must exert the dominant influence on prices in smaller cities and hence on much of the total market turnover. Professor Spulber's conclusion appears to be disposed of quite effectively even without recourse to an argument based on market imperfections. Surely, he would be the last to claim that these do not exist in the USSR.

#### IV

In conclusion, it seems advisable to emphasize the major points of the discussion. First, unless a source defines the meaning of the term "collective farm market trade" very precisely, the information so provided should be treated with great circumspection. It is hoped, of course, that the estimates presented here will assist other analysts in the future interpretation of such data. Second, the importance of the market in the total food supply of the nonagricultural population is still quite substantial, especially when reference is to the smaller towns or urban centers. Third, chiefly as a result of weighting problems, comparisons between average market prices for commodities defined rather broadly and similar prices effective in other retail markets in the USSR are not very meaningful at best and may, at times, be quite misleading. Fourth, the level of market prices (especially in the smaller cities) is more often responsive to fluctuations in supply than demand. Fifth, the fact that I have apparently been able to present estimates which are very close to the figures employed for the same purpose by Soviet statistical agencies should not obscure the fact that the very nature of these data leaves much to be desired. Sixth, it should be remembered that my estimates apply to a particular concept of "extra-village collective farm market" (in-

cluding commission trade).<sup>18</sup> Although they might help to reduce the range of speculation about magnitudes appropriate to other definitions of this elusive and so often misinterpreted term, they should not be used indiscriminately.

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## AN INTERNATIONAL COMPARISON OF FACTOR COSTS AND FACTOR USE

### *A Review Article*

By WASSILY LEONTIEF\*

For over 30 years—to be exact, since 1928—whenever a working economist was called on to describe in numbers or to interpret in analytical terms the relationship between the inputs of capital and labor and the final product of a plant, an industry, or a national economy as a whole, he was more likely than not to reach out for the Cobb-Douglas production function. Theorists questioned the arbitrariness of its form and statisticians the validity of procedures used in fitting it to given sets of data, but despite all criticism the familiar exponential equation was used over and over again, essentially, I think, because of its convenient simplicity. But now this remarkable career is apparently coming to an end. The old formula is being rapidly replaced by a new, improved recipe: the constant elasticity of substitution production function. In quantitative empirical analysis, the C.E.S. function can perform essentially the same role that the Cobb-Douglas function played up until now, but, owing to its less restrictive shape, it offers at the same time the indisputable advantage of greater flexibility.

In this monograph,<sup>1</sup> the new tool is used with considerable skill in a statistical inquiry designed to test—and, as it turns out, to disprove—one of the factual assumptions of the much-debated Heckscher-Ohlin interpretation of the classical theory of international trade. Mr. Minhas is one of the four joint authors—Professors Arrow, Chenery, and Solow are the others—of the article<sup>2</sup> published three years ago in which the C.E.S. function was not only described in some detail, but also, so far as I know, for the first time fitted to actual statistical data. Thus, it is not surprising to encounter in his book formulations and arguments already developed, or at least suggested, in that article.

The principal ideas are developed in four chapters which make up the first half of the book; the three remaining chapters are devoted to systematic statistical description and international comparison of the rates of return on capital in different industries. Presenting the results of what apparently first was conceived as a separate inquiry, the second half of

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<sup>1</sup> Bagicha Singh Minhas, *An International Comparison of Factor Costs and Factor Use*. Contributions to Economic Analysis, No. 31. Amsterdam: North-Holland Publishing Co., 1963. Pp. xii, 124. Paper, \$4.20.

<sup>2</sup> "Capital-Labor Substitution and Economic Efficiency," *Rev. Econ. Stat.*, Aug. 1961, 43, 225-50.

the book bears only a loose, sketchily delineated relationship to the central line of argument developed in the first four chapters.

In a laudable endeavor to bring together theoretical and factual analysis, Minhas continuously shifts his argument from one to the other. For purposes of a critical review it seems to be more appropriate, after restating the substantive issue to which he addresses himself, to examine separately the new tool he chooses to use, the specific method of its application, and the interpretation of the results obtained.

The factual assumption of the modern theory of international trade that Minhas sets out to disprove is that a meaningful distinction can be made between capital- and labor-intensive industries, a distinction that incidentally plays a crucial role in analysis of economic development.

If the amounts of capital and of labor employed per unit of their respective outputs were technologically fixed, the ranking of different industries in accordance with the relative magnitude of the two input coefficients would certainly be valid. It still would be meaningful even if, in response to a given change in relative prices of the two factors, capital were substituted for labor or vice versa, provided the downward or the upward shifts of the capital-labor input ratios were so uniform as not to disturb to any significant extent the relative position of the individual industries on the capital-labor intensity scale. If, on the contrary, some industries responded to a given change in the relative price of the two factors by a much larger shift in their relative inputs than others, then their comparative position on the capital-labor intensity scale would often be reversed. The distinction between capital- and labor-intensive industries must lose in such a case much of its analytical usefulness. Neither in explanation of the pattern of international trade nor in the study of economic growth would it be permissible to utilize it as a technological datum. Minhas sets out to demonstrate empirically that this is actually the case, and he employs the constant elasticity of substitution production function to do so.

The constant elasticity of substitution—or as Minhas prefers to call it, the homohypallagic—production function can be written in the following form:

$$(1) \quad V = (AK^{-\beta} + \alpha L^{-\beta})^{-1/\beta}$$

where  $V$  represents the output;  $K$  and  $L$  stand respectively for the inputs of capital and labor. Each one of the three quantities should be thought of as being measured in different physical units or, in the case of aggregative analysis, described by an appropriate index number.  $A$ ,  $\alpha$ , and  $\beta$  are constants which are supposed to reflect the technical characteristics of the particular production process. If  $K$  and  $L$  on the right-hand side of the formula are multiplied by an arbitrary positive constant,  $\lambda$ , the corresponding total output on the left-hand side will become  $V\lambda$ : this means that the production function described by equation (1) is homogeneous of the first degree; it obeys the law of constant returns to scale.

The partial derivatives of  $V$  in respect to  $L$  and  $K$ , i.e., the marginal productivities of labor and of capital, are:

$$(2a) \quad \frac{\partial V}{\partial L} = \alpha \left( \frac{V}{L} \right)^{\beta+1} \quad (2b) \quad \frac{\partial V}{\partial K} = A \left( \frac{V}{K} \right)^{\beta+1}$$

and the marginal rate of substitution of capital for labor—let it be called  $x$ —is:

$$(3) \quad x = \frac{\partial V}{\partial L} / \frac{\partial V}{\partial K} = \frac{\alpha}{A} \left( \frac{K}{L} \right)^{\beta+1}.$$

Translated into logarithmic terms, that equation describes a straight line:

$$(4) \quad \log x = \log \frac{\alpha}{A} + (\beta + 1) \log \left( \frac{K}{L} \right).$$

Its constant slope  $(\beta+1)$  is the reciprocal of the elasticity of substitution between capital and labor,  $\sigma$ :

$$(5) \quad \sigma = d \log \left( \frac{K}{L} \right) / d \log x = \frac{1}{\beta + 1}.$$

To demonstrate that the Cobb-Douglas production function represents a special case of the C.E.S. function in which  $\sigma=1$ , i.e.,  $\beta=0$ , we can rewrite (3) interpreting its left-hand side as a derivative of  $K$  in respect to  $L$  along a constant output curve:

$$(6) \quad \frac{dK}{dL} = - \frac{\alpha}{A} \frac{K}{L} \quad \text{or} \quad - A \frac{dK}{K} = \alpha \frac{dL}{L}.$$

Integration of the two sides of the second expression gives:

$$(7) \quad V = K^A L^\alpha$$

where the constant of integration,  $V$ , represents the output measured in appropriately defined units. After raising both sides of (1) to the power  $\beta$ , we can see that  $A+\alpha=1$ , if  $\beta=0$ , which is indeed the condition satisfied by the two exponents in the homogeneous Cobb-Douglas production function.

Perfect substitutability between capital and labor can also be interpreted as being a special case of the C.E.S. function (1): If  $\sigma = \infty$  and consequently  $\beta = -1$ , it acquires the simple linear form,

$$(8) \quad V = A\bar{K} + \alpha L.$$

On a familiar two-dimensional graph the corresponding isoquants are represented by a set of negatively sloping parallel straight lines.

At the opposite extreme, when the elasticity of substitution tends toward 0 and  $\beta$  tends toward  $\infty$ , equation (1) degenerates into an input-output relationship characterized by constant capital and labor coefficients of production. However, for reasons that I will explain later, this rather special inter-

pretation of strictly complementary relation between capital and labor, though formally correct, is apt to be misleading when applied in statistical analysis of observed facts.

If a profit-maximizing industry considers the price of labor,  $w$ , and the price of capital,  $r$ , as given, it will employ these two factors of production in such amounts as to equate the price ratio,  $\frac{w}{r}$ , to the marginal rate of substitution of capital for labor. According to (3) and (4), in the case of a C.E.S. function, the dependence of the factor input ratio,  $\frac{K}{L}$ , on the price ratio,  $\frac{w}{r}$ , is described by the simple log-linear relation,

$$(9) \quad \log\left(\frac{w}{r}\right) = \log \frac{\alpha}{A} + (\beta + 1) \log\left(\frac{K}{L}\right).$$

Minhas illustrates his crucial argument concerning the possible effect of changing price ratios on the capital-labor input ratio in different industries by drawing the graph in Figure 1.

The two lines represent the relationship between the capital-labor ratio,  $\frac{K}{L}$ , and the relative price,  $\frac{w}{r}$ , in two different industries. The first industry will be more capital-intensive (and less labor-intensive) than the second if the labor-capital price ratio happens to be higher than  $\frac{\bar{w}}{\bar{r}}$ , but the second

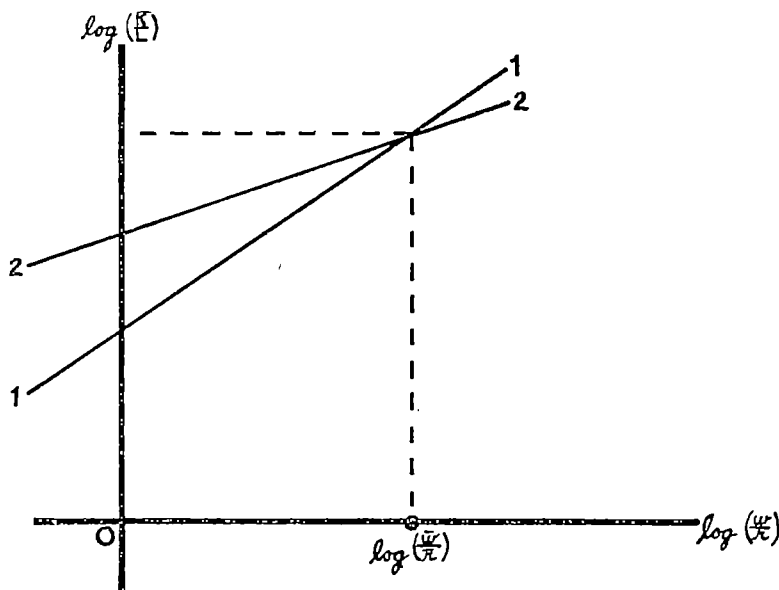


FIGURE 1

will become more capital-intensive (and less labor-intensive) than the first if the labor-capital price ratio is lower than the critical level,  $\frac{\bar{w}}{\bar{r}}$ . The point  $A$  at which the two industries, while confronted with the same relative prices of capital and labor, would combine the two factors in exactly the same proportion is called by Minhas the crossover point. The position of the crossover depends on the slope  $(\beta+1)$  and the level  $\left(\frac{\alpha}{A}\right)$  of each of the two curves. If the slopes of the two lines—that is, the elasticity of substitution between capital and labor in the two industries—happen to be exactly the same, the capital intensities of both industries will be identical throughout if the levels of both the lines happen to be also equal; otherwise, they will be parallel, which means that the capital-labor ratio in one of the two industries will be higher throughout than in the other. In case the elasticities are unequal, that is, the slopes of the two lines differ, they must necessarily intersect somewhere. The crossover points might, however, be located to the right or to the left of the usual or even possible range of observed capital-labor or price ratios. In this case, one industry can still be, for all practical purposes, unequivocally characterized as using more capital per unit of labor than the other.

Minhas sets out to demonstrate that, in fact, crossovers can be expected to occur within the practically relevant range so often as to vitiate the analytical usefulness of conventional distinctions between capital- and labor-intensive industries. I cannot agree with this and will now try to show that Minhas' own empirical evidence justifies the opposite conclusion.

To demonstrate the importance of the crossovers, Minhas fits, by a very ingenious procedure, C.E.S. production functions to 24 industries distributed over 19 different countries. The approach is cross-sectional; the primary data (presented in his Appendix I) consist of "value added produced per man year of labor input" and "annual wage rate payment per worker" compiled from official statistical publications for each industry in each country. Ingenuity is called for because no information on corresponding capital inputs or prices is used in the process. Values-added, i.e., the gross revenues minus the costs of materials, are used throughout to represent the outputs,  $V$ . All wage rates and values-added are converted to U.S. dollars in accordance with the fixed official or free market exchange rate.

On the assumption that the real wage rate paid equals the marginal productivity of labor,  $w$  can be substituted for  $\frac{\partial V}{\partial L}$  on the left-hand side of (2a). The resulting equation can be written in the form of the following log linear relationship:

$$(10) \quad \log \left( \frac{V}{L} \right)_i = \log a_i + b_i \log w_i$$

$$\text{where } \log a_i = -\log \alpha_i \frac{1}{\beta+1} \quad \text{and} \quad b_i = \frac{1}{\beta+1} \equiv \sigma_i.$$

Subscript  $i$  identifies the industry and subscript  $j$  the country. The constants,  $\log a_i$  and  $b_i$ , carry only the industry but not the country subscript since the C.E.S. production function, the shape of which they are supposed to reflect, is assumed to be the same in all the countries.

The magnitude of parameters,  $\log a_i$  and  $b_i$ , can thus be estimated by fitting a least-squares regression line through the scatter of  $\log \left( \frac{V}{L} \right)_j$  on  $\log w_{ij}$ , with a fixed  $i$  and varying  $j$ . The slope  $b_i$  of that regression line represents the elasticity of substitution between labor and capital in the  $i$ th industry. Among the 24 regression lines fitted by Minhas, the estimated magnitude of  $\sigma_i$  ranges from 0.7211 in Dairy Products to 1.0114 in Primary Nonferrous Metals; in 20 instances it exceeds 0.8, and in eight of these it lies above 0.9.

To determine the location of potential "crossover" points marking the reversal in the relative capital-labor intensity of any two of the 24 industries, it is necessary to draw up for each one of them a log linear relationship between  $\frac{K}{L}$  and  $\frac{w}{r}$  as described in equation (9) and shown in Figure 1.

The elasticity of substitution between capital and labor determines, however, only the slope of the straight lines drawn on that graph. Their levels depend on the value of the two other constants,  $\alpha_i$  and  $A_i$ , entering in the C.E.S. function of each industry,  $i$ . Equation (10) in addition to  $\beta_i$  (and, consequently,  $\sigma_{ij}$ ) yields an estimate of  $\alpha_i$ —the constant associated in the C.E.S. production function (1) with the labor input,  $L$ —but it is incapable of supplying also an estimate of  $A_i$ , which is the corresponding constant associated with the capital input,  $K$ .

To apply an analogous procedure in estimating the  $A$ 's, it would be necessary only to replace the ratio  $\frac{V}{L}$  on the left-hand side of (10) by the corresponding ratio  $\frac{V}{K}$ , and on its right-hand side replace the wage rate,  $w_{ij}$ , by  $r_{ij}$ , i.e., the rate of profit earned per unit of capital employed by industry  $i$  in country  $j$ .

Minhas presents the estimates of both the  $\alpha$ 's and the  $A$ 's for only six of the many industries covered by his elasticity computations. The magnitudes of the  $\alpha$ 's and  $\sigma$ 's entered in small Table IV correspond exactly to the least-squares estimates of these parameters—based on equation (10)—shown for all the 24 industries in his Tables I and II. No word is said, however, in explanation of the origin of the estimates of the six corresponding  $A$ 's. This is the more surprising since the examination of the five "crossovers" between the capital-labor intensities of these particular six industries (shown on his Figures 5 and 6) constitutes the sole and only factual evidence that Minhas can cite in support of his sweeping and emphatic rejection of the conventional distinction between capital- and labor-intensive industries.

As I have said above, in Chapters 5 and 6, Minhas presents a rather detailed statistical analysis of the rates of return on capital invested in the

same industries in different countries. Table XVII on page 92 summarizes the results of his inquiry; it covers 17 industries and five countries: United States, Canada, United Kingdom, Japan, and India. Most of the industries included in the larger set of data which Minhas actually uses to estimate two of the three parameters of the 24 C.E.S. production functions are represented directly or in slightly aggregated form also in Table XVII. The information contained in it can thus be used to estimate the missing third parameter for 17 of the 24 industries covered in his Table I. The two-step procedure I have used is described below.

Parameter  $A_i$  enters as denominator in the middle term of equation (9); for the purpose at hand it suffices to estimate for each industry the magnitude of that entire term, rather than of  $A_i$  alone. Equation (9) can be re-written in the following form:

$$(9a) \quad \log \left( \frac{\alpha_i}{A_i} \right) = \log \left( \frac{w_{ij}}{r_{ij}} \right) - (\beta_i + 1) \log \left( \frac{K_{ij}}{L_{ij}} \right).$$

In estimating the elasticities of substitution, Minhas has already obtained the magnitudes of the corresponding  $\beta_i$ 's. He compiled and used in his computations the wage rates,  $w_{ij}$ ; he also compiled—but apparently did not use for the same purpose—the  $r_{ij}$ 's for 17 industries in five countries.

The magnitude of the capital-labor ratio,  $\left( \frac{K}{L} \right)_{ij}$ , appearing in the second right-hand term of (9a), can be derived by combining the profit-rates data with information on wage rates and value-added per worker,  $\left( \frac{V}{L} \right)_{ij}$ , which, as we have seen above, Minhas uses too. By his own assumption, the value-added in any industry is exactly exhausted by payments to capital and labor employed by it:  $V_{ij} = L_{ij}w_{ij} + K_{ij}r_{ij}$ . Dividing both sides by  $L_{ij}r_{ij}$  and rearranging the terms, we arrive at the following relationship:

$$(11) \quad \left( \frac{K}{L} \right)_{ij} = \left( \frac{V}{L} \right)_{ij} \frac{1}{r_{ij}} - \frac{w_{ij}}{r_{ij}}.$$

With all the magnitudes appearing to the right of the equation sign given, we can compute  $\left( \frac{K}{L} \right)_{ij}$ . Inserted on the right-hand side of (9a), this completes the information required to determine the magnitude of the constant,  $\log \left( \frac{\alpha}{A} \right)_i$ .

I have performed these additional computations for 21 of the 24 industries covered in Minhas' study; all those industries for which his Table XVII supplies an estimate of the rate of return on capital,  $r_{ij}$ . The results are shown on Figure 2. The factor price ratios,  $\frac{w_{ij}}{r_{ij}}$ , inserted in the course of these computations in equation (11) are those recorded for the industry in question in the United States. This means that the magnitude of the con-

stant term,  $\log \left( \frac{\alpha}{A} \right)_i$ , was determined so as to make each one of the straight lines shown on Figure 2 pass exactly through the point describing the combination of factor prices and factor inputs actually recorded for that particular industry in the United States. As should have been ex-

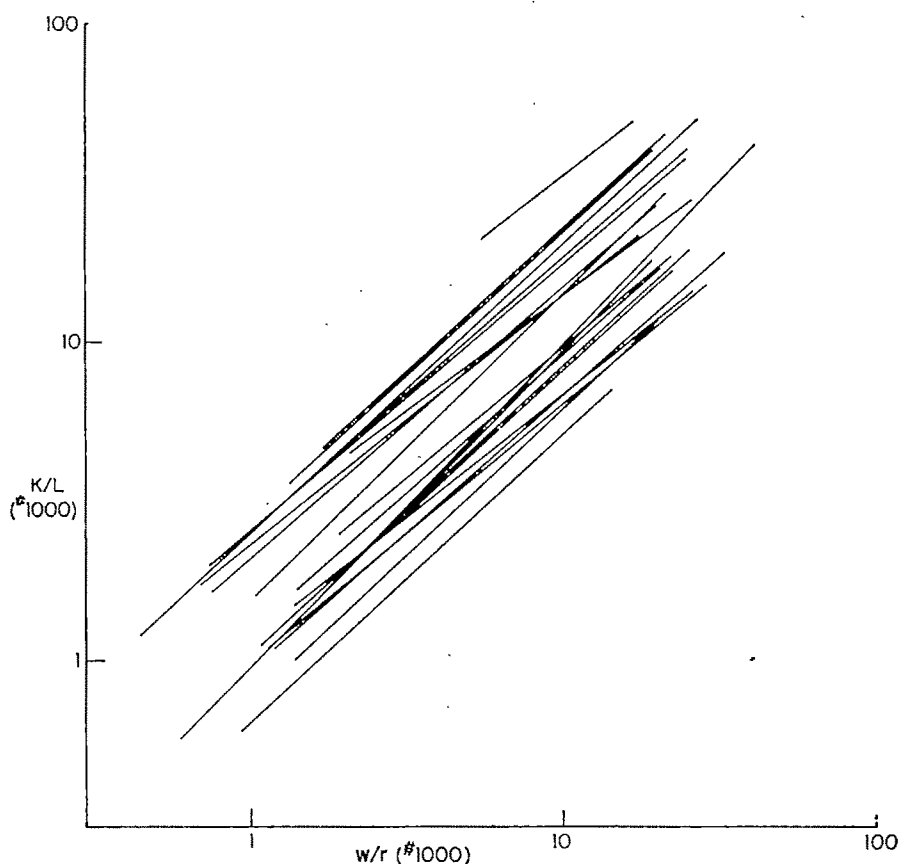


FIGURE 2

pected, all these points are located at the upper right-hand ends of all the 21 corresponding lines. The lowest of the  $\left( \frac{K}{L} \right)_{ij}$  ratios observed in any industry  $i$ —typically observed in India—determine the cutoff at the lower left-hand end of each line. The corresponding lowest factor price ratios,  $\left( \frac{w}{r} \right)_i$ , would have been equal to the wage-profit ratio actually observed in India if the theoretical assumption on which these computations are based were faultless and the empirical information error-free. In

fact, the actual ratios deviate, of course, from those predicted on the assumption that the U.S. price and input ratios lie exactly on the curve.

The picture emerging from supplemental computations as shown on Figure 2 does not confirm Minhas' emphatically stated conclusion that "the strong factor intensity assumption, the conventional distinction between capital and labor intensive industries is of limited practical validity." On the contrary, it seems to confirm the conventional view. Of the theoretically possible 210 crossover points between the 21 lines entered on the graph, only 17 are found to be located within the wide range of factor price ratios, spanned on the one end by those observed in the United States and on the other by those reported from India. Moreover, most of these crossovers occur between industries whose curves run so close together throughout the entire range that for all practical purposes their capital-labor intensities would be considered identical. With two or three exceptions, each one of the 21 industries represented can be characterized as capital-intensive, labor-intensive, or as belonging to an intermediate group. In the light of this evidence the modern theory of international trade stands vindicated.

To avoid undesirable confusion of related but separable issues, up to this point I have presented Minhas' arguments and examined his conclusions without questioning the general theoretical framework within which they have been set. Interested in demonstrating the practical importance of crossovers, he naturally rejected the Cobb-Douglas function—which excludes crossovers by definition—and reached out for a formula capable of showing their existence. But if this were the principal reason for acquiring one more degree of freedom, the result of my extended computations shown in Figure 2 could easily justify a return to the simpler Cobb-Douglas formula. This suggestion would appear to be even more plausible if in applying the least-square method to estimate the slopes,  $b_i$ , in the log linear equations (10), Minhas had not proceeded on the assumption that only the variable  $\left(\frac{V}{L}\right)_i$  is subject to random errors, while the variable

$w_i$  is not. Had he instead, in fitting the slopes of these regression lines, allowed also for errors affecting the observed magnitudes of  $w_i$ , all estimated elasticities would necessarily turn out to be larger, since in 23 out of the 24 industries examined by him, the magnitudes of the  $b_i$ 's, i.e., the elasticities of substitution, turn out to be less—although in most instances only slightly less—than 1. This means that their values would be still closer to 1—the constant elasticity of the Cobb-Douglas function.

The inverse proportionality (implied by  $b_i=1$ ) between the number of workers employed per unit of output of a particular industry and the wage rate paid to them by that industry in different countries can be explained in entirely different terms. The assumption that a man-year of labor in one part of the world is equivalent to a man-year of labor in any other part, i.e., that the typical worker employed, say, in India is equal in productive efficiency to his similarly employed counterpart in the United States, can be questioned. If such equivalence were the rule rather than an exception,

why should economists studying problems of economic development be so much concerned with investment—or rather the lack of it—in “human capital”?

Let it be assumed, for argument's sake, that an average man-year of labor employed by a given industry in one country is twice as efficient as a man-year employed by the same industry in another country. The production function in both instances can still appear to be, and actually will be, essentially the same, provided that, in measuring labor inputs for purposes of comparison, we multiply the figure describing the amount of labor absorbed by that industry in the first country by 2. At the same time, for comparison of the real unit costs of labor to that industry, the actual annual wage rate paid by it in the first country would have to be divided by 2. Such a procedure would be analogous to that used by Ricardo in his *Theory of Rent*. He visualized an agricultural production function allowing for several different grades of land and explained the higher price paid for an acre of better land by its proportionally greater efficiency. A similar argument was used recently by Houthakker when he interpreted the difference in the unit price paid by consumers for grades of nominally the same article as a measure of intrinsic qualitative difference.

The elasticity which Minhas estimates by fitting equation (10) to cross-section data measures—if it is interpreted in this sense—not substitution between capital and labor but rather substitution between different grades of labor, or possibly some combination of both. In the first case, the magnitude of the elasticity constant,  $b_i$ , in equation (10), as estimated by him, would necessarily be close to 1.

To determine which interpretation of Minhas' findings is correct, it is necessary to bring information on capital inputs explicitly into the picture. An elaborate comparison of rates of return on capital in different industries and countries can be found in the last two chapters. As I said above, the author nevertheless relies exclusively on the elegant but, even for his own purposes, not sufficiently powerful procedure in the course of which the elasticity of substitution between capital and labor is derived on the basis of information pertaining to labor only.

I have performed the simple numerical manipulations (similar to those described above in connection with the construction of my Figure 2) required to determine the capital-output ratios,  $\left(\frac{K}{V}\right)_i$ , that would match the labor-output ratios,  $\left(\frac{L}{V}\right)_i$ , used in Minhas' own computations. An examination of the resulting scatters shows that, as compared to the corresponding labor intensity, the capital intensity of any given industry varies little from country to country, and only in a few instances could one discern a visible negative relationship between the two. The over-all picture is thus quite different from that which emerges from what Minhas calls the straightforward, but which in fact is a rather one-sided, method of estimating the elasticity of substitution between capital and labor.

In the light of closer examination of empirical evidence, fixed capital

and labor coefficients (the latter measured in comparable efficiency units) might after all prove to be more appropriate for description of the specific productive relationships than the C.E.S. function in its general, or its particular Cobb-Douglas, form.

Judging by the practical implication that Minhas draws from it, the formally correct interpretations of fixed capital and labor coefficients as a special case of the C.E.S. function tend to be misleading. Fixed coefficients of production can be interpreted more meaningfully as representing a special case of technological conditions under which the two factors can be substituted for each other, but only within relatively narrow limits: the rate of substitution of capital for labor decreasing sharply and approaching zero whenever the capital-labor input ratio approaches a finite upper limit, but falling and becoming infinitely large when that ratio approaches the—also finite—lower limit. The elasticity-of-substitution concept proves to be a very awkward tool for analyzing this type of situation, and the assumption of constant elasticity of substitution simply breaks down in such a case. If the upper and lower limits of the admissible capital-labor input ratios lie comparatively close together, the average fixed coefficients will give an adequate description of such a technology. A combination of two or more alternative sets of such coefficients would of course do still better.

Except in its degenerate form when  $\sigma=0$ , the C.E.S. function itself represents, as a matter of fact, a special case of homogeneous production characterized by literally unlimited substitution possibilities between factors, thus implying—when these factors are capital and labor—that any amount of a finished product can be obtained with a practically negligible amount of either capital or labor provided the supply of the other factors is large enough. This might be a good enough assumption in aggregative analysis where all possible products and processes of production are subsumed under a single loosely defined production function describing not so much a substitution of one method of production for another as a changing product mix. It is, however, hardly adequate for description of alternative input structures of sharply defined individual industries.

The length of this review testifies to the amount of stimulation an interested reader can find in this slim volume. The questions which the author asks are so well put that they will advance the understanding of factor use by the various branches of production in an international setting even if some of the answers which he gives cannot be accepted.

## COMMUNICATIONS

### The Case for Capital Controls to Relieve the U.S. Balance of Payments

It is doubtful that many economists would disagree that one of the two or three current major economic problems of the United States is the reduction and ultimate elimination of the balance-of-payments deficit, and the accomplishment of this without restraining growth of the domestic economy. Presumably no one would contest the grave seriousness of a failure to show a marked improvement in the balance of payments over the course of the next three or four years or the necessity of reducing the deficit without serious adverse side effects.

It is evident, however, that there is no consensus among economists of what, if anything, should be done to meet the problem. There are two kinds of divergences of opinion: first, between those who believe nothing in the way of positive new policy measures should be undertaken and those who advocate various specific policy remedies; second, among the latter, differences as to what specific measures would be optimal. The primary purpose of this paper is to examine the second issue—what specific measures would be optimal, with the case for measures to reduce the outflow of private capital given special attention.

Interest in measures to reduce the outflow of private capital has been stimulated by the Administration's proposal, now before the Congress, to levy a tax on the purchase of securities from foreigners [H.R. 8000]. I shall not be concerned in this paper with this or any other specific types of measures to reduce outward capital movements, nor with the problems—some of which may be formidable—attending the implementation of such measures. I shall instead concentrate on the theoretical aspects of capital controls, with "controls" understood to embrace any kind of measure, direct or indirect, designed to reduce private capital outflows. Finally, the analysis applies only to long-term capital movements, the special case of short-term movements being excluded from consideration.

Before entering into the discussion of alternative policy measures to correct the U.S. balance-of-payments deficit and of the case for capital controls as the least undesirable alternative, a brief comment on the first issue mentioned above—whether any kind of specific balance-of-payments policy is called for—is in order.

#### *I. Market Correction of the Deficit*

Whether or not it would be wise for specific policy measures to be adopted to relieve the balance of payments depends upon the probability that market forces—operating under present policies—will correct the deficit within a reasonable period of time and without interfering with policies to expand the

domestic economy. Fortunately, the option of relying on market forces is still open, since the reserve position of the United States, together with the various swap agreements and other arrangements among central banks, permit a continuation of the deficit for some time without leading to a crisis situation.<sup>1</sup>

The thesis that market forces can be expected to work in the direction of eliminating the U.S. balance-of-payments deficit, even though (and in part *because*) domestic policies succeed in accelerating the rate of growth in the United States, has been most ably supported by the recent Brookings study under the direction of Walter Salant [6]. However, the study's optimistic projections are based on several assumptions which may or may not be realized. Most critical among these is the assumption that U.S. costs and export prices will fall relatively to those of Western Europe. Because of the large degree of uncertainty underlying this assumption, together with the uncertainty involved in extrapolating into the future functional relationships derived from past behavior, the present writer, as well as numerous others, has argued that the Brookings study's projections are not a sufficiently solid base on which to arrive at policy conclusions [10]. This is, of course, a personal judgment, based on the subjective weighing of how much confidence can be placed in balance-of-payments projections in comparison with the consequences of failure of the projections to be actually realized.

Apart from the question of whether one can confidently expect market forces satisfactorily to solve the payments problem without the aid of additional discretionary policy measures, there is the second question of what particular policy measures would be appropriate and acceptable. If there are acceptable policies that could be adopted to relieve the deficit, the case for relying on market forces to do the job is weakened; if not, the case is strengthened.

## II. *The Criteria of Acceptable Policies*

The crux of the matter, of course, is what constitutes "acceptable" policies. There are three general economic criteria to which, it is believed, most economists would agree. These may be briefly identified as effectiveness, employment, and efficiency. In addition, there is a fourth criterion, essentially non-economic in nature, that I shall call institutional.

The three economic criteria are rather obvious and require only summary explanation. Effectiveness refers strictly to the balance-of-payments repercussions of a measure. It is clearly a primary desideratum, any measure failing substantially to reduce the payments deficit being ineligible for consideration at the outset.

The employment criterion refers to the effects of a measure on domestic employment—or, more generally, on economic growth. Regardless of how effective a given measure might be in reducing the payments deficit, it would be objectionable if it caused serious domestic unemployment or if it was

<sup>1</sup> One must also assume, however, that the deficit does not continue unimproved for so long that fear of eventual drastic measures induces widespread destabilizing speculative actions.

inconsistent with domestic policies for full employment and a reasonable growth rate.

The criterion of efficiency relates to the allocation of resources. Any measure that would tend to interfere with the optimum use of resources or that would aggravate an already existing deviation from the optimum pattern of resource use would violate the efficiency criterion.

Finally, the institutional criterion refers to the feasibility of a policy in the context of the contemporary world and the historical position of the United States in that world. Even though this aspect of policy has no theoretical interest to the economist, *qua* economist, any serious discussion of policy requires its consideration. A policy may not be feasible, in the sense employed here, either because it conflicts with noneconomic objectives that have priority or because it requires institutional changes that for political or other reasons are not acceptable.

An ideal policy would completely satisfy all four of the above criteria.<sup>2</sup> If there were such a policy available, presumably the issue of whether to rely on (uncertain) market forces to correct the payments deficit would disappear. Obviously, however, there is no such "ideal" policy. But there surely are different policies whose degree of fulfillment of the stated criteria vary by wide margins. The question, therefore, remains of whether there are particular measures sufficiently satisfactory to deserve consideration as an alternative to reliance on market forces.

### III. *Alternative Payments Policies*

In pursuing this question, we may begin by listing the major policies, each of which, alone or in combination with others, could conceivably be effective in reducing a payments deficit. The list is as follows:

1. Internal deflation of real income and/or prices.
2. Devaluation and establishment of new fixed dollar exchange rates.
3. Abandonment of fixed dollar exchange rates and acceptance of free or floating rates.
4. Quantitative limitation of imports or increased tariffs.
5. Export subsidies or other devices (tax rebates, etc.) directly to stimulate exports.
6. Reduced foreign military expenditure.
7. Reduced foreign aid.
8. Limitations or penalties on private capital outflows.

The list includes only those policies over which the deficit country has some control and excludes such potentially effective policies on the part of other countries as upward revaluation of currencies, expansionary monetary and fiscal policies, control over capital inflows, etc.

It may appear that one very important possible approach to reducing the payments deficit has been omitted—namely, measures to increase productivity

<sup>2</sup>I am assuming that other welfare criteria—such as those relating to income distribution—are not significantly involved.

while holding the line on wage and profit increases. In the Brookings study [6] this is the principal avenue of the projected improvement over the forthcoming years in the balance of payments.

While it cannot be denied that increases in productivity, accompanied by relatively stable money-wage rates and profit margins, would indeed tend to contribute significantly to improving the balance of payments and, further, would fully meet the other criteria of acceptability, unfortunately such a method is not amenable to discretionary policy determination, at least in the short run. It is true, of course, that different policies may influence differently the rate of productivity increases and the level of factor prices, and that particular policies might be identified as more contributive to economic growth at stable prices than are other policies. But the influence of policies in this area is uncertain, marginal, and, in any event, long-run. It is fairly easy through discretionary policies to *retard* the rate of growth and *increase* money wages and profit margins; it is much more difficult to accelerate the rate of growth while restraining wage rates and profit margins.

These comments do not argue against the pursuit of policies most likely to influence favorably growth with price stability; they only argue against the view that such policies can be expected to have a significant impact within the space of the next few years during which the balance-of-payments problem must in some fashion be solved.

#### *IV. Effective But Otherwise Objectionable Measures*

The first point suggested by an examination of the alternative policies listed is that those which best meet the criterion of effectiveness are the worst from the point of view of one or more of the other criteria. For sheer effectiveness in reducing a payments deficit, internal deflation, abandonment of fixed exchange rates, and quantitative limitation of imports rank at the top. Deflation is the most serious violator of the employment criterion, quantitative limitation of imports is the most serious violator of the efficiency criterion, and abandonment of fixed exchange rates is objected to mainly on the basis of the one noneconomic criterion, institutional. Undoubtedly this is the reason why there is a consensus among economists opposing deflation and import controls but far from a consensus opposing free exchange rates. Disagreement over the abandonment of fixed rates as a method of eliminating the payments deficit arises in part out of differences in opinion as to the merits of fixed rates versus free rates in encouraging an optimum level of world trade and investment [2] [3] [4, Ch. 15] [7]. But this is an economic argument, whereas the more definitive consideration so far as policy-making is concerned is essentially noneconomic in character, embracing such questions as the effect of abandoning fixed rates on the financial and economic (and perhaps political?) leadership of the United States in the Free World. Whatever the economic merits of the case, it would appear that fundamentally changing the system of pegged exchange rates is not at present a feasible choice so long as alternative measures involving a smaller departure from established institutions and practices continue to be available.

### V. Devaluation

If abandonment of pegged exchange rates is not feasible, what about devaluation of the dollar and its repegging at a lower gold content? The employment and efficiency criteria are as well satisfied as they are with floating rates, but the institutional criterion is violated to nearly the same extent as it is with floating rates, and, in addition, devaluation carries the risk of being ineffective in eliminating the payments deficit.

The institutional criterion is not met by devaluation because it would involve a fundamental shock to the present international monetary system based on a gold- and dollar-exchange standard. The status of the dollar as an international monetary "anchor" would in all probability be destroyed if the weight of the anchor were to be allowed to change.

The second objection to devaluation is its potential ineffectiveness. It may be ineffective for either one of two kinds of reasons. The first is that it may evoke counterdevaluations by other countries, especially Western European countries and the United Kingdom, which are the chief international competitors of the United States, and Canada, which is the largest market for U.S. exports.

Assuming the problem of counterdevaluation is overcome, the second reason devaluation may be ineffective is more strictly economic in character, relating to the difficulties of selecting the right degree of change in the international value of the dollar. Various estimates of the amount of overvaluation of the dollar have been made [8], and although these may be reasonable, there is no known way of being certain they do not contain a large margin of error. The danger of too small degree of devaluation can be reduced by choosing the amount of devaluation indicated by the higher ranges of estimated overvaluation. However, in this case the opposite danger is run of ending up with an undervalued dollar, thereby increasing the probability of counteraction by other countries, either in the form of devaluation of their currencies or in the form of various import restrictions. Again, the special international position of the dollar leaves less room for mistakes than in the case of other currencies.

### VI. *Reduction of Foreign Military Expenditure and Aid*

If, as I have so far concluded, there are strong objections to deflation, import controls, abandonment of fixed exchange rates, and devaluation, the field of choice is considerably narrowed. It can immediately be further narrowed by eliminating two other possible measures—reduction of military expenditure and reduction of foreign aid.

There is one common ground on the basis of which both of these latter measures are rendered unacceptable as a means of correcting the payments deficit—they are autonomously determined by noneconomic objectives. They are both instruments of foreign policy and therefore not appropriately determined by balance-of-payments considerations.

A second, though subsidiary, consideration applicable to foreign aid is the ineffectiveness of reductions in significantly contributing to eliminating the payments deficit. Because aid recipients tend to spend most of their grant

and loan dollars directly on U.S. exports, reinforced by the government policy of tied aid, only a fraction of any reduction in aid would be reflected in improvement in the U.S. balance of payments [6, pp. 155 ff.].

### VII. *Export Subsidies*

Theoretically, exports could be increased just as effectively through subsidies—direct or indirect—as through devaluation. Indeed, a uniform subsidy for all exports would be tantamount in effect to a “half” devaluation. (The effect could be made “full” by simultaneously imposing a uniform ad valorem tariff on all imports.) However, the efficiency and institutional criteria are seriously by-passed by export subsidies, and, in addition, the effectiveness criterion is not likely to be met.

Export subsidies are the counterpart of import duties and, like them, distort international cost-price comparisons which underlie the optimum allocation of resources internationally. And, again like import tariffs, subsidies go counter to the prevailing and generally accepted code of commercial conduct. Finally, the balance-of-payments effectiveness of subsidies is rendered doubtful because of probable retaliation by other countries, either in the form of increased import tariffs against the subsidizing country's goods or in the form of subsidies on their own export goods.

### VIII. *Capital Controls*

The last remaining measure to investigate is control over capital outflows. While there are certainly serious objections to this policy, there are reasons to believe they may be less serious, in the context of the present balance-of-payments problem of the United States, than any of the other alternatives available. This conclusion is supported by examining capital controls from the point of view of each of the four criteria of acceptability.

#### A. *Effectiveness Criterion*

Starting with the effectiveness criterion, the first question is to what extent the payments deficit is attributable to private capital movements. In view of the interrelationships among the various items in the balance of payments, it is practically impossible to identify with precision the net role of any one item. But theoretical considerations, plus empirical studies, permit conclusions as to the general nature and direction of the balance-of-payments effect of capital movements.

There are many different possible approaches to explaining the reasons for the U.S. payments deficit, and nearly all of them have been cited [9]. Different approaches may serve different purposes. If the purpose of the analysis is to find appropriate policy measures to correct the deficit, approaches may be broadly classified according to whether the emphasis is on enlarging the current-account surplus or on reducing the autonomous capital-account (including unilateral transfers) deficit.

In terms of the *causes* of the deficit, there is no basis for choosing between these two approaches. One may equally well say either that the deficit is caused by the failure of the “transfer” mechanism or that it is caused by a

too large capital outflow. These are saying the same thing, for a "too large" capital outflow can be meaningfully identified only in terms of a "too small" current-account surplus, and likewise "failure" of the transfer mechanism can be identified only in terms of an insufficiently large current-account surplus.

Capital outflow contributes to a payments deficit to the extent that the outflow does not, directly or indirectly, lead to an equivalent current-account surplus. Conversely, a reduction in capital outflow contributes to eliminating a deficit to the extent that it does not lead, directly or indirectly, to an equivalent reduction in the current-account surplus.

On theoretical grounds, the influence of capital flows on the current account of the balance of payments is in the direction of an accommodation. In the absence of countervailing policies, and with fixed exchange rates, a capital movement tightens money and discourages spending in the capital-exporting country and eases money and encourages spending in the capital-receiving country. However, there is no a priori way of knowing whether these price and income effects will be sufficiently strong in any particular case to bring about a real transfer equal to the money transfer. Either underadjustment or overadjustment may occur, depending upon numerous conditions—such as reserve positions, propensities to spend, institutional ties, etc.—which are a matter for empirical determination [4, Pt. II]. Moreover, and perhaps most important in the modern world, the transfer mechanism may be either reinforced or completely swamped by discretionary policies on the part of monetary and fiscal authorities.

In the case of the United States in recent years, the strain on the adjustment mechanism from capital outflow has been great, and the evidence indicates its failure fully to meet the challenge.

A sharp change in the volume of private long-term capital movements out of the United States occurred in 1956. Compared to an annual outflow averaging about \$0.8 billion during the preceding five years, beginning in 1956 and continuing through 1962 the outflow reached an annual average of about \$2.7 billion [6, pp. 278-79].

The nearly two billion dollars of added annual payments on account of private long-term capital outflow from 1956 onward did not, of course, contribute an equivalent net amount to the payments deficit. Even in the absence of indirect adjustment processes, the outflow of capital can be expected directly to cause an increase of exports of goods and services, especially in the case of direct foreign investments, which ordinarily involve the capital-exporting country's shipment of equipment and materials, the receipt of management fees, etc. Some portfolio investments have similar direct effects—as, for example, when the recipient is a capital-hungry underdeveloped country with close trade ties to the United States.

In addition to contributing to the sending-country's exports, a capital outflow also contributes to current receipts in the form of interest and dividends, and, for direct investments, other "invisible" receipts. However, the benefits of such return flows are received only after some delay, the length of which depends upon various circumstances. The balance-of-payments "payoff" pe-

riod for U.S. direct investments has been estimated as between four to seven years [1, pp. 412-13] [6, pp. 142-46], though under some assumptions with respect to investments in Europe and Canada the payoff period may be as long as 10 to 15 years [1, p. 413]. In the case of portfolio investments, at average rates of return received in recent years, "it will be a very long time before even a single investment outflow is recouped in cumulative interest and dividend receipts" [1, p. 416].

While the lack of complete information on all the relevant variables and functions makes it impossible to specify with precision the net balance-of-payments effect of the outflow of U.S. capital, a careful econometric investigation by Philip W. Bell [1] led to the general conclusion that "Shifts in the flow of private capital, in particular in U.S. private capital flowing overseas, have been a major cause of our deteriorating balance-of-payments position since 1956" [p. 458].

From a long-run point of view, after taking into account return flows, capital movements out of the United States may have a net favorable balance-of-payments effect. Thus, as frequently noted, in recent years U.S. income receipts from past foreign investments have exceeded the annual outflow of new capital. But this is not a particularly relevant consideration if the immediate problem is to reduce the balance-of-payments deficit within the course of the next few years. Receipts from past investments would presumably continue unabated regardless of the level of future capital flows. Any reduction in new capital outflows can therefore be expected to have a favorable balance-of-payments impact during the period for which such a result is critical.

For the purposes of the remaining discussion, I shall assume that the balance of payments of the United States could be substantially improved over the course of the next four or five years by reducing the outflow of private capital.

### *B. Employment Criterion*

Turning now to the second criterion of acceptable policy—the employment criterion—control over capital movements falls in between the adverse effects associated with internal deflation and the favorable effects associated with such measures as devaluation. A reduction in capital outflow would cause an adverse effect on domestic employment to the extent that exports are reduced by the diminished outflow of capital. On the other hand, employment would be increased to the extent that domestic investment expenditure is substituted for foreign investment. Neither effect is likely to be very significant, however, and for practical purposes it is fairly safe to regard the net domestic employment effect as neutral.

There are advantages to balance-of-payments policies that do not directly increase (or decrease) domestic employment. Any policy that simultaneously improves the balance of payments and increases domestic employment is very likely to *decrease* employment in other countries. Thus, devaluation, to the extent that it succeeds in reducing the deficit by decreasing imports and in-

creasing exports, exerts a deflationary impact on other countries. Besides being unfortunate on its own grounds, such foreign effects increase the probability of countervailing measures abroad that would negate the initial favorable balance-of-payments effect.

It may be argued, however, that reduction in the payments deficit inherently involves an adverse foreign employment effect, regardless of the means employed to bring about the reduction. To the extent that the payments deficit is eliminated by increasing the current-account surplus, this is necessarily true. But if the deficit is attacked via the capital account it is a different matter.

Looking at it from the point of view of the capital-recipient countries, a distinction should be drawn between those countries whose rates of domestic investment expenditure is significantly dependent upon foreign capital inflow and those where such is not the case. Generally, the former consist principally of underdeveloped economies, and the latter of developed economies. A decreased inflow of foreign capital may well cause decreased investment in underdeveloped countries and thereby reduced employment opportunities. It is much less likely that total domestic investment expenditure would be significantly affected by diminished capital inflow into already developed countries, such as those of Western Europe.<sup>3</sup>

The unfavorable foreign employment effect of capital controls could be largely eliminated by confining restrictions to capital flows going to developed countries. Since the adverse balance-of-payments effect of capital flows is probably highly concentrated in these particular movements, such selective application of controls would not greatly reduce their effectiveness in improving the balance of payments.

There remain for discussion the efficiency and institutional effects of capital control. Because of the larger issues involved in the former, I shall reserve discussion of it until after the institutional criterion is examined.

### *C. Institutional Criterion*

It must be admitted forthwith that one of the strongest objections to capital control by the United States is the violation it would cause to the principle of freedom in international economic relations. This principle has two aspects, one domestic and the other international.

From a domestic point of view, the United States has a long-established principle that individuals and businesses should be free to invest as they see fit, both at home and abroad. This is part and parcel of a more general philosophy of government and economics, and deviations cannot be taken lightly. Justification for restricting freedom of international investment may nevertheless be found on at least two grounds.

The first is based on another equally valid and accepted principle—namely, that when the national interest demands it, the interests of particular indi-

<sup>3</sup>Of the major recipients of U.S. capital, Canada may be an exception to this general rule. If so, consideration might be given to including Canada in the exemption referred to in the next paragraph.

viduals become secondary. I think it can be maintained that the balance-of-payments problem of the United States is one of critical importance, and, if left unsolved, it could cause great harm to both the domestic and international economy. Therefore, measures that otherwise would be unacceptable may become acceptable, provided they would effectively contribute to its solution.

Support for this position is strengthened by the fact that failure to correct the payments deficit in itself results in a loss of freedom that is in some respects much more serious and extensive than the loss of freedom to invest abroad without restriction. Freedom is of very limited meaning if opportunities for exercising it are absent. The presence of a payments deficit imposes constraints upon domestic policy which, in turn, reduce opportunities for *domestic* investment. Specifically, the payments deficit directly restrains growth of the domestic economy and prevents the adoption of expansionary monetary and fiscal policies which would be capable of creating greater investment opportunities at home.

Turning now to the international aspect of capital controls, again there is the objection that they would go counter to the principle of freedom which the United States has vigorously espoused. However, there are two points to be noted in this connection.

The first is that, unlike quantitative trade controls, control over capital movements is recognized as a legitimate exception to the general principle of freedom of international economic relations. This acceptance is formalized in the "codes" of conduct set out in the charter of the International Monetary Fund.<sup>4</sup>

The second point is that nearly all other countries are currently, or have in the recent past, employed control over capital movements as a device for correcting or protecting their balances of payments. In short, it is a policy both accepted in principle and sanctioned by practice and therefore not subject to legitimate criticism by other countries.

#### D. *Efficiency Criterion*

Finally, we arrive at the examination of capital control from the standpoint of the efficiency criterion. From a strictly economic point of view, this is where the most important issues arise, for freedom of capital movements is strongly embedded in the tradition as a necessary part of the market mechanism allocating resources in an optimal fashion. If it can be shown that capital controls would seriously interfere with the optimum allocation of resources, the economic case against them might be decisive.

The theory of gain from long-term capital movements is well known and requires only brief summary [5]. The optimum allocation of capital resources requires that the marginal net rate of return be equalized in alternative uses, both at home and internationally. If the marginal rate of return is higher in country B than in country A, world output and the national income of A

<sup>4</sup> Section 3 of Article VI of the Articles of Agreement of the International Monetary Fund states that, "Members may exercise such controls as are necessary to regulate international capital movements. . . ."

and B are increased by the movement of capital from A to B until the marginal rate of return is equalized in the two countries.<sup>5</sup>

Under ideal conditions, the flow of capital from areas of lower marginal yield to areas of higher marginal yields will take place in response to market forces. In the real world, these ideal conditions are never completely fulfilled and, indeed, may under particular circumstances fall so far short as to cause a *perverse* movement of capital. The absence of ideal conditions is not, *per se*, a reason for concluding that actual capital movements are undesirable from a welfare point of view or that capital controls are justified. It depends upon the extent and seriousness of deviations from the ideal in particular cases.

In the case of the United States, there is a strong presumption that over the long run she should be a net exporter of long-term capital. But there is a serious question of whether the large increase in capital outflow of recent years can be justified on welfare grounds. Leaving aside the distortion caused by imperfect markets, differential rates of taxation and of subsidies, and similar factors that are always present to some degree, special elements have arisen in the last few years to weaken the presumption that uncontrolled capital movements are desirable.

The first of these special elements is the slowdown in the rate of economic growth of the United States, accompanied by above-normal unemployment, in conjunction with extraordinarily high growth rates and below-normal unemployment in Western Europe. There is little room for doubting that the increased flow of capital—especially in the form of direct investment—to Western Europe has been stimulated by the relative boom conditions there compared to the United States.

Now it can be argued that an outflow of capital induced by more prosperous conditions abroad than at home is perfectly rational and economically desirable from the points of view of both the sending and recipient countries. The sending country finds productive employment for some of its "excess" saving, and the receiving country is supplied with additional resources to relieve the strain on its economy. Even if it actually works this way (see discussion below), however, the benefits derived are not those stemming from a more efficient allocation of resources but rather from a fuller use of resources.

An outflow of long-term capital caused by differences in cyclical stages at home and abroad cannot be defended on the basis of the efficiency criterion. Higher rates of return abroad are in this case not an indication of a relatively greater scarcity. Presumably, such differences in return are temporary and would tend to be eliminated, *without* a flow of capital, as recovery in the capital-exporting country occurs.

In short, the comparison of marginal rates of return is relevant as a guide to proper resource allocation only when the rates are those emerging under full employment both at home and abroad. Such has not been the case dur-

<sup>5</sup> The marginal test may not be adequate in those cases where large flows of capital are economic, even though smaller movements are not, such as movements to underdeveloped countries to finance "social overhead" and infrastructure investment.

ing much of the time of the great increase in capital outflow from the United States in recent years.

The second special element that weakens the objection to capital controls on efficiency grounds has been the distorting influences exerted by the European Economic Community. If it is true, as commonly asserted, that U.S. capital has been attracted to Western Europe in order to get "under" the common tariff wall being erected there, the adverse efficiency effects of tariffs may be only compounded by the movement of capital into the area. If we suppose that capital would not have moved into Europe if a discriminatory tariff wall were not to be created, the actual capital movement cannot be regarded as contributing to the more efficient use of resources.

The third special element interfering with the process of optimum allocation is institutional in character. Compared to the financial markets in the United States, in most other countries they are "thin." It is considerably easier to float new issues in New York than in most other financial centers. The apparent lack of any significant relationship between interest rates in New York and in other centers and the floating of new foreign issues [1, pp. 419-20] suggests that the attraction of New York as a capital market has to do less with the cost of borrowing than the availability of credit and the ease of acquiring it. To the extent that the latter is a function of more developed financial institutions rather than simply of capital supply, there is no assurance that the flow of capital is desirable on welfare grounds.

Finally, I turn to what is perhaps the most important point of all. In order for a money capital movement to result in a more efficient allocation, it must lead to or be accompanied by a *real* capital movement. Real capital movements take the form of net current-account balances. To the extent that a net current-account balance of equal amount (but of opposite sign) to a money capital movement fails to develop, the capital movement aborts. In such cases, the capital outflow is accompanied by a reverse movement of short-term capital of an accommodating character and/or by an outflow of gold. If these are temporary movements, they are normal stages in the process of adjustment, but when they persist it is an indication that adjustment and a real transfer have failed to occur.

It will be observed that the failure of real capital to move with money capital is tantamount to one variety of balance-of-payments disequilibrium. What we are saying, then, is that, if a country incurs a deficit disequilibrium accompanying an outflow of long-term capital, control over the capital outflow cannot be regarded as contrary to the efficiency criterion, for real capital is not in fact being reallocated.

The issue runs deeper than this, however, for it might be argued that if there is an autonomous long-term capital movement out of a country, what is required, on welfare grounds, is not suppression of the movement but rather adjustment of the balance of payments to the movement. This argument is applicable, of course, only on the assumption that the marginal productivity of capital is higher abroad than at home. But even with this assumption, is it a valid argument?

If balance-of-payments adjustment to the capital movement could be brought about by means that have no adverse welfare effects elsewhere in the economy, the answer is unambiguously in the affirmative. But if, in order to match the money capital flow with a real capital movement, other welfare criteria are violated, the argument for allowing the capital freely to move is a "second best" argument of doubtful validity.

The issue therefore comes back to the more general question this paper began with: which of the available alternative approaches to correcting the payments deficit is the most satisfactory? On the basis of the criteria I have proposed, it was concluded that there are no policy measures which would effectively adjust the balance of payments to the current volume of autonomous capital outflow (plus unilateral transfers) without serious violation of one or more welfare criteria. If this is the case, it would appear that control over capital outflow may be a more feasible and acceptable means of balance-of-payments adjustment than attempts to force the current account into equilibrium relationship with uncontrolled capital movements.

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#### External Economies and Second-Order Optimality Conditions

This note explores the somewhat surprising relationship between external economies (or diseconomies) and the second-order conditions for maximization of the social welfare function. I shall argue that externalities directly reduce the likelihood that the second-order conditions will be satisfied, thereby significantly complicating the pertinent problems of public policy. I will also suggest that this interconnection helps to explain an

interesting argument about externalities and optimal output levels which has recently been put forth by Professors Buchanan and Kafoglis [2]. These authors have argued by counterexamples, both hypothetical and observed, that, contrary to what is normally assumed, the presence of external economies generated by activity  $X$  need not cause the level of activity  $X$  to be below the social optimum. Even if external economies are generated by their operations, industries or firms may voluntarily produce adequate or even excessive outputs, and consumers whose purchases confer indiscriminate social benefits may nevertheless buy more than the ideal amounts.

This comment should help to substantiate the validity and significance of the Buchanan-Kafoglis argument. However, I will try to show how their contention fits in with the Pigovian position, which these authors question, and how it relates to the corpus of value theory. I shall argue that their result is also a consequence of a more general and rather more disturbing theorem, one which tells us that even the second-order conditions are of little reassurance to the policy-maker who takes a partial and piecemeal approach to the determination of ameliorative measures.

### *I. Second-Order Conditions and Optimal Outputs under External Economies*

It does indeed seem plausible, as Buchanan and Kafoglis point out, that "If a municipal government should cease, forthwith, to provide any collective police protection, individuals would, surely, respond by hiring private policemen [and] . . . quite likely . . . , in such a situation, total resource outlay on providing protection to life and property would be greater than under collectivization" [2, pp. 412-13]. Whether or not one is persuaded by such an illustration, it should at least induce us to review the simple standard argument which seems to lead to contrary conclusions. In equilibrium any consumer or producer will have carried his activity to a point where its marginal private benefit equals its marginal private cost (which, for simplicity, we assume to be equal to marginal social cost). If the marginal social benefit exceeds marginal private benefit, we clearly have  $MSB > MC$ , and it would appear that, from the point of view of society, an increase in the activity level must *necessarily* be beneficial.

There is, strictly speaking, nothing wrong with the argument as it stands. We get into trouble only if we proceed one more step and argue that the optimal output is necessarily larger than the equilibrium output. For there is one very obvious reason why this result may not hold—the second-order maximum conditions may just not be satisfied. In these circumstances there are likely to exist local maxima in the social welfare function, and a move that increases net social benefits may then well lead us toward one of these little hills in the welfare function and away from its global optimum.<sup>1</sup> In other words, the Pigovian argument assures us that *some* benefit

<sup>1</sup> Note that Pigou [6, p. 224] explicitly makes the appropriate reservation: "Disregarding the possibility of multiple maximum positions, I propose . . . to call [the national dividend maximizing output] . . . the ideal output. Under conditions of simple competition, if in any

can be achieved by increasing external-economy-yielding activity levels from their equilibrium values, but it by no means precludes the possibility that still greater gains will sometimes result from restriction of these activities.

So far, we have only expressed a reservation which applies to all of value theory. But in the present context there is more to the story. We shall see presently that there is, in fact, some connection between the presence of external economies and the likelihood that the second-order conditions will somewhere fail to hold. But first I shall offer an explanation for the Buchanan-Kafoglis result which is even (at least to me) more surprising and more disturbing.

## II. *Second-Order Conditions and Partial Policy Measures*

Let us restate the proposition that Buchanan and Kafoglis dispute in somewhat more general terms. Suppose the social welfare function *does* satisfy the second-order maximum conditions and that we are at a point where its partial derivative with respect to activity  $a_1$  (its marginal social benefit) is positive. Then it is a plausible allegation, which seems to be widely accepted, that an increase in  $a_1$  will *not* move us away from the true maximum. In other words, if the second-order conditions hold and  $a_1$  has a positive marginal yield, this statement alleges that one can confidently recommend an increase in  $a_1$  as a step toward the social optimum. Unfortunately, though it is plausible and, perhaps, widely accepted, the proposition is false.

Thus consider the following counterexample.<sup>3</sup>

$$\text{Maximize } W = -2a_1^2 + a_1a_2 - 2a_2^2.$$

Using the notation  $W_{12} = \partial^2 W / \partial a_1 \partial a_2$ , etc., we obviously have:

$$\begin{aligned} W_1 &= -4a_1 + a_2, & W_2 &= a_1 - 4a_2, \\ W_{11} &= -4, & W_{12} &= W_{21} = 1, & W_{22} &= -4, \end{aligned}$$

hence:

$$\begin{vmatrix} W_{11} & W_{12} \\ W_{21} & W_{22} \end{vmatrix} = \begin{vmatrix} -4 & 1 \\ 1 & -4 \end{vmatrix} = 14 > 0, \quad W_{11} < 0, \quad W_{22} < 0$$

so that the second-order conditions are satisfied. Moreover, it follows di-

industry the value of the marginal social net product of investment is greater than the value of the marginal private net product, this implies that the output obtained is less than the ideal output . . . ." It is not clear from this statement that Pigou saw the full significance of his unique maximum assumption. However, he was acutely aware of the policy problems caused by the presence of a multiplicity of local maxima. (Thus see, e.g., [6, p. 140].) Moreover, he seems to have given no recognition to the serious multivariable problems discussed in the next section.

<sup>3</sup> I am deeply indebted to Harold Kuhn for this counterexample. Appendix A, below, describes a model which brings out the economic circumstances implicit in the counterexample. It will also be recognized that our counterexample does not really apply to the strictest form of the neoclassical welfare theorem which does remain valid. This rather narrow theorem is discussed and proved in Appendix B.

rectly by setting the first-order partials equal to zero that the maximum value of  $W$  is attained at  $a_1 = a_2 = 0$ . But  $a_1$  has a positive marginal yield at many points in the positive quadrant, e.g., for  $a_1 = 1$ ,  $a_2 = 5$ , the value of  $W_1$  is 1. In fact this is true for any positive  $(a_1, a_2)$  such that  $4a_1 < a_2$ .

Though this result may shake one of our ingrained articles of faith, it is easy to show diagrammatically how the difficulty may arise. In Figure 1a we see that the surface has a unique maximum at  $M'$ . Yet an increase in  $a_1$  from  $A'$  to  $B'$  simultaneously moves us away from  $M'$  and gets us to a higher point on the surface.

The consequences of this result for the theory of economic policy are rather unpleasant to contemplate. The planner who normally has little if any information about the details of the welfare function is, however, able to identify some activities which yield external benefits (of unknown magnitude). On the basis of this very limited information it is tempting to encourage the expansion of those activities. Our argument shows that though such an expansion will indeed increase welfare, it may lead us to neglect opportunities to obtain far larger social gains. Like the second-best theorem, our result exhibits the pitfalls that beset such piecemeal planning procedures. Even the second-order conditions cannot offer the comforting

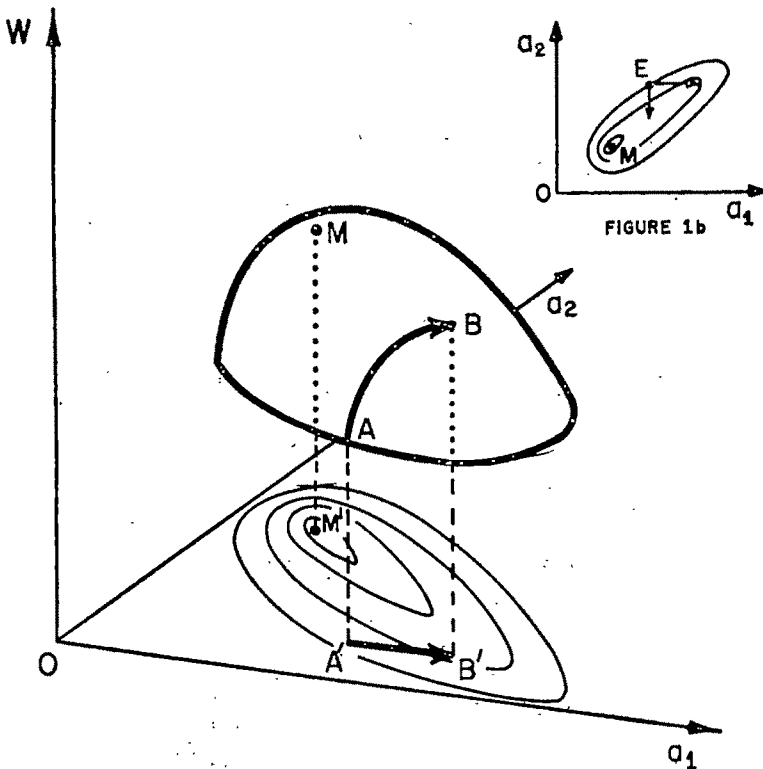


FIGURE 1a

FIGURE 1b

reassurance that a proposal which takes us uphill will really move us toward the top! The standard neoclassical welfare theory, because of its preoccupation with a partial analysis, has thus failed to warn us of what may be a very serious danger.

### III. *External Benefits and the Second-Order Conditions*

Nor are we yet done with our mischief. The policy-maker who concerns himself with externalities may still have some reason to hope that second-order problems will not be added to his concerns. For if the second-order conditions do hold, we can at least be sure that a series of changes that constantly increases social welfare by some nonnegligible amount must ultimately conduct us to the optimal point, even if it does so via a highly circuitous route. But I shall argue now that the very presence of externalities makes it less likely that the second-order conditions will be satisfied. This view seems at a number of points to be close to the surface in the standard welfare discussions. It may have been suggested by the idea that the second-order conditions, in economic terms, require activities to be characterized by diminishing returns. But external economies have gotten themselves associated with increasing returns to scale.<sup>3</sup> It would then seem to follow that the second-order conditions are rendered suspect by the presence of externalities.

It turns out that there is firmer ground for this suspicion, as I shall now demonstrate. In our economy let there be  $n$  agents (individuals, firms),

<sup>3</sup> In part this association results from the context in which much of the early explicit discussion of external economies took place. It will be recalled that one objective in using the concept was to show how, in a decreasing-marginal-cost industry, the firms which constitute it may nevertheless be subject to the increasing marginal costs necessary for them to achieve equilibrium under pure competition. And one explanation advanced was that external economies can produce decreasing costs for the industry even where the cost curve of each of its firms is rising. (See, e.g., Marshall [5, Book V, Ch. 10].) As a matter of fact, there seems to be no general reason to associate external economies with increasing returns in the sense of decreasing marginal costs that result from a rise in the industry's output, though external economies do always tend to reduce an industry's marginal costs *at a given output level*. To show this, let  $T, T_1, \dots, T_n$ , respectively, represent the total cost of the industry and its firms 1, 2,  $\dots$ ,  $n$ , and let  $Q, Q_1, Q_2, \dots, Q_n$  represent the corresponding outputs. Then the industry's marginal cost is

$$M = \frac{dT}{dQ} = d(T_1 + T_2 + \dots + T_n)/dQ = \left( \frac{\partial T_1}{\partial Q_1} + \frac{\partial T_2}{\partial Q_1} + \dots + \frac{\partial T_n}{\partial Q_1} \right) \frac{dQ_1}{dQ} + \dots + \left( \frac{\partial T_1}{\partial Q_n} + \dots + \frac{\partial T_n}{\partial Q_n} \right) \frac{dQ_n}{dQ}.$$

But if a rise of the output of firm  $i$  confers an external economy on firm  $j$  we must have  $\partial T_j / \partial Q_i < 0$ . Therefore, provided  $dQ_i / dQ > 0$ , i.e., so long as the output of firm  $i$  increases with that of the industry, the presence of any such external-economy term  $\partial T_j / \partial Q_i$  must reduce the value of  $M$ .

However, these external benefits do *not* make for increasing returns, i.e., they do not yield  $dM/dQ < 0$ , unless the magnitude of the externalities increases with the output of the industry.

Of course, it is always possible to associate increasing returns with external economies if one follows Bator and simply defines the former to be a subcategory of the latter. Cf. Bator [1, pp. 365-69].

whose activity levels are measured by the variables<sup>4</sup>  $a_1, \dots, a_n$ . Then, given the social welfare function, let the welfare conferred on agent 1 be given by  ${}_1W(a_1, \dots, a_n)$ , etc., and let the social welfare,  $W$ , be the sum<sup>5</sup> of these individual measures:  $W = {}_1W + {}_2W + \dots + {}_nW$ . We may write  ${}_iW_{jk}$  for  $\partial^2 {}_iW / \partial a_j \partial a_k$ . Then if, say, agent 2's activity confers an external economy (benefit) on agent 1, we must have  ${}_1W_{12} > 0$ ; i.e., an increase in  $a_2$  must increase  ${}_1W_1$ , the direct marginal contribution of his own activity to the welfare of agent 1.<sup>6</sup> More generally, a direct external benefit conferred by activity  $j$  on agent  $i$  must involve

$$(1) \quad {}_iW_{ij} > 0.$$

Let us, for the sake of simplicity, assume that we can ignore what may be called "indirect externalities,"<sup>7</sup> i.e., that all

$$(2) \quad {}_iW_{jk} = 0 \quad \text{for all } i \neq j \text{ and } i \neq k.$$

It will be recalled that the second-order maximum conditions require

$$(3) \quad W_{ii} < 0, \quad \begin{vmatrix} W_{ii} & W_{ij} \\ W_{ji} & W_{jj} \end{vmatrix} > 0, \quad \begin{vmatrix} W_{ii} & W_{ij} & W_{ik} \\ W_{ji} & W_{jj} & W_{jk} \\ W_{ki} & W_{kj} & W_{kk} \end{vmatrix} < 0, \dots$$

for all  $i, j, k, \dots$ .

Then we have the following theorems:

**Theorem 1.** In the absence of all direct and indirect externalities and provided all activities exhibit diminishing returns, the second-order maximum conditions (3) will necessarily be satisfied.

**Proof:** In the absence of indirect externalities, condition (2) must hold, and similarly, because there are no direct externalities, we must have all  ${}_iW_{ij} = {}_iW_{ji} = 0$ . Hence, all off-diagonal elements of the determinants in (3) must be zero, so that (3) becomes

$$(4) \quad W_{ii} < 0, \quad \begin{vmatrix} W_{ii} & 0 \\ 0 & W_{jj} \end{vmatrix} > 0, \quad \begin{vmatrix} W_{ii} & 0 & 0 \\ 0 & W_{jj} & 0 \\ 0 & 0 & W_{kk} \end{vmatrix} < 0, \text{ etc.}$$

<sup>4</sup> The possibility that an agent may engage in several activities makes no substantial difference to the argument. Nor is more trouble caused by the fact that it is not always possible to measure the level of an activity by a single number. This is argued in footnote 8, below.

<sup>5</sup> The assumption that the social welfare function is just the sum of the individual welfare functions involves no loss of generality for our purposes. All of the arguments that follow remain valid so long as  $\partial W / \partial {}_iW > 0$  for all  $i$ , i.e., so long as the social welfare is an increasing function of the welfare of each member of the community, for then the signs of the partials of  $W$  with respect to the  $a_i$  remain unchanged.

<sup>6</sup> This definition of "externality" has implicit features that require some examination. These are discussed in Appendix C, below.

<sup>7</sup> E.g., activity 3 must have a negligible influence on the marginal external effects of activity 2 on agent 1.

Since, by hypothesis, diminishing returns hold throughout, we must have  $W_{ii} < 0$  for all  $i$ , and all other conditions in (4) follow at once.<sup>8</sup>

We may also prove:

**Theorem 2.** If there are some external economies and no diseconomies present in the system (or some external diseconomies and no external economies), then if the externalities are of sufficient magnitude, the second-order conditions (3) will be violated.

*Proof:* Suppose we have  $W_{ij} > 0$  and all  $W_{rr} \geq 0$ . Since  $W_{ji} = W_{ji} + W_{ji} > 0$  and, similarly,  $W_{ij} > 0$ , then we must have a negative second-product term in the determinantal expansion,

$$\begin{vmatrix} W_{ii} & W_{ij} \\ W_{ji} & W_{jj} \end{vmatrix} = W_{ii}W_{jj} - W_{ji}W_{ij}.$$

But because there are no diseconomy components to balance off the external-economy components, sufficiently strong externalities, i.e., sufficiently large  $W_{rr}$ , will, *ceteris paribus*, render the value of this determinant negative, thus violating one of the second-order requirements, (3).

Thus we see that there is a rather strong connection between externalities and the second-order conditions, but that external diseconomies can be just as troublesome in this respect as can external economies.<sup>9</sup>

#### IV. Externalities and Second-Order Relationship: Explanation

Though the source of the relationship between the presence of externalities and the second-order conditions is not immediately obvious, an intuitive explanation can be provided. First, it is easy to offer an illustrative

<sup>8</sup> If there are several activities per person but no externalities, and the second-order conditions are satisfied for each individual, then, even though not all off-diagonal elements need be zero, the second-order conditions for the social welfare function must still necessarily be satisfied. For the matrix of the second-order conditions may then be partitioned into

$$\begin{pmatrix} \alpha_1 & 0 & \cdots & 0 \\ 0 & \alpha_2 & \cdots & 0 \\ \vdots & \vdots & \ddots & \vdots \\ 0 & 0 & \cdots & \alpha_k \end{pmatrix}$$

where each  $\alpha_i$  is a matrix whose principal subdeterminants all satisfy the second-order conditions and, therefore, the product matrix  $\alpha_1, \alpha_2 \cdots \alpha_k$  must also do so.

<sup>9</sup> I must be careful to avoid implying that Buchanan and Kafoglis' counterexamples and arguments can all be classed under the theorems of Sections II and III. They point out, justly, that a number of writers on welfare economics (myself, no doubt, included) have carelessly equated the desirability of an increased output by some industry with an injunction to increase the allocation of resources to that industry. But if the increased outputs are produced under central management because of the externalities, it may be that their mode of manufacture can be reorganized in a more efficient manner so as to obtain larger outputs from smaller inputs. Surely that is the rationale behind the Buchanan-Kafoglis example where there is reduced employment of policemen when a central authority takes over the function of police protection. There is very likely a larger output (increased safety for the citizen), but it takes smaller input quantities (fewer police officers) to achieve this happy result.

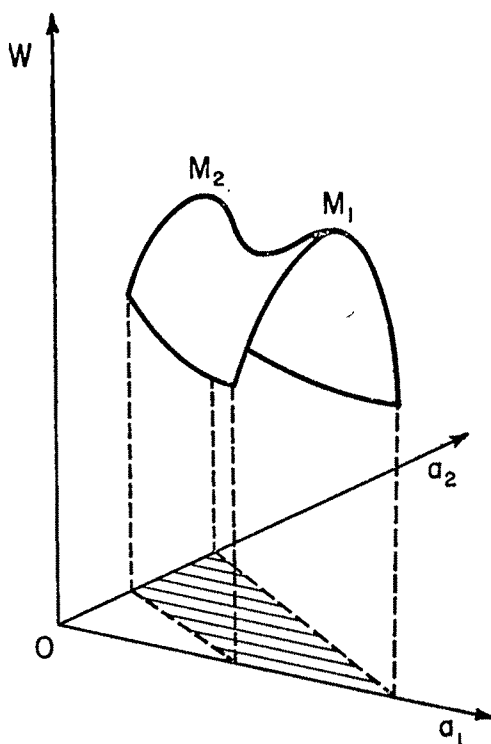


FIGURE 2. EXTERNAL DISECONOMIES

algebraic relationship by means of a slight modification of an earlier example,

$$W = -a_1^2 + ka_1a_2 - a_2^2,$$

to which we can always add a constant term (without changing the analysis) to make sure that some activities yield a positive amount of total welfare. On our criterion, this function yields external economies if  $k > 0$  and diseconomies if  $k < 0$ . Moreover, our determinantal second-order conditions show that this relationship violates the second-order conditions if either  $k > 2$  or if  $k < -2$ .

We next examine the two types of case graphically. In Figure 2 an external-diseconomies welfare function that violates the second-order conditions has been plotted, while Figure 3 represents the corresponding external-economies case. The first of these figures shows that local maxima may be expected to lie directly above the axes, i.e., at  $M_1$  and  $M_2$ . The reason for this is that at such a point one of the activities, e.g.,  $a_2$  is abandoned altogether, so that its deleterious effects on the yield of the other activity,  $a_1$ , is eliminated. The returns to  $a_1$  will therefore be at a maximum at points above the  $a_1$  axis, and we therefore need not be surprised to find a maximum,  $M_1$ , there, and a corresponding explanation applies to  $M_2$ .

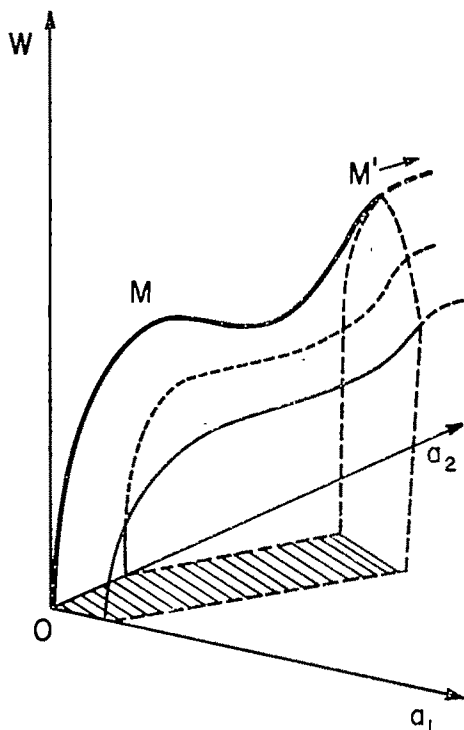


FIGURE 3. EXTERNAL ECONOMIES

In the external-economies case, the graph of a somewhat more complex relationship has been drawn to produce a *plausible* illustration of a multiplicity of optima. Here, as  $a_1$  increases, it adds to the welfare productivity of  $a_2$  and vice versa. As a result welfare may even increase without limit with the levels of  $a_1$  and  $a_2$  so that a maximum,  $M'$ , may occur at the highest levels of activity permitted by the available resources. Or, if the magnitude of the externality,  $W_{12}$ , decreases sufficiently after some point or if the diminishing returns terms,  $W_{11}$  and  $W_{22}$ , increase sufficiently in absolute value, we may obtain an interior maximum at some combination of high levels of activity,  $a_1$  and  $a_2$ . On the other hand, we can obtain a second maximum,  $M$ , corresponding to lower values of  $a_1$  and  $a_2$  if at that level the externalities have not yet risen sufficiently to offset the diminishing returns to each activity taken by itself.<sup>10</sup> This interpretation offers one comforting sop to the policy-maker, one which is, at least in spirit, at variance with the Buchanan-Kafoglis conclusions. For it tells us that, if external economies are sufficiently strong and sufficiently persistent, it will indeed pay society

<sup>10</sup> The reader can construct for himself an even more simple-minded externalities case by moving the surface in Figure 2 so that point  $M_2$  goes through the origin, and  $M_1$  lies above a 45° line through the origin in the  $a_1a_2$  plane. Here production or any other activity constantly decreases welfare until the levels of the activities are sufficiently high to yield significant external benefits.

to increase all activity levels indefinitely. For, speaking loosely, the social welfare function will then have its maximum at infinity. Unfortunately, no such simple-minded approach suggests itself for the diseconomies case where the number of local maxima may well grow with the number of individuals in the society, and where there seems to be no easy way to select among the very large number of maxima which may be encountered. 11~

#### V. Concluding Comment

It would appear, then, that Buchanan and Kafoglís have raised a fundamental point in questioning whether external economies need always call for increased outputs, and in doing so they have led us to recognize a number of ways in which we may have been misled by the received welfare analysis. While our theoretical writings often readily assume that the second-order conditions hold, the very presence of externalities may be taken to cast doubt on the validity of that premise. But where the premise fails, the increased-output argument loses its force and intuition becomes a very treacherous guide for the policy-maker. One may well suspect that the presence of externalities may sometimes even cause the second-order conditions to be violated throughout the relevant range. In that case there will exist no interior optimum points. And it is well known that where a number of alternative corner points serve as competing local optima, going uphill (i.e., increasing output because that moves us to a higher point on the social welfare surface) is apt to be a particularly unreliable procedure for getting to the global maximum. Worse still, it transpires that, in a multi-variable world, guidelines for action may not be much clearer even if the second-order conditions hold. The planner's task may be compared to an attempt on a foggy day to get to the highest point on what, for all he knows, may be the top of a ridge or the rim of a crater. Just going uphill may well take him in a very wrong direction.

WILLIAM J. BAUMOL\*

#### APPENDIX A: A COUNTEREXAMPLE MODEL

It is by no means obvious how externalities or the neoclassical considerations on the allocation of the economy's limited resources are reflected in the counterexample equation of Section II or in Figure 1, by means of which it is shown that a commodity whose output yields external benefits may be produced in overoptimal amounts by a free market, even if the second-order conditions are satisfied. Though only two activities,  $a_1$  and  $a_2$ , enter the equation or the diagram, these assume implicitly that there is a third activity,  $a_3$ , at whose expense it is possible to increase  $a_1$  and  $a_2$ . This is why the transformation function (the production possibility function) does not appear.

This can be shown with the aid of the following model. Though most of

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the literature refers to a competitive economy containing many individuals and many small firms, it is convenient in our counterexample to let our economy be composed of only three individuals who behave like pure competitors. We may represent their respective activity levels by  $a_1$ ,  $a_2$ , and  $a_3$  and their welfare functions by  ${}_1W$ ,  ${}_2W$ , and  ${}_3W$ . Let these functions be given by

$${}_1W = 2a_1a_3 - a_1^2 + a_2^2 + 14a_1 + 22a_3$$

$${}_2W = 5a_1a_2 - a_2^2 + a_3^2 - 14a_1 + 5a_2$$

$${}_3W = 2a_2a_3 - a_3^2 + a_1^2 - 5a_2 - 22a_3.$$

It will be noted that these functions contain a number of transfer payments. For example, an expansion in the level of activity  $a_1$  yields 14 (dollars?) to individual 1, but this amount is simply taken from individual 2 (the term  $14a_1$  occurs in  ${}_1W$ , and the term  $-14a_1$  appears in  ${}_2W$ ). Hence, here is a payment to individual 1 which yields no corresponding net social gain.

It is also convenient for simplicity of our relationships to measure activity levels  $a_1$ ,  $a_2$ , and  $a_3$ , not in absolute terms, but as deviations from their optimal levels, so that, e.g.,  $a_3 = -6$  means only that activity 3 is 6 units below its optimal level and that welfare maximization will require  $a_1 = a_2 = a_3 = 0$  as we shall see presently.

Differentiating each individual's welfare function in turn with respect to his own activity (and writing, e.g.,  ${}_1W_1$  for  $\partial {}_1W / \partial a_1$ ) we obtain

$${}_1W_1 = 2a_3 - 2a_1 + 14$$

$${}_2W_2 = 5a_1 - 2a_2 + 5$$

$${}_3W_3 = 2a_2 - 2a_3 - 22.$$

Since in atomistic equilibrium each individual will act independently to maximize his own welfare, this requires  ${}_1W_1 = {}_2W_2 = {}_3W_3 = 0$ . Hence, setting each of the preceding equations equal to zero and solving, we see that the competitive activity levels will be  $a_1 = 1$ ,  $a_2 = 5$ , and  $a_3 = -6$ .

Now adding the three individual welfare functions together to obtain the social welfare function,  $W$ , we have

$$W = {}_1W + {}_2W + {}_3W = 2a_1a_3 + 5a_1a_2 + 2a_2a_3.$$

Let the social transformation function be given by

$$a_1 + a_2 + a_3 = 0.$$

Using this to eliminate  $a_3$  from the social welfare function, it becomes

$$W = -2a_1^2 + a_1a_2 - 2a_2^2,$$

which is the welfare function of the counterexample in Section II. Moreover, at the competitive equilibrium point where  $a_1 = 1$  and  $a_2 = 5$ , we have as the social marginal yields of these activities,

$$W_1 = -4a_1 + a_2 > 0 \quad \text{and} \quad W_2 = -4a_2 + a_1 < 0,$$

so that  $a_1$  yields external benefits and  $a_2$  yields external diseconomies, even though *both* the activity levels exceed the social welfare maximizing levels,  $a_1 = a_2 = 0$ . (That these are the optimal values may be verified by direct differentiation of our social welfare function  $W = -2a_1^2 + a_1a_2 - 2a_2^2$ .)

#### APPENDIX B: PROOF OF THE NEOCLASSICAL EXTERNALITIES THEOREM

It should be made clear that the arguments of the text do not conflict with what may be called the *strict* neoclassical externalities theorem—a theorem which this appendix will show to be true if the second-order conditions hold. This theorem can be restated as follows:

Suppose all activities are at their competitive equilibrium levels and that activity  $a_1$  generates external benefits (so that  $W_1 > 0$ ), but that *none of the remaining activities yields any externalities* (so that  $W_2 = \dots = W_n = 0$ ). Then the socially optimal value of  $a_1$  must be greater than its equilibrium level.

It will be shown presently that the presence of any externalities—either economies or diseconomies—can invalidate the result. Unfortunately, then, the modest assertion of this strict externalities theorem is not terribly helpful to the policy-maker in a world where externalities and departures from equilibrium pervade the economy.

To prove the theorem we will assume that the social welfare function,  $W = f(a_1, \dots, a_n)$ , is everywhere strictly concave<sup>11</sup> (concave downwards).

Thus we are given:

(i)  $W = f(a_1, \dots, a_n) = f(A)$  is strictly concave.

(ii) The function has a maximum (which by our concavity assumption is necessarily unique and absolute). This maximum occurs at

$$A_{\max} = (a_1^{\max}, \dots, a_n^{\max}) \quad \text{where} \quad f(A_{\max}) = W_{\max}.$$

(iii) The function has an equilibrium point,  $A_{eq} = (a_1^{eq}, \dots, a_n^{eq})$ , where  $f(A_{eq}) = W_{eq}$ , and at which  $\partial(f/\partial a_1) > 0$  and  $(\partial f/\partial a_j) = 0$  for  $a_1 = a_1^{eq}$ ,  $a_j = a_j^{eq}$ ,  $j = 2, \dots, n$ .

We wish to prove  $a_1^{\max} > a_1^{eq}$ .

*Proof* (see Figure 4): Suppose the contrary, that  $a_1^{\max} \leq a_1^{eq}$ , and let  $U$  be the vector  $U = (1, 0, \dots, 0)$ . Then, by (iii),

(iv)  $W_{eq+\delta} = f(A_{eq} + \epsilon U) > f(A_{eq})$  for sufficiently small  $\epsilon > 0$ . Moreover, by (ii),  $f(A_{\max}) > f(A_{eq})$  and so, by (i) and (iv),

(v)  $f(A) > f(A_{eq})$  for all  $A$  on the line segment joining  $W_{\max}$  to  $W_{eq+\delta}$ .

Now, by our assumption  $a_1^{\max} \leq a_1^{eq}$ , and because  $a_1^{eq} < a_1^{eq} + \epsilon$ , there exists a point  $C = (C_1, \dots, C_n)$  on the line segment joining  $W_{eq+\delta}$  to  $W_{\max}$  and lying in the hyperplane  $a_1 = a_1^{eq} = \text{constant}$ .

But since  $W$  is strictly concave, so is its intersection ( $RS$  in Figure 4) with the hyperplane  $a_1 = a_1^{eq}$ . By (iii) it follows that  $W_{eq}$  must be the absolute maximum on this intersection so that

<sup>11</sup> Concavity of a differentiable function is tantamount to its satisfying the second-order maximum conditions. The reader will recall that a function is defined to be strictly concave if it passes the following test: Let  $A$  and  $B$  be any two points on the graph of the function, and let  $AB$  be the straight line segment connecting  $A$  to  $B$ . If every point (except the end points) of line segment  $AB$  lies below the graph of the function, then the function is strictly concave.



of the marginal private net product, this implies that the output obtained is less than the ideal output . . . [6, p. 224].

It seems strange in retrospect that there has apparently been no previous attempt to prove the Pigovian theorem, considering how often it has been discussed (and slightly amended).

#### APPENDIX C: ON THE DEFINITION OF "EXTERNALITY"

It transpires that it is not quite as easy to provide a usable formal definition of "externality" as might at first appear. This appendix offers some remarks on this subject and comments on some peculiarities in the use of the term which has been made in this paper.

Two implicit features of the definition of "externality" employed in this note should be brought to the reader's attention:

1. The usual externality concept asserts that agent 1's activity affects the *total* welfare of some other individual, ( ${}_1W_1 \neq 0$ ), and does not necessarily imply that agent 1's activity influences the *marginal* return to the activities of the other individual, 2, ( ${}_2W_{12} \neq 0$ ). However, in this note the latter, marginal criterion ( ${}_2W_{12} \neq 0$ ) is employed throughout. Clearly the two criteria do not necessarily yield the same results: e.g., if the welfare function of individual 2 is  ${}_2W = K a_1 / a_2$  (where  $a_1$  again measures the activity level of individual 1, etc.), then  ${}_2W_1 = K / a_2 > 0$ , so that in the conventional sense external economies are present; but  ${}_2W_{12} = -K / a_2^2 < 0$ , so that in terms of my usage individual 2's activity produces external diseconomies. Nevertheless, both in principle and in practice, the two criteria are not unrelated. Thus if for  $a_2 = 0$ , the total welfare of the second individual,  ${}_2W$  is unaffected by  $a_1$ , and if  ${}_2W_{12} > 0$  throughout, then we must have  ${}_2W_1 > 0$  for all  $a_2 > 0$ . That is, if the  $y$  intercept of 2's welfare function does not change but its slope (marginal value) rises throughout, then his *total* welfare must also increase to the right of the  $y$  axis. More important, most if not all of the examples of external economies which are found in the literature involve *both*  ${}_2W_1 > 0$  and  ${}_2W_{12} > 0$  (with a corresponding assertion holding for diseconomies). For example, if an increased output by firm 1 makes it easier for company 2 to obtain skilled workers or some other specialized inputs, then both the total and marginal costs of company 2 may be expected to fall.

It will be noted that my externality concept then rules out the relatively uninteresting case Davis and Whinston [4] have called "separable externalities," i.e., externalities which involve  ${}_2W(a_1, a_2) = {}_2W^*(a_1) + {}_2W^{**}(a_2)$  so that, necessarily,  ${}_2W_{12} = 0$ .

2. A second important element in the definition of an externality has, with one noteworthy exception, been overlooked in most of the formal definitions which are found in the literature. It has become customary to take the easy option and equate externalities with interdependence—that is, an externality is defined to exist if, and only if, individual 2's welfare is affected by the level of individual 1's activity. In fact, such a definition of an externality robs it of its most important welfare implications. For an

externality in the Marshallian or Pigovian sense exists *only if proper compensation is not paid* to the actor so that his private marginal gain is not equal to the marginal social gain. Only this sort of externality produces a nonoptimal competitive equilibrium. Thus, interdependence is a necessary but not a sufficient condition for the significant classical externality concept.<sup>13</sup>

Strictly speaking, the assertion that marginal social gain differs from the marginal private gain requires information about the effects of the activity in question upon at least two persons: the effects upon the actor, 1, and the effects on 2, the person whose welfare is influenced by 1's activity. For without such information we cannot judge whether or not 1's marginal gain equals that of the group (1 and 2 together). Following Buchanan and Stubblebine [3], we may then conclude that a pair of sufficient conditions for the existence of an externality (they call it a Pareto-relevant externality) require (a) that the actor, individual 1, be in equilibrium with respect to his own activity, so that  ${}_1W_1=0$ , and (b) that the marginal effect of his activity on individual 2 be nonzero, i.e.,  ${}_2W_1\neq 0$ . Thus, throughout the preceding note it is assumed implicitly that each individual is in equilibrium so that no *ceteris paribus* marginal change in his activity will produce any change in his own welfare. For then we can be sure that, if a marginal change in his activity does affect the welfare of others, its private payoff (zero) must differ from its yield to society.<sup>14</sup>

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<sup>13</sup> A little thought should make it obvious that interdependence by itself does not constitute an externality in any significant sense. Surely, my welfare is increased by the activities of most persons whose wares I purchase. But *at the margin* I pay as much for such an item as it is worth to me so that the social contribution of its supplier is matched by my payment to him.

<sup>14</sup> For a good summary statement on some of the recent literature on externalities, see Turvey [7].

#### A Note on Progression and Leisure

One of the most widely accepted conclusions of tax theory is that more progression in an income tax leads to less work effort. The conclusion may be formally stated as follows: on the assumption of declining marginal util-

ities of income and leisure, a rational individual will choose to work fewer hours if a proportional income tax is replaced by a progressive tax with an equal yield. There are two versions of what is meant by "equal yield" in this context. The first, as used by O. von Mering, stipulates that the worker would pay the same amount of tax under progression if hypothetically he continued to work the number of hours chosen under the proportional tax [2, pp. 113-14; pp. 454-55 in reprint]. The second version, which has been used by R. A. Musgrave, K. E. Boulding, and A. Williams, stipulates that the worker will in fact pay the same amount of tax after having adjusted his work hours in response to progression [3, pp. 241-43] [1, pp. 824-26] [4, pp. 65-69]. Equality of yield has therefore been defined both *ex ante* and *ex post* by the writers on this subject, but both definitions have led to the same conclusion regarding the effect of progression on work effort.

It is the purpose of this note to show that the conclusion follows irreproachably if an *ex ante* definition of equal yield is used, but needs to be qualified if an *ex post* definition is used. Given only the assumptions of rationality and declining marginal utilities, it is possible for an *ex post* equal-yield progressive tax to result in more work effort.

## I

The model for the analysis is depicted in Figure 1. The worker has an endowment of  $OA$  hours which he can use for work or leisure. The wage-rate before tax is  $OB/OA$ . After a proportional income tax at the rate of  $CB/OB$  is imposed, the opportunity-line or wage-line pivots from  $AB$  to  $AC$ . The worker maximizes welfare by distributing his hours between work and leisure in such a way that the ratio of the marginal utility of leisure to the marginal utility of income (the slope of an indifference curve) is equal to the marginal wage-rate net of tax (the slope of the wage-line). He therefore locates himself at  $D$ , choosing  $OE$  hours of leisure and  $EA$  hours of work, receiving a gross income of  $FE$  and paying a tax of  $FD$ .

We now consider the consequences for work effort of substituting various types of progressive taxes for the proportional tax. There is first the progressive tax discussed by von Mering: one which provides *ex ante* an equal yield. Such a tax gives rise to a wage-line of the form  $AGDH$ . The single kink in the line at  $G$  indicates that in our example the progressive tax is applied to two brackets of income. It is clear from the pattern of indifference curves associated with declining marginal utilities of income and leisure that the new point of tangency must lie on  $DGA$  to the right of  $D$ . Less work effort is entailed. To the worker located initially at  $D$ , the new tax presents a raised marginal rate and an unchanged average rate. There will therefore be a substitution effect adverse to work effort and no income effect at all, and von Mering is correct in asserting that in this case "a progressive income tax tends to decrease efforts more than a proportionate income tax" [2, p. 114; p. 455 in reprint].

## II

It is also evident that the *ex ante* equal-yield tax will produce a smaller yield *ex post*. We now turn to the progressive taxes which produce *ex post*



tax of this type will produce *ex post* an equal yield as a result of stimulating work effort. The wage-line  $APQ$ , which is tangent to the indifference curve  $i_2$  at  $R$  on the equal-yield line, illustrates this case.

It can readily be shown by geometrical reasoning that an equilibrium at  $R$  is consistent with our stated assumptions. The slope of the progressive-tax wage-line at  $R$  can be greater than the slope of the proportional-tax wage-line  $D$  if the degree of progression is sufficiently mild. The slope of  $i_2$  at  $S$  is less than its slope at  $R$  but may still be greater than the slope of  $i_1$  at  $D$  if the marginal utilities decline slowly enough. If the slope of  $i_2$  at  $S$  exceeds the slope of  $i_1$  at  $D$ , the geometrical requirements of the assumption of declining marginal utility of income are satisfied. The assumption of declining marginal utility of leisure implies that the slopes of indifference curves be successively steeper moving leftwards along a horizontal line, and this requirement is also satisfied in our example.

#### IV

The geometrical reasoning suggests that increased progression will induce more work effort and produce the same yield only if a rather special relationship exists between the degree of progression and the shape of the marginal-utility schedules. The conditions under which this neglected outcome is possible can be precisely specified if the model is expressed mathematically. For deducing these conditions with the least possible complexity, a special kind of progressive tax is considered, namely one which applies a proportional rate to income in excess of an exemption. The mathematical argument outlined below is designed to determine the conditions under which an increase in the exemption level of such a tax, accompanied by an adjustment in the rate sufficient to ensure equal yield, will cause more work effort. The conditions thus determined will apply without modification to the case being considered in this paper, which is the substitution of a progressive tax of unspecified form for a proportional tax. A substitution of this sort can be interpreted in the terms of the mathematical model as an increase in the exemption level from zero to some positive number, and the result which holds true for the flat-rate tax with the positive exemption will also hold true for a progressive tax of any form which imposes at the new equilibrium level of income the same average and marginal rates.<sup>1</sup>

Let  $U(y, s)$  be a utility function of disposable income  $y$  and leisure  $s$ , which is twice differentiable. To simplify the analysis we assume that the utility function is additive, that is, the marginal utility of income is assumed to be independent of leisure and vice versa.<sup>2</sup> If  $w$  is the wage rate,  $k$  the total time

<sup>1</sup> An analogous geometrical statement would be that any progressive tax giving rise to a wage-line which passed through  $R$  with the same slope as that of the wage-line  $APQ$  at  $R$  would lead to the same equilibrium as that reached under the tax giving rise to the wage-line  $APQ$ .

<sup>2</sup> Additivity is also assumed in the geometrical statement above. Most of the theoretical conclusions that have been obtained for the effect of taxation on work effort have been based on cardinal utility or the even more restrictive case of additive utility. For example, Musgrave implicitly assumes additivity throughout his discussion, while Cooper develops a proposition for the effect of a proportional tax on work effort which depends only on ordinal properties

endowment, and  $e$  the exemption level, taxable income  $x$  is given by

$$(1) \quad x = w(k - s) - e$$

where it is assumed that gross income exceeds the exemption level. Then disposable income is given by

$$(2) \quad (y = 1 - t)x + e$$

where  $t$  is the tax rate. The first-order condition for a maximum is

$$(3) \quad \frac{U_s}{U_y} = (1 - t)w.$$

The *ex post* equal-yield constraint is given by

$$(4) \quad tx = m$$

where  $m$  is the desired yield. To determine the effect of a change in the tax schedule, we differentiate equations (2), (3), and (4) with respect to  $e$  to obtain

$$(5) \quad \frac{dy}{de} + (1 - t)w \frac{ds}{de} + \frac{dt}{de} = t$$

$$(6) \quad -(1 - t)wU_{yy} \frac{dy}{de} + U_{ss} \frac{ds}{de} + wU_{sy} \frac{dt}{de} = 0$$

$$(7) \quad -tw \frac{ds}{de} + x \frac{dt}{de} = t.$$

From (7),

$$(8) \quad \frac{dt}{de} = \frac{t}{x} \left[ 1 + w \frac{ds}{de} \right].$$

Subtracting (7) from (5),

$$(9) \quad \frac{dy}{de} = -w \frac{ds}{de}.$$

Substituting in (6) and simplifying, we obtain

$$(10) \quad \frac{ds}{de} = - \frac{1}{w + \frac{y - e}{t} \left( \frac{wU_{yy}}{U_y} + \frac{U_{ss}}{U_s} \right)}.$$

---

of the utility function but obtains a conveniently expressible result only in the case in which additivity is assumed. See Musgrave, *op. cit.*, pp. 232-38 and G. Cooper, "Taxation and Incentive in Mobilization," *Quart. Jour. Econ.*, Feb. 1952, 66, 43-66, reprinted in Musgrave and Shoup, *loc. cit.*, pp. 470-92.

Thus  $\frac{ds}{de} < 0$  and more hours of work are induced when the exemption is raised if

$$(11) \quad w + \frac{y - e}{t} \left[ w \frac{U_{yy}}{U_y} + \frac{U_{yy}}{U_s} \right] > 0.$$

In the case under consideration, the initial exemption level is zero so that this condition may be written

$$(12) \quad \eta_y + \frac{y}{ws} \eta_s < t$$

where  $\eta_y$  and  $\eta_s$  are written in place of  $-y \frac{U_{yy}}{U_y}$  and  $-s \frac{U_{ss}}{U_s}$  and are the

elasticities (multiplied by  $-1$ ) of the marginal utility of income and the marginal utility of leisure schedules respectively. The replacement of a proportional tax by a progressive tax with lower average and marginal rates can therefore induce more work effort and produce the same yield if the sum of (a) the elasticity of the marginal utility of income, and (b) the elasticity of the marginal utility of leisure multiplied by the factor  $y/ws$ , is less than the rate of the proportional tax.

## V

The circumstances which give rise to this neglected result are of doubtful realism but they do fall within the assumptions of the standard theorem concerning progression and leisure. For all practical purposes the theorem remains valid, although for accuracy's sake we should qualify its conclusion in this way: if the marginal utilities of income and of leisure both decline, the substitution of an *ex post* equal-yield progressive tax on work income for a proportional tax will cause the taxpayer to reduce work effort, *except that when the marginal utilities decline slowly, a mildly progressive tax with rates substantially lower than that of the proportional tax may produce an equal yield as a result of inducing more work effort.*

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## Foreign Exchange Earnings and Price Stabilization Schemes

Whether price and output stabilization schemes for primary commodities are likely to increase or decrease foreign exchange earnings from what they would be otherwise is a question of great importance to the countries contemplating the formation of such institutional arrangements. This paper sheds some light on this question and clarifies under what circumstances Ragnar Nurkse's assertion<sup>1</sup> is valid that countries will fail to maximize foreign exchange earnings if they do stabilize prices.

The analysis abstracts from other benefits that underdeveloped countries can allegedly be expected to derive from a stabilization of world primary commodity prices, such as, for instance, increased long-run demand for these products from the industrial countries, on the grounds that stable prices would remove one of the incentives for finding substitute and synthetic materials. This conclusion follows from the idea that manufacturers often prefer a raw material with a stable price over a raw material with similar physical qualities that has a highly fluctuating price, even if that price may turn out to be somewhat lower over the long run.

On the supply side and benefiting underdeveloped countries more directly, stable product prices and demand make it possible to plan agricultural, mining, and industrial outputs so that average cost per unit of output would be at a technical minimum. Another benefit of stable prices of primary products may be more stable foreign exchange earnings which in turn would facilitate long-run development planning and the importation of capital goods. (See [5, pp. 139-276].)

However, if Nurkse's suggestion is correct and price stabilization schemes would tend to reduce over-all foreign exchange earnings, this effect could outweigh all of the other potential benefits and would thus make altogether undesirable the institution of such schemes.

### I. *The Model*

For purposes of exposition the following assumptions are made: The material is produced only in underdeveloped countries and requires no input costing foreign exchange. Production is responsive to domestic price as indicated by the supply curve shown in Figure 1, which does not shift throughout the length of the business cycle. The specific shape or position of the supply curve is of no importance at this point of the analysis as long as it has neither a zero nor a negative slope. The business cycle in the industrialized countries consists of two periods of equal length, a depression period during which the demand curve looks like  $D_1D_1$  and a boom period with a demand curve  $D_2D_2$ , parallel above  $D_1D_1$ . Domestic frictional costs

<sup>1</sup> "Stabilizing the prices received by producers interferes with the incentive to produce more when export prices are high, and serves perversely to keep up production for export when export prices are low. This is obviously not a pattern that maximizes the producing country's export proceeds over the business cycle" [2, p. 149].

of adjusting output from period to period, i.e., external diseconomies not reflected in the supply curve itself, are zero. It is clear that these assumptions imply that variations in demand are responsible for existing price fluctuations.<sup>2</sup> It is also implied that futures markets and storage of the product are insufficient to offset these cyclical variations in demand. Whatever the size of these equilibrating forces, their influence is assumed to be reflected in the shape of the supply curve or demand curve.<sup>3</sup>

In the framework of this model total foreign exchange earnings without stabilization schemes over the full cycle are equal to  $(OA \cdot OK) + (OC \cdot OM)$ .

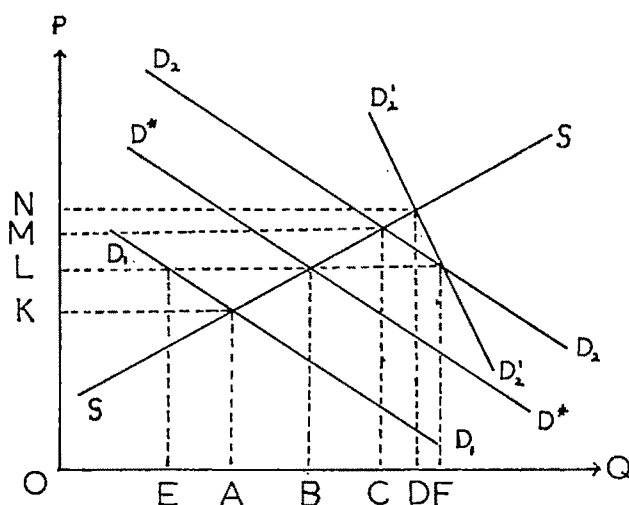


FIGURE 1

The basic question of this paper can now be reformulated to the following: Does the introduction of a stabilization scheme increase or decrease the full-cycle export earnings equivalent to  $(OA \cdot OK) + (OC \cdot OM)$ ? As is intuitively obvious, the answer to this question depends on what type of stabilization scheme is chosen and what the elasticities of the demand and supply functions are. In the following the buffer-stock and buffer-fund schemes will be examined and a brief reference to two other types of funds will be made.

## II. Buffer Stocks

Under this scheme the stabilization board acts essentially as a storage agent. It is assumed to make no profits, to accumulate stocks during depres-

<sup>2</sup> An analysis of export proceeds of primary producing countries during 1901-51 made by the United Nations [4] shows frequent parallel movements of export prices and export quantities. This reflects the dominant influence of demand conditions. J. D. Coppock also found substantial correlation between export quantity and proceeds [1, p. 143].

<sup>3</sup> Since this paper was written, an article has been published which uses much the same basic model but explores the implications which variable home demand and backward sloping supply curves have on foreign exchange earnings when prices are allowed to fluctuate and when a buffer-fund scheme is employed [3].

sion, and to decumulate them during booms. It sets an equal price for both users and suppliers and maintains it from period to period. These conditions mean that the sum of the quantities demanded during boom and depression must be equal to output at that price over the entire time interval. The demand schedule  $D^*D^*$  in Figure 1 shows the average quantity demanded at each price per half of the cycle. The condition that demand should equal supply over the entire cycle is met at a price  $OL$ . At this price output in the first period is  $OB$ , of which  $OE$  is sold and  $EB$  is stored. Second-period demand at price  $OL$  is  $OF$ , which exceeds production  $OB$  by the amount  $BF$ . Since  $BF$  is equal to  $EB$ , the amount stored in the first period is just equal to meet the excess demanded in the second and the net accumulation of stock over the cycle is zero.

Total foreign revenue at the price  $OL$  is equal to  $2(OL \cdot OB)$  and the relevant comparison is whether this quantity is equal to, smaller, or greater than  $(OA \cdot OK) + (OC \cdot OM)$ . The general mathematical formulation of these conditions is complicated even under the assumption of linear schedules, and depends on the elasticities of the functions.

By the use of the diagram, however, it is possible to show the properties of a special case from which some interesting generalizations can be made. Assume that the boom and depression demand curves in Figure 1 shift parallelly. Now it is true that during one period the increase in revenue from the buffer-stock scheme is equal to the area  $(OB \cdot OL) - (OA \cdot OK)$ , i.e., the two strips making up the area  $ABKL$ . Analogously the loss in revenue during the second period is represented by the area  $BCLM$ . But it is true that  $ABKL$  is smaller than  $BCLM$  because, on the basis of the assumptions and the shape of the schedules,  $AB = BC$  and  $KL = LM$ , so that the widths of the strips are equal but, as the diagram shows clearly, their lengths and heights differ. Thus under the given assumptions the increase of revenue in one period fails to compensate for the decrease during the second period so that the buffer stock always reduces foreign exchange earnings below what they would be if the unrestricted market were allowed to function.<sup>4</sup>

<sup>4</sup> The following algebraic proof of this theorem is due to A. C. Harberger.

$$(1) \quad q = a_1 + bp \quad \text{demand schedule during depressions}$$

$$(2) \quad q = a_2 + bp \quad \text{demand schedule during boom}$$

$$(3) \quad q = c + fp \quad \text{supply schedule}$$

$$(4a) \quad c + fp_1 = a_1 + bp_1 \quad p_1 = \frac{a_1 - c}{f - b}$$

$$(4b) \quad c + fp_2 = a_2 + bp_2 \quad p_2 = \frac{a_2 - c}{f - b}$$

$$(4c) \quad c + fp = \frac{a_1 + a_2}{2} + bp \quad p = \frac{\frac{a_1 + a_2}{2} - c}{f - b}$$

But

$$(5) \quad \frac{p_1 + p_2}{2} = \left( \frac{a_1 - c}{f - b} + \frac{a_2 - c}{f - b} \right) / 2 = \frac{\frac{a_1 + a_2}{2} - c}{f - b}.$$

This conclusion confirms the validity of Nurkse's proposition for the case of a parallel shift in the demand curve [2, p. 249]. Nurkse in his paper, however, concentrated on the supply side of the problem and failed to investigate what happens when the assumption of a parallel shift in demand is abandoned. The alternative boom demand schedule  $D_1'D_2'$  drawn in the diagram shows that the amount by which revenue changes as a result of the introduction of a buffer-stock scheme depends on the slope of the demand schedules. Thus by the way in which  $D_1'D_2'$  has been drawn, the buffer-stock revenue would be unchanged because the new  $D_1'D_2'$  curve (not shown) coincides with the old one at the critical price  $OL$ . But under the unrestricted market system boom-period revenue would be  $(OD \cdot ON)$ , which is greater than  $(OC \cdot OM)$ , while the depression revenue remained unchanged at  $(OA \cdot OK)$ . By analogous reasoning it follows that flatter  $D_1'D_2'$  schedules will reduce boom-period revenue from what it is under the parallel-shift case. In fact, a sufficiently elastic  $D_1'D_2'$  curve can lead to an excess of buffer-stock revenues over unrestricted market revenues.

Using the technique just developed and starting again from the case of parallel schedules, it is easy to see that changes in the slope of the depression demand curve have effects opposite from those experienced by changes in the same direction of the slope of the boom demand curve. More inelastic (elastic) depression schedules yield smaller (larger) depression revenues without altering boom or buffer-stock revenues if the  $D_1D_2$  curve is rotated around the price  $OL$ .

How do different supply schedules influence the conclusions reached so far? The method used to show that the introduction of a buffer-stock scheme will always reduce foreign exchange earnings in the case of parallel shifts in the demand curves is valid for any positive slope of the supply curve. However, the exact size of the reduction is dependent upon the slope. Perfectly horizontal and vertical supply curves are the limiting cases where the introduction of the buffer-stock scheme will cause no changes in foreign exchange revenues. For the sake of completeness it is worth mentioning also that a negatively sloped supply curve, given parallel shifts in the demand curves, causes buffer-stock revenues always to be greater than un-

Therefore,

$$(6) \quad \frac{p_1 + p_2}{2} = p, \quad \text{from which it follows that}$$

$$(7a) \quad p_1 = p + \Delta p$$

$$(7b) \quad p_2 = p - \Delta p.$$

Revenue during each period is equal to  $q$  times the equilibrium prices, i.e., period-one receipts  $[e + f(p + \Delta p)](p + \Delta p)$ , etc., so that the formulation of the revenue question becomes:

$$(8) \quad [e + f(p + \Delta p)](p + \Delta p) + [e + f(p - \Delta p)](p - \Delta p) \leq 2(e + f\bar{p})(p)$$

which reduces to

$$(9) \quad 2f\Delta p^2 \leq 0.$$

$\Delta p^2$  will always be positive. When  $f$  is also positive (i.e., the supply curve slopes upward and to the right), the left side will be larger than the right, which means that buffer-stock revenue will always be smaller than unrestricted market revenue. For the case of negative values of  $f$  (i.e., a backward-sloping supply curve) the conclusion is reversed.

restricted-market revenues. (For proof see the algebraic treatment in the last footnote.)

From a purely logical point of view the conclusion from this model is that the introduction of a buffer-stock scheme may or may not increase foreign exchange earnings from what they are under an unrestricted-market system. While this in itself is a valuable insight, the analysis would be more useful if it were possible to establish whether in the real world boom or depression demand schedules are typically more elastic. Unfortunately, no empirical investigations of this question seem to have been undertaken. From a statistical point of view it is probably possible but difficult because of the paucity of data to distinguish between shifts in demand curves and changes in their slopes. On the theoretical level, however, one reason comes to mind why it is likely that boom schedules are less elastic than depression schedules.

Many of the commodities for which price stabilization schemes are considered serve as factor inputs for firms in industrial countries, and demand for them is derived from the demand for the manufacturing firms' products. When during a period of high demand for this output the price of the factor input rises, firms are usually reluctant to substitute another one because such substitutions typically require changes in production processes which are often accompanied by technical uncertainties and temporary reductions in output. While it is likely that there is always some critical difference in the prices of competing inputs at which the switch is undertaken in spite of these difficulties, the point is here that the critical difference is greater during periods when demand for the firms' output is high than when it is depressed. Such a reluctance to switch to a substitute expresses itself statistically in a more inelastic demand curve for factor inputs during boom and leads to the conclusion that under the assumption of a positively sloped supply curve the introduction of buffer-stock schemes will reduce foreign exchange earnings from their free-market level.

### III. *Buffer Funds*

The basic analytical model developed above can be used to examine the influence of a second type of stabilization scheme. Instead of storing the product and setting a uniform price for both users and suppliers, the agency under this scheme sets a stable price only for the producers and lets the market in each period find a price which equilibrates quantity supplied with the quantity demand. If the stabilization agency is required to make no profit and pay out as much to producers as it receives from buyers over the entire cycle, then the price paid to the producers and the quantity supplied by them are uniquely determined. To see how such a scheme would work, consider Figure 2. The basic assumptions are the same as those underlying the first graph.  $D_1D_1$  and  $D_2D_2$  are depression and boom demand schedules,  $SS$  is the supply curve. The locus of points an equal vertical distance from the two demand curves is shown as  $D^*D^*$ . Where this schedule intersects the supply curve, the conditions are met that the agency's excess revenue during boom is exactly offset by the

deficient revenue during depression, and the total earnings over the cycle are equal to the value paid to producers. At price  $OL$  quantity  $OB$  will be produced in each period. The price to users will be established at  $OJ$  during the depression period and  $OQ$  during the boom period, yielding a revenue over the entire cycle of  $(OB \cdot OJ) + (OB \cdot OQ) = 2(OB \cdot OL)$ .

The problem of what happens to total foreign exchange earnings as a result of the introduction of the price stabilization scheme in terms of this diagram comes down to the question of comparing the areas of three rectangles. Is  $(OA \cdot OK) + (OC \cdot OM) \lesseqgtr 2(OB \cdot OL)$ ? For the case of parallel demand curves the conclusions arrived at for the buffer-stock scheme are equally applicable to this scheme because the geometric properties of the

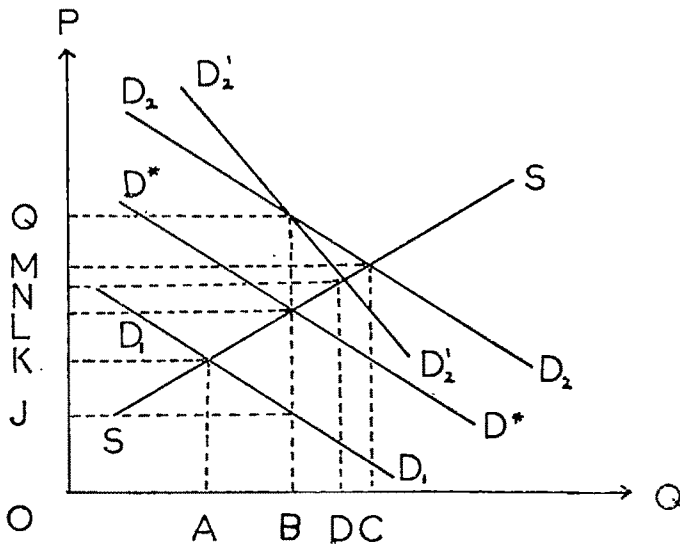


FIGURE 2

functions again are such that the relevant comparison is of the strips  $ABKL$  and  $BCLM$ . Thus the introduction of a buffer fund will always reduce foreign exchange earnings. However, different slopes of the boom demand schedule will have opposite effects from those encountered under the buffer-stock scheme. As can be seen from  $D_2'D_2'$  in Figure 2, a boom schedule less elastic than a depression schedule *reduces* the excess of free-market revenue from what it was under the parallel-shift case. In general the steeper the boom demand curve, the greater the likelihood that the buffer-fund scheme will increase foreign exchange earnings above their free-market level.

The influence which different combinations of the two demand and the supply schedules have on total revenue can be discovered by reasoning in the same way as for the buffer-stock scheme. This analysis is not presented here. The conclusion from such an exercise is again that as a matter of logic

alone the introduction of buffer-fund schemes can either raise or lower foreign exchange earnings. If, however, it is assumed that the boom-schedule is less elastic than the depression schedule and that the supply curve is positively sloped, then it follows that buffer-fund schemes are likely to lead to higher and not, as in the case with the buffer-stock scheme, to lower foreign exchange earnings.

#### IV. *Other Marketing Schemes*

Space limitations make it impossible to explore in detail the application of the basic model to schemes under which the authority is not expected to break even but where instead it seeks to pursue other objectives such as the maximization of foreign exchange earnings or the optimum exploration of its monopoly position. Suffice it to indicate briefly that the first maximization principle requires setting price and output at the points of unitary elasticity on the two demand curves. While such a scheme guarantees maximum foreign exchange earnings, the supply-curve elasticity determines whether the quantities sold can be obtained from domestic producers at an average price greater or smaller than that at which it is sold in the outside world. Thus it depends on the supply elasticity whether the scheme can be used as a taxation device or whether it requires a subsidy.

The second objective leads to a choice of output and prices on the basis of the marginal-cost-equals-marginal-revenue principle. The agency's behavior under the scheme parallels that of a monopolist discriminating in two markets. While this scheme will always improve the terms of trade for the sellers of the product, total foreign exchange earnings may increase or decrease, and simple generalizations about necessary conditions for a specific outcome cannot be made.

#### V. *Conclusions*

The model just presented could be modified to take account of such real world phenomena as typically longer recovery than depression periods of business cycles, storage costs, shifts in demand curves in response to monopoly pricing, etc. It is not likely, however, that the most general conclusion that can be drawn from this analysis would be changed. This conclusion is that the introduction of world primary commodity stabilization schemes does not necessarily increase total foreign exchange earnings accruing to the producing countries.

By introducing assumptions that are in addition to those underlying the basic model itself, it is possible to draw more specific conclusions. If demand is typically less elastic during periods of high demand than it is during periods of depression, buffer-stock schemes are likely to decrease foreign exchange earnings below what they would have been in the absence of such schemes because stable prices over the cycle forego the opportunity to exploit the inelasticity of demand during boom periods. Under these circumstances buffer-stock schemes therefore lead to a conflict between the stabilization and earnings-maximization objectives pursued by government agencies.

Given the same demand conditions, however, buffer-fund schemes do

not present such a conflict between objectives because they allow foreign prices to fluctuate and exploit the inelasticity of boom demand schedules.

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### Fluctuations in Economic Activity: Planned and Free-Market Economies, 1950-60

Three of the many claims to superiority made for planned economies over free-market economies stand out clearly: first, that they grow faster; second, that they provide full employment; and, third, that they are not subject to fluctuations in outputs [12, pp. 95-102] [23, pp. 19-35]. The first claim has been discussed at some length in Western literature. Independent indexes of growth for the planned economies have been developed by Western scholars, and the results have been analyzed and compared with similar indexes for the free-market economies [1] [11]. Considerably less attention has been paid to the second claim, possibly because measuring the degree to which the two systems utilize available inputs is an even more complex problem than the calculation of growth indexes.<sup>1</sup> Little attention has been devoted to the third claim<sup>2</sup> that "Socialism is characterized, above all, by the absence of cyclical disturbances in production" [14, p. 9]; no statistical comparisons of fluctuations in economic activity in the two systems are available.

The purpose of this paper is to compare the year-to-year fluctuations in output for a group of planned and a group of free-market economies. The first section describes the scope of the project and the data used, the second presents the results, and the third comments on the findings.

<sup>1</sup> The possibility of underutilization of inputs in planned economies has been pointed out, among others, by Joseph Berliner [2, pp. 358, 363-66], Emily Brown [3, pp. 181-82], Robert Campbell [4], Naum Jasny [9], Alec Nove [15, p. 579], Alfred Oxenfeldt [21], and Lazar Volin [35, p. 306].

<sup>2</sup> Fluctuations in the Soviet Union are discussed by Naum Jasny [10, pp. 11-13], G. W. Nutter [18, pp. 204, 221], and Eugene Zaleski [36, pp. 264-83].

I. *Scope and Methods*

We define as planned economies those whose means of production are owned predominantly by the State and whose economic activities are directed pursuant to an over-all economic plan. The countries for which information is readily available belong to the European Communist bloc: Bulgaria, Czechoslovakia, East Germany, Hungary, Poland, Rumania, the Soviet Union, and Yugoslavia.<sup>3</sup> The free-market countries are the members of the Organization for Economic Co-operation and Development: Austria, Belgium, Canada, Denmark, France, the Federal Republic of Germany, Greece, Iceland, Ireland, Italy, Luxemburg, the Netherlands, Norway, Portugal, Sweden, Turkey, the United Kingdom, and the United States.<sup>4</sup> Neither group is homogeneous. In each there prevail significant differences in per capita income. The approaches to problems of integral planning vary from country to country within the Communist bloc as much as the degree of government intervention varies within the free-market group. Yet a sharp distinction can be drawn between the two on the basis of the institutional framework within which their economies operate, and meaningful comparisons can be made.

The investigation covers 11 years, 1950-60. The recovery years immediately following World War II are excluded as unreflective of normal "peacetime" differences in the performance of the two systems. By 1950 outputs in practically all the countries studied had either reached or surpassed the prewar level. And the basic institutional changes in the Communist bloc had been completed; all these economies were operating on long-term plans.

Variations in volume of four output aggregates are investigated: total output, output in agriculture, in industry (composed of mining, manufacturing, and utilities), and in construction. Since there is no unique way to measure these fluctuations, several methods were tested. They all led to essentially the same results. The results of two methods are presented.<sup>5</sup> The first measure is the standard error of the least-squares fit of the series  $Y_t/Y_{t-1}$  to time, where  $Y$  stands for the value of the variable in constant monetary units and  $t$  stands for time; it expresses the fluctuations in percentage points measured from the trend-line fit to yearly growth rates. The second measure is the antilog of the square root of logarithmic variance of the series  $Y_t/Y_{t-1}$ , with unity subtracted from the antilog.<sup>6</sup>

<sup>3</sup> The first seven countries form the COMECON. No data were available on Albania.

<sup>4</sup> Adequate data were not available for Spain and Switzerland.

<sup>5</sup> Other measures of fluctuations tested were: (1) Pearson's coefficient of variation; (2) the standard error of the least-squares fit to the index of growth with 1950 as the base year; and (3) the standard error of the least-squares fit to the logarithms of index of growth with 1950 as the base year.

<sup>6</sup> For a discussion of the second method, see [5, pp. 23-24]. The formula is:

$$\text{Fluctuation} = \text{antilog} \sqrt{\frac{\sum \left[ \log (Y_t/Y_{t-1}) - \frac{1}{N} \sum \log (Y_t/Y_{t-1}) \right]^2}{N}} - 1,$$

where  $Y_t$  stands for the value of the variable in constant monetary units,  $t$  stands for time, and  $N$  for the number of years minus one.

Data for planned economies were obtained from official statistics released by the central statistical offices of each country. Practically all information on free-market economies was obtained from OECD publications.<sup>7</sup> Two objections might be raised to these data. The first is that the official statistics for growth of output in planned economies suffer from an upward bias. This bias in the trend should not, however, distort the variations around the trend.

The second, more troublesome, objection is that the aggregates studied are not strictly comparable. Most data for all four aggregates in the free-market group refer to gross domestic product at factor cost.<sup>8</sup> For the planned economies, data on total output refer to income originating in material production exclusive of depreciation. In an attempt to make the two sets of data more nearly comparable,<sup>9</sup> two alternative series were used for the free-market economies. First, the part of domestic product originating in the services sectors was omitted.<sup>10</sup> This adjustment probably overcompensates for exclusion of services in planned economies. As Alec Nove points out [16, pp. 253-54], national income of planned economies is built up from the production side by subtraction of material expenditures from total value of their output. Consequently, services rendered to productive enterprises find their way into the "material" national income. Therefore, calculations were also made for the free economies using the official concept of domestic product at factor cost. Lack of data makes impossible any adjustment for depreciation charges, which are excluded from total output in planned but not in free-market statistics. This shortcoming should not, however, greatly affect the comparability of fluctuations. Depreciation charges are small compared to national income in the planned economies,<sup>11</sup> and consequently their exclusion could not increase significantly the measured degree of instability even if we make the extreme assumption that their growth rate was constant.

Unfortunately, no adjustments are possible to make the concepts of agricultural, industrial, and construction outputs more nearly comparable. The boundaries of productive activity in these sectors are not identical in the two groups of economies.<sup>12</sup> More importantly, the data for these three sectors in

<sup>7</sup> Data for the United States are from the Department of Commerce.

<sup>8</sup> In the case of Ireland, Luxemburg, the Netherlands, Portugal, Sweden, and the United States, outputs of the respective industries are represented by the official industrial production indexes; output of construction is represented for each country by the "residential construction" and "other construction" components of the gross domestic fixed capital formation.

<sup>9</sup> For a discussion of an attempt by the Russian economist, Kolganov, to adjust the United States national income to Soviet definition, see [17].

<sup>10</sup> The following sectors compose the gross domestic material product at factor cost: agriculture, forestry, and fishing; mining and quarrying; manufacturing; electricity, gas and water works; construction; transportation and communication; wholesale and retail trade. Excluded are: banking, insurance, and real estate; ownership of dwellings; public administration and defense; health and education services; and miscellaneous services [19, XXXVII].

<sup>11</sup> In Poland, for example, depreciation amounted to about 8 per cent of national income [8, p. 58].

<sup>12</sup> For example, logging and fisheries are included in industry in the Soviet Union. In

planned economies are the familiar gross values of output, which include large amounts of double-counting.<sup>13</sup> Only for three of the eight planned economies are data on income originating in these three sectors available. The income-originating aggregates exhibit larger fluctuations than the gross value aggregates in all but one of the nine cases.<sup>14</sup> Since the concept of income originating is closer to gross product at factor cost, used in the free-market economies, it is likely that the subsequent use of gross value of output data for the planned group somewhat underestimates the degree of their fluctuations.

The limitations of the available information should not be overemphasized. More uniform and detailed data<sup>15</sup> would of course make the comparisons more precise. I am convinced, however, that the conclusions presented in the next section would not be altered.

## II. *The Results*

Table I presents the fluctuations for the individual countries composing the two groups. There is considerable variation in degree of fluctuation both within and between the groups. The path of growth was quite smooth in some countries (Poland, the Soviet Union; France, Italy, Norway), while in other countries it fluctuated widely (Bulgaria, Yugoslavia; Greece, Turkey, the United States). Considering both groups together, the lowest fluctuation in total output occurred in the Soviet Union and in Norway, the highest in Yugoslavia and, to a lesser degree, in Turkey. The output of agriculture fluctuated little in Poland, Czechoslovakia, Luxemburg, Sweden, and the United States; widely in Yugoslavia and Canada. Industrial output of the Soviet Union grew more smoothly than that of any other country considered; the United States registered the highest fluctuations. The growth of construction was smooth in the Soviet Union, in France, and in the United Kingdom; it varied greatly in Bulgaria, Rumania, and Turkey.

The performance of the Soviet Union is of particular interest not only for the obvious reason that it is the most powerful nation of the Communist bloc, but also because it is the one with most experience in economic planning. Except for agriculture, it experienced the lowest fluctuations among the planned economies. Its performance, however, seems to have been duplicated by free-market countries. It was either matched or surpassed by France, Italy, Norway, and Sweden in total output; by Austria, Denmark, France, the Federal Republic of Germany, Luxemburg, Sweden, the United States, and the United Kingdom in agriculture; and by Denmark, France, Italy, and the United Kingdom in construction. Only in industry—the show-window of its

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Czechoslovakia agricultural output does not include forestry but does include fisheries. Yugoslavia publishes separate series for these three sectors. Definitional differences would of course be important if we attempted to measure, for example, the relative levels of output in the two systems. These differences should not have much influence on fluctuations over time.

<sup>13</sup> Net production constituted in Poland less than one-half of gross production in agriculture, about one-third in industry, and about one-half in construction [8, pp. 58-59].

<sup>14</sup> Fluctuations are about equal in the net and gross output of Czechoslovak industry.

<sup>15</sup> Data for some aggregates are published as three-digit numbers only.

economic growth—does the Soviet Union demonstrate a greater stability in growth than the free-market economies, being followed by France, Sweden, and Turkey.

The results for the two groups are summarized in Table II, which compares their mean fluctuations. Three pairs of averages have been calculated. First, the averages for the eight planned and the 18 free-market economies. Second, since fluctuations in total output might depend on the level of economic development (with agricultural output comprising a large and widely fluctuating share), only those OECD countries are considered which had in the mid-fifties per capita incomes similar to those of the Communist bloc.<sup>18</sup> Finally, four planned economies of Central Europe (Czechoslovakia, East Germany, Hungary, and Poland), which had previously belonged to the economic community of Western Europe but which introduced economic planning after World War II, are compared with the Common Market countries.

In all these comparisons the indicated mean fluctuations were higher in the Communist bloc than the OECD economies for all four aggregates. Only in industry is the mean for the planned economies close to the average for free-market countries.

### III. *Comments*

The findings of this study—that the planned economies of the Communist bloc were subject to fluctuations in economic activity equal to or greater than those experienced by the free-market economies of the OECD—raise four questions. First, was there a difference between the two blocs in the extent to which fluctuations took the form of mere changes in positive growth rather than absolute decline when compared to the preceding year; if a decline, how long did it last? Second, assuming that the eight planned economies considered here in some way represent all planned economies and the 18 free-market economies represent all free-market economies, what statistical significance can be attached to the differences of means in the two groups? Third, may the larger fluctuations of the planned economies have been merely the price paid for an allegedly more rapid increase in output? Fourth, what influence does the particular time period studied have on the results obtained?

1. The percentage of absolute declines in all year-to-year changes amounted in the planned economies to 10.1 for total output, 30.4 for agriculture, 2.5 for industry, and 8.7 for construction. The corresponding figures for the free-market economies are 5.6 for total output, 27.9 for agriculture, 9.5 for industry, and 23.2 for construction. Thus, the free-market economies had fewer interruptions in positive growth of total output and, by a slight margin,

*(See next page for Tables I and II)*

<sup>18</sup> These countries are Austria, the Federal Republic of Germany, Greece, Ireland, Italy, the Netherlands, Portugal, and Turkey. For the planned economies, the range of per capita income is \$177 in Yugoslavia and \$500 in Czechoslovakia. For the free-market economies, the range is \$219 in Turkey and \$508 in the Federal Republic of Germany [27, pp. 69-73].

TABLE 1.—FLUCTUATIONS IN OUTPUTS: PLANNED AND FREE-MARKET ECONOMIES, 1950-60<sup>a</sup>

Country	Total Output <sup>b</sup>		Agriculture		Industry		Construction	
	Method 1	Method 2	Method 1	Method 2	Method 1	Method 2	Method 1	Method 2
Bulgaria	10.4	8.8	18.4	16.6	4.0	3.2	18.5	16.7
Czechoslovakia	2.4	2.0	4.6	4.2	3.5	2.9	6.2	5.6
East Germany <sup>d</sup>	4.6	3.1	7.5	6.4	4.6	4.7	11.2	9.2
Hungary <sup>e</sup>	7.2	6.2	14.1	13.8	6.1	5.7	16.1 <sup>f</sup>	14.8 <sup>f</sup>
Poland	2.2	2.1	4.0	4.0	2.8	3.9	9.1	9.4
Rumania	11.6	10.2	17.6	16.8	4.8	4.5	17.4	15.4
Soviet Union	1.9	1.9	6.3	5.7	1.4	1.8	4.7	3.9
Yugoslavia	11.8	11.0	25.6	24.7	6.2	7.2	— <sup>g</sup>	— <sup>g</sup>
Austria	4.0 (4.1)	3.4 (3.5)	6.3	5.8	4.5	3.9	10.5	9.4
Belgium	2.3 (3.1)	2.0 (2.7)	6.6	6.1	4.3	3.8	6.9	6.8
Canada <sup>h</sup>	3.8 (—) <sup>e</sup>	3.6 (—) <sup>e</sup>	20.2	20.4	4.1	3.8	7.7	7.3
Denmark	2.2 (2.3)	2.3 (2.7)	6.4	5.8	3.3	3.9	4.9	4.5
France	1.9 (2.4)	1.6 (2.1)	6.0	5.7	2.9	2.5	4.4	3.9
Germany (F.R.)	2.3 (2.7)	2.2 (2.5)	4.7	4.1	3.4	3.4	6.1	5.5
Greece	4.3 (5.8)	3.7 (5.1)	11.0	10.5	5.0	4.3	12.8	13.3
Iceland	5.3 (—) <sup>e</sup>	5.1 (—) <sup>e</sup>	— <sup>g</sup>	— <sup>g</sup>	— <sup>g</sup>	— <sup>g</sup>	— <sup>g</sup>	— <sup>g</sup>
Ireland <sup>h</sup>	2.5 (—) <sup>e</sup>	2.3 (—) <sup>e</sup>	9.0	8.0	4.3	3.9	10.2	10.2
Italy	2.1 (2.6)	1.8 (2.2)	6.7	6.1	4.0	3.3	4.6	7.5
Luxemburg	2.0 (4.8)	1.7 (4.1)	2.7	2.4	6.5	5.7	12.7	10.4
Netherlands <sup>i</sup>	3.4 (5.0)	3.0 (4.3)	10.3	9.3	4.7	4.3	10.1	8.6
Norway	1.8 (—) <sup>e</sup>	1.6 (—) <sup>e</sup>	6.7	6.0	3.3	2.9	5.4	5.0
Portugal	2.0 (2.2)	1.7 (2.6)	7.0	6.5	3.0	2.9	8.2	7.5
Sweden <sup>h</sup>	1.9 (—) <sup>e</sup>	1.8 (—) <sup>e</sup>	4.3	3.9	2.4	2.3	7.2	6.3
Turkey	6.3 (7.5)	6.1 (7.3)	10.5	10.4	2.3	2.7	17.1	16.8
United Kingdom	2.0 (2.7)	1.9 (2.5)	2.7	2.5	3.6	3.3	3.7	3.5
United States	3.4 (4.7)	3.1 (4.3)	4.5	4.0	7.5	6.8	5.1	4.5

<sup>a</sup> Decimal moved two places to the right for both methods.

<sup>b</sup> Numbers in parentheses are for the domestic "material" product at factor cost.

<sup>c</sup> Data not available.

<sup>d</sup> Data for East Germany are not available in constant DM. Stolper's figures covering the period 1950-58, expressed in constant 1950 prices, show the following fluctuations (Method 1): income, 2.1; agriculture, 13.2; industry, 3.2; and construction, 14.8 [26, p. 418].

<sup>e</sup> The year 1956 was omitted from the calculation to reduce the effect the Hungarian Revolution had on the path of growth. Should this year be included, the fluctuations in the four aggregates are as follows (Method 1): income, 10.8; agriculture, 14.3; industry, 9.4; and construction, 15.3.

<sup>f</sup> Gross value data in constant florints are not available; data on income originating in construction were used.

<sup>g</sup> Gross value data are not available. Hours worked in construction show the following fluctuation: 16.0 (Method 1), and 11.7 (Method 2).

<sup>h</sup> The period for agricultural output is 1952/53-1960/61.

<sup>i</sup> The period covered is 1952-59.

*Sources:* Planned economies: Bulgaria [22, pp. 159-60] [28, pp. 96, 100, 170, 246]; Czechoslovakia [33, p. 37] [34, pp. 38, 157, 211, 230]; East Germany [25, pp. 176-78]; Hungary [13, pp. 43, 65, 133]; Poland [8, pp. 56, 81, 146, 172]; Rumania [7, pp. 94, 118, 259, 268]; Soviet Union [29, p. 152] [30, pp. 171, 292, 551]; Yugoslavia [24, p. 98]. OECD countries: [6, p. 291] [19, pp. XXXVII-LII, p. 2] [20, p. 3] [31, p. 32] [32, pp. 127, 226].

TABLE II—MEAN FLUCTUATIONS IN OUTPUTS: PLANNED AND FREE-MARKET ECONOMIES, 1950-60

Countries	Total Output <sup>a</sup>		Agriculture		Industry		Construction	
	Method 1	Method 2	Method 1	Method 2	Method 1	Method 2	Method 1	Method 2
Planned, all	6.5	5.7	12.3	11.5	4.2	4.2	11.9	10.7
Free market, all	3.0 (3.8)	2.7 (3.6)	7.4	6.9	4.1	3.7	8.1	7.7
Planned, all	6.5	5.7	12.3	11.5	4.2	4.2	11.9	10.7
Free market, middle income group <sup>b</sup>	3.4 (4.3)	3.0 (3.9)	8.2	7.6	3.9	3.6	9.9	9.8
Planned, Central Europe <sup>c</sup>	4.1	3.4	7.5	7.1	4.2	4.3	10.6	9.7
Free market, EEC <sup>d</sup>	2.3 (3.4)	2.1 (3.0)	6.2	5.6	4.3	3.9	7.5	7.1

<sup>a</sup> Numbers in parentheses are for the domestic "material" product at factor cost.

<sup>b</sup> Austria, the Federal Republic of Germany, Greece, Ireland, Italy, the Netherlands, Portugal, and Turkey; these Western countries were classified by Studenski [27, pp. 67-70] together with the Communist bloc countries in the Middle-Income Group.

<sup>c</sup> Czechoslovakia, East Germany, Hungary, and Poland.

<sup>d</sup> Belgium, the Federal Republic of Germany, France, Italy, Luxembourg, and the Netherlands.

*Sources:* Table I.

of agriculture, while planned economies showed fewer lapses from positive growth in industry and construction. Typically, all decreases lasted for only one year in both groups, though in several free-market countries agriculture and construction outputs declined in two consecutive years.

2. If the fluctuations experienced by the two groups are normally distributed and independent, we can test for statistical inference our finding that the mean fluctuations of each aggregate were higher for the first than the second group. The results of the test are shown in Table III. The variances of total outputs in the two groups are too different to permit the *t*-test.<sup>17</sup> For the other three aggregates, the mean fluctuation is significantly higher in planned economies in agriculture and construction, but not in industry.

3. The degree of association between fluctuations and growth rates can be expressed by the correlation coefficient between the two variables. Considering both groups together, the coefficient for total output is  $+0.15$  and is not significant. A significant relationship exists in agriculture where the coefficient is  $+0.53$  and in construction where it is  $+0.45$ ; the first one is significant at 1 per cent level, the second one at 5 per cent level. In industry, a surprisingly negative (though not significant) relationship exists between the two variables; the coefficient is  $-0.25$ . Thus no consistent pattern is apparent between fluctuations and rate of growth.

4. The period under study is the only one for which comparisons between a number of planned and free-market economies can be made. Only the future will show how "unrepresentative" the decade of the 'fifties might have been for the planned economies or for the free-market group. The experiment of integral planning was new for all countries of the Communist bloc except the Soviet Union, and, with the exception of agriculture, the Soviet Union experienced the lowest fluctuations. As more planning expertise is acquired and perhaps more institutional stability achieved, these countries will probably reduce fluctuations in their economic activity. The free-market economies may, on the other hand, find their era of relatively stable growth coming to an end. We can state with reasonable certainty, however, that unless the performance of the free-market countries deteriorates substantially, the planned economies have some considerable way to go before they can substantiate the claim of superiority with respect to fluctuations. Should the bloc countries be unsuccessful in dampening their output fluctuations experienced in the last decade, a "cycle theory" will perhaps have to be advanced for this new type of economic organization. The explanation would presumably run in terms of such factors as rapid shifts in economic policies, changes in

<sup>17</sup> Perhaps it should be pointed out here that a significant value of *t* is not due to the lack of equality of the two variances. The variance for planned economies is larger in all instances, i.e., as far as fluctuations are concerned, there was less homogeneity in planned than free-market group. However, if claims such as "planned economies grow faster" are advanced, the difference in fluctuations variances does not impair the comparability of the two groups. It would be difficult, for example, to accept planning as the cause of more rapid growth but ascribe the fluctuations in growth to another cause.

TABLE III—DIFFERENCE OF MEANS IN FLUCTUATIONS: PLANNED AND FREE-MARKET ECONOMIES, 1950-60

	Total Output		Agriculture		Industry		Construction	
	Method 1	Method 2	Method 1	Method 2	Method 1	Method 2	Method 1	Method 2
Degrees of freedom	(7, 17)	(7, 17)	(7, 16)	(7, 16)	(7, 16)	(7, 16)	(6, 16)	(6, 16)
F-ratio	7.71	9.34	3.62	3.23	1.48	2.46	2.26	2.04
Value of <i>t</i>	3.875	2.925	2.038	1.970	— <sup>b</sup>	— <sup>b</sup>	1.970	1.683
Mean higher in planned economies at the level of	— <sup>a</sup>	— <sup>a</sup>	5%	5%	—	—	5%	10%

<sup>a</sup> Variance cannot be assumed equal in the two groups.<sup>b</sup> Means not different.

Source: Table I.

organization and planning techniques, political unrest, overambitious goals, and planning errors.

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### The Effectiveness of "The American Economy" in Training Secondary School Teachers

As part of a major national effort to aid secondary schools in improving economic instruction at the high school level, the American Economic Association joined with the National Task Force on Economic Education, the Joint Council on Economic Education, and Learning Resources Institute in sponsoring a nationwide television course entitled "The American Economy" during the 1962-63 academic year. Professor John R. Coleman of Carnegie Institute of Technology served as the national teacher for the television course, and a large group of distinguished economists from universities, business, labor, and agriculture served as consultants and visiting lecturers.<sup>1</sup> A major goal of the course was to teach more economics to high school social studies teachers.

"The American Economy" has now completed its run on some 182 CBS television stations and an additional 54 educational and 5 independent television stations throughout the country. The estimated audience of regular viewers for this 160-lesson series (128 lessons on economic content and 32 lessons on teaching methods) was over 1 million persons, including some 5,500 persons who took the course for college credit at one of the 361 colleges and universities across the land which offered credit for "The American Economy."

Apparently most of the 5,500 persons taking the course for credit were school teachers. Since there are presently some 61,000 secondary social studies teachers in the United States, this is just a beginning. But it is a significant beginning. Most educational television stations will run the series again during the 1963-64 academic year, and the entire course of the 160 lessons is now available on movie films that can be shown on any standard 16 mm. sound projector. This makes "The American Economy" available as a basic resource for in-service teacher training programs and summer workshops for secondary school teachers, as well as others, for some years to come.<sup>2</sup>

How effective was the national TV course in teaching high school teachers? The National Opinion Research Center has been engaged to do a thorough evaluation, using a standardized test with appropriate control groups along lines recommended by the National Task Force on Economic Education. This paper reports the results of an earlier, similar, though smaller, study in the Pittsburgh area. These results indicate that the TV course was successful

<sup>1</sup>For a complete listing of the lesson topics and guest lecturers, see [2]. Professor Coleman also discussed the course in [1].

<sup>2</sup>The film series may be used either in its entirety or in one of several alternative analytical subdivisions prepared by Professor Coleman. Details on rental or purchase arrangements for these films can be obtained through the National Education Television Film Service at the Indiana State University Audio-Visual Center, Bloomington, Indiana.

in improving teachers' knowledge of the fundamental economic concepts outlined in the 1961 report of the National Task Force on Economic Education as "the minimum understanding of economics essential for good citizenship and attainable by high school students" [3, p. 4].

The background of the television course in the Pittsburgh area will be presented. The testing instrument and the testing procedure used in the study will be discussed, and the results will be analyzed, using both simple correlation and multiple regression techniques. Some concluding observations and suggestions for further research will complete the paper.

### *I. Background of the TV Course in Pittsburgh*

"The American Economy" was broadcast three times daily over Pittsburgh television stations. Seventy-one high school and junior high school teachers in Allegheny County took the course for graduate credit in education at one or another of the three local Pittsburgh universities—Carnegie Institute of Technology, Duquesne University, or the University of Pittsburgh.<sup>3</sup> Regardless of the university involved, all the students were required to watch the television series thirty minutes each day, Monday through Friday, in addition to meeting at a local university for two hours every other Saturday morning to discuss the economic content lessons with an economics faculty member. The students were divided into three subgroups of approximately 24 persons each for these discussion sessions. Each discussion group met with the same faculty member throughout the entire year.

### *II. The Testing Instrument and the Testing Procedure*

An ideal way to determine the effect of the TV course on these students would have been to administer a standardized test of economic understanding on a before-and-after basis. Unfortunately, such a test was not available at the beginning of the course. During the past year, however, a committee of leading economists, educators, and psychometricians, under the chairmanship of Dr. John M. Stalnaker, President of the National Merit Scholarship Corporation, has been developing a standardized test to measure the basic amount of economic understanding outlined in the National Task Force report cited above.<sup>4</sup> A preliminary form of the Stalnaker Committee's test was available shortly before the completion of the television course, and it was used to measure the economic understanding attained by the TV students and by two other groups selected for "control" purposes.

The test consists of 50 multiple-choice questions, with four possible responses to each question. It has now been administered to some 3,000 high school students and to a smaller number of college economics students and

<sup>3</sup>Thirty-five other persons took "The American Economy" for undergraduate credit at one of the Pittsburgh universities, but these people were not included in the study described in this paper.

<sup>4</sup>The economists on the Stalnaker Committee are G. L. Bach, Carnegie Institute of Technology; Edgar Edwards, Rice University; Joseph Kershaw, RAND and Williams College; Ben Lewis, Oberlin College; and Lewis Wagner, State University of Iowa.

high school teachers. National norms are being established and refinements made so that the test may be available for widespread use during the 1963-64 academic year.<sup>5</sup>

The Stalnaker test was administered to the 71 television students on the next to the last class meeting of the year. The test was also administered to 113 Carnegie Tech sophomore economics students during the last week of their second semester of study; and another group of 73 school teachers, substantially identical to the TV students, but who had not watched "The American Economy," was also selected and tested.

In selecting the teacher control group, one overriding concern was to protect against the possibility that the TV group might be "self-selected" and that they might have a different motivational make-up than another group of randomly selected school teachers. The teacher control group, therefore, was selected in two parts. The first part consisted of another non-TV class in graduate education which met on a night school basis somewhat similar to the TV course. Presumably the 30 persons in this course were motivated to do extra work for graduate credit in education, just as were the TV students. The second part of the control group consisted of 43 Allegheny County school teachers selected at random by the four major school superintendents in the county.

Owing to substantial differences in age, orientation, and training, it was realized that only crude comparisons could be made between the TV students and the college sophomores. In selecting the control group of teachers, however, it was hoped that the influence of the TV course could be identified by using simple correlation and multiple regression analysis to isolate the relationships of several factors which might be presupposed on a priori grounds to have an influence on a respondent's test score.

### III. *The Test Results*

The TV group scored an average of 40.9 correct responses on the 50-item multiple-choice test, with a range of 27 to 50, and a standard deviation of 4.8.

The Carnegie Tech sophomores achieved a mean score of 40.8 correct responses out of 50, with a range of 29 to 48 correct responses, and a standard deviation of 4.2.

The control group scored an average of only 33.9 correct responses on the test, with a range of 21 to 46, and a standard deviation of 5.6.<sup>6</sup>

Thus, those who took either the TV course or the regular college economics course scored substantially higher than the teacher control group. But there was no significant difference between the scores of the teachers who took "The

<sup>5</sup> A revised version of the test is now available through Science Research Associates, 259 East Erie Street, Chicago, Illinois.

<sup>6</sup> There was no significant difference in the mean scores of the two segments of the control group. The graduate-education class obtained a mean score of 32.3 out of 50, and the teachers selected by the county superintendents obtained a mean score of 35.0, but this difference could have occurred by chance three times out of four.

American Economy" for credit and the college sophomores on this very basic test of economic knowledge.<sup>7</sup>

The seven-point difference between the mean scores of the TV students and the teacher control group can be interpreted as a 14 per cent improvement on a 50-item test, or it can be interpreted as a 20.6 per cent improvement over the base of 33.9 scored by the teacher control group, or it can be viewed as an increase of 43.5 per cent of the distance between the control group score and the perfect score of 50. In any case, it would appear that the teachers who took "The American Economy" in the Pittsburgh area knew substantially more economics at the end of the course than did a similar group which did not take the course. The question which remains is how significant is the TV course in explaining this difference when compared to other factors that might have influenced either the TV group or the control group.

#### *IV. Analysis of the Results*

In order to answer this question, data were collected from each respondent in the TV group and from each respondent in the control group on several variables which might be assumed on a priori grounds to have an influence on a respondent's test score. Then a simple correlation and a multiple regression analysis were conducted in an attempt to isolate the influence of the TV course itself. For each respondent, information was obtained on: taking the TV course for credit ( $X_1$ ), age ( $X_2$ ), sex ( $X_3$ ), teaching experience ( $X_4$ ), educational background ( $X_5$ ), and a measure of the respondent's motivation for advanced education ( $X_6$ ). No direct information was available on the very important question of the respondent's native intelligence, but there is no reason to believe that either group was significantly biased one way or the other on this variable. There was also no information available on how conscientiously the TV students had followed the daily broadcasts.

Various measures were obtained for some of the major variables mentioned above, but in these cases only the most significant measures of each variable will be discussed.

Table 1 shows the simple correlation coefficients between the six independent variables and the Stalnaker test scores of all 144 respondents. Completion of the television course was the variable most positively correlated with the test scores, followed by previous work in college economics, teaching a high school social studies course, chronological age, and male sex in descending order. Motivation for recent college work beyond the bachelor's degree was negatively related to the respondents' test scores.

The positive correlation between age and test performance and the negative correlation between the motivation variable of recent college work beyond the bachelor's degree and the test scores are somewhat surprising, since

<sup>7</sup> These results, of course, do not indicate how the two groups might have scored on a more difficult test, nor do they necessarily indicate that the TV course is an adequate substitute for a regular college course. Much more research will have to be done with more sophisticated measuring instruments and more comprehensive study designs before any firm conclusions can be reached on this point.

one might assume that younger teachers and those actively taking advanced work within the past three years might be more "up-to-date" and perhaps have a better knowledge of basic economic ideas. The small size of the correlation coefficients, however, indicates that these relationships are not likely to be of major importance. Positive or negative, coefficients of .14 and  $-.08$  suggest that neither age nor the motivation to take advanced work had much relation to performance on the Stalnaker test.

Going beyond a simple correlation analysis, a multiple regression model of the linear form  $Y = a + b_1 + b_2 + \dots b_n + u$  was used to assess the importance of each of the independent variables in the presence of the other variables. Here,  $Y$  represents the dependent variable of the test score, the  $b$ 's represent the independent variables described in Table 1, and  $u$  represents a random disturbance term assumed to have the usual simplifying properties. Statistically, this analysis helps to determine the weights to be applied to each of the independent variables in a way that eliminates the possible interrelation of the variables considered in the simple correlation analysis.

Table 2 shows the regression coefficients ( $b$ 's) and the " $t$ " statistics for various combinations of independent variables used in the multiple regression model. The per cent of variance "explained" ( $R^2$ ) for each regression is shown in the right-hand column, and the " $p$ " values required for significance at various confidence levels are shown at the bottom of Table 2.

Regressions one through five run the TV variable and each of the other independent variables in relation with the test score. The TV variable ( $X_1$ ) remains highly stable and highly significant in all of these regressions. Male sex ( $X_3$ ), teaching a high school social studies course ( $X_4$ ), and a previous college course in economics ( $X_6$ ) are all significant at the .005 level when considered separately with the TV variable in explaining the test scores.

TABLE 1—SIMPLE CORRELATION COEFFICIENTS BETWEEN VARIOUS RESPONDENT CHARACTERISTICS AND STALNAKER TEST SCORES

Independent Variables	Simple $r$ With Test Score ( $Y$ )
$X_1$ <u>Television Course</u> (Dichotomous Variable: TV=1, No TV=0)	.56
$X_2$ <u>Age</u> (Continuous Variable for Respondent's Chronological Age)	.14
$X_3$ <u>Sex</u> (Dichotomous Variable: Male=1, Female=0)	.13
$X_4$ <u>Teaching Experience</u> (Dichotomous Variable: Teach High School Social Studies=1, Do Not Teach High School Social Studies=0)	.22
$X_5$ <u>Educational Background</u> (Dichotomous Variable: Previous College Economics=1, No Such Economics=0)	.26
$X_6$ <u>Motivation for College Work Beyond Bachelor's Degree</u> (Dichotomous Variable: Work Beyond Bachelor's Degree Since '60=1, No Work Beyond Bachelor's Degree Since '60=0)	$-.08$

TABLE 2—MULTIPLE REGRESSION RESULTS FOR SELECTED REGRESSIONS

Regression Number	Independent Variables						$R^2$
	$X_1$	$X_2$	$X_3$	$X_4$	$X_5$	$X_6$	
$b$	7.12	-0.01					.316
1. $t$	7.79††	-0.24					
$b$	7.28		2.38				.349
2. $t$	8.48††		2.69††				
$b$	6.94			2.56			.354
3. $t$	8.15††			2.89††			
$b$	6.89				3.29		.368
4. $t$	8.19††				3.44††		
$b$	7.08					0.16	.315
5. $t$	7.97††					0.17	
$b$	7.10	-0.03	1.16	1.16	2.54	-0.31	.391
6. $t$	8.07††	-0.58	1.18	1.16	2.34***	-0.32	

$t$  values required for significance at various levels of confidence

$t$ value	confidence level
1.29	.10*
1.66	.05**
1.98	.025***
2.67	.005††

Neither the age variable ( $X_2$ ) nor the motivation variable ( $X_6$ ) is significant in relation with the TV variable, but in this analysis the age variable takes on a very small negative coefficient and the motivation variable takes on a small positive coefficient. This reversal of the signs from those in the preceding simple correlation analysis is more in line with what one might expect; but, again, neither of these variables appears significant.

Regression six runs all of the independent variables in the same equation. The TV variable remains significant at the .005 level, and taking a previous college economics course is significant at the .025 level. None of the other variables is significant in this regression. The  $R^2$  of .391 indicates that the six independent variables considered in this analysis leave a large amount of the variation in the test scores unexplained. The purpose of this paper, however, is not to explain all of the variation. Rather, it is to see if the significance of the TV variable holds up after introducing the other variables.

If data on the respondents' intelligence and conscientiousness in watching

the TV course were available, the  $R^2$  could undoubtedly be increased; but, in the absence of such data, taking the TV course remains by far the most significant variable in explaining the differences in the test scores. The coefficient for this variable also remains highly stable in all of the regressions shown in Table 2, as well as in all the other combinations of these variables not shown in this paper.

Since the test used to measure economic understanding in this experiment was a very simple, objective test, and since some of the limitations of this type of testing are well known, one final observation may be useful in interpreting these results. This is simply the fact that regular-course grade information, based entirely on essay exams, was available for both the TV students and the Carnegie Tech sophomores. The Stalnaker test scores of the Carnegie Tech students had a simple correlation coefficient of .70 with their independently determined course average, and the Stalnaker scores of the TV students had a simple correlation coefficient of .82 with their independently determined course grades.

### *V. Conclusion*

This analysis has shown that the teachers taking "The American Economy" television course for graduate credit in education in the Pittsburgh area scored substantially higher on a standardized test of elementary economic understanding than did a similar group of teachers who did not take the television course. In explaining the difference in test scores, the TV course itself proved to be by far the most significant of the six major variables considered. The next most important variable in explaining the difference was previous college work in economics.

How much of the superior performance of the TV students can be attributed to the TV broadcasts themselves and how much attributed to the biweekly follow-up sessions cannot be estimated from these Pittsburgh results, but the forthcoming nationwide study by the National Opinion Research Center will investigate this problem directly.

Pending the results from this more comprehensive study, one may tentatively conclude from the results examined in this paper that the nationwide broadcast of "The American Economy" did contribute significantly to its main purpose of increasing secondary school teachers' grasp of the minimum level of economic understanding outlined in the 1961 report of the National Task Force on Economic Education. No evaluation of the long-range effectiveness of this course is possible, however, until more research is done on what, if any, use is made of this knowledge by the teachers in their own classrooms. With a large number of teachers having recently completed the television course, the opportunity for launching serious research in this area by professional economists willing to cooperate with these people has been increased substantially.

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### A Controlled Evaluation of "The American Economy"

During the 1962-63 academic year 27 students at the University of Nebraska took their principles of economics through the CBS-TV college credit course, "The American Economy."<sup>1</sup> These students viewed the half-hour television lectures four days per week, each lecture being followed by 10-15 minutes of questions and answers or elaborating comments by the professor supervising the television course. Students in the television group were all full-time university students rather than public school teachers or casual adult viewers. The lecture group with which the television group was compared was a quite sizable class of 107 students whose program of instruction consisted primarily of three 50-minute lectures per week. Both groups—the television group and the lecture group—used the same textbook and the study guide which accompanies it. The television group also used the television study guide, *The American Economy* [2], while the lecture group did not.

This over-all arrangement afforded a unique opportunity to evaluate through a controlled comparison the relative performance of the television group and the lecture group. The general dearth of scientific investigation in the teaching of economics [3] and the recent interest in economic education seemed compelling reasons for undertaking such an evaluation.

#### I. Testing Techniques and Procedures

The television group and the lecture group were given two multiple-choice examinations totaling 170 questions. The first examination of 120 questions, which we shall refer to hereafter as the Course Examination, was prepared by the authors of this paper. The questions on this examination were of three quite distinct types: factual, conceptual, and problem-analytical. The 20 factual questions were of a "basic data" character: At what average annual rate has the U. S. gross national product increased over a period of years? What is the single most important source of revenue and type of expenditure of the federal government? What is the most important cause of the public debt? Sixty questions were designed to evaluate the student's ability to define or identify basic economic concepts, e.g., the marginal propensity to consume, marginal cost, the multiplier, the short run, price elasticity of de-

<sup>1</sup>The underlying assumptions and character of this course have been summarized by the national teacher. See John R. Coleman [1].

mand, the law of increasing costs. The remaining 40 questions were problems or analytical questions wherein, for example, students were required to interpret graphs, calculate and analyze cost and demand data, compute the elasticity coefficient from demand data, and determine the gains from trade and the terms of trade from comparative cost data. All of the 120 questions were based upon textbook material which was common to both the television and lecture groups.

A brief discussion of the rationale for identifying and employing three types of questions in the Course Examination is in order. Inasmuch as the television course was designed primarily to further general economic understanding rather than to prepare students for more advanced work in economics,<sup>2</sup> it was hypothesized that the lecture group would perform relatively better than the television group on questions that required the mastery of technical economic concepts and their application to the solution of numerical and graphical problems.

The second examination given to the two groups was the Test of Economic Understanding prepared by a distinguished group of economists and educators and published by the Joint Council on Economic Education. It is to be noted that this 50 multiple-choice question examination was designed primarily for high school students who might or might not have had formal coursework in economics. In other words, this examination was not designed to measure achievement after a student had taken a specific course or read certain materials. While the questions contained therein are a mixture of the three types used in the Course Examination, the Test of Economic Understanding is undoubtedly less discriminating at the college level.

Because the cumulative grade averages of the television group and the lecture group were substantially different,<sup>3</sup> the authors decided to make a matched-pairs comparison. Twenty-seven of the 107 students comprising the lecture group were matched to the 27 television students on the basis of three variables: cumulative grade average, number of credit hours of college work completed, and college affiliation (course of study). The mean cumulative grade averages for the matched groups were identical (4.8). The average number of credit hours completed per student was very similar—61.8 hours in the television group and 60.4 hours in the lecture group. College affiliations, while not identical, varied little: each group contained 15 business administration students; the television group contained three engineering students and the lecture group four; etc.

## II. Test Results and Their Significance

The results of the matched-pairs comparison are summarized in Table 1. The television group scored very slightly higher on the Test of Economic

<sup>2</sup>The television lectures on price theory, for example, made no reference to marginal cost and marginal revenue in the determination of output adjustments under different market conditions.

<sup>3</sup>Based on a scale where 9 is the highest and 1 is the lowest grade, the lecture group was found to have a 5.4 cumulative grade average as compared to 4.8 for the television group.

Understanding and the 20 factual questions than did the lecture group. On the other hand, the lecture group performed substantially better on the 60 conceptual and 40 problem-analytical questions than did the television group.

Computation of "*t*" values revealed that the mean differences on the conceptual questions were statistically significant at the 5 per cent level, while on the problem-analytical questions the difference was significant at the 0.1 per cent level. The mean differences on the factual questions and the Test of Economic Understanding were not statistically significant.

A non-parametric test of significance, namely, the Wilcoxon matched-pairs signed-rank test, was also applied to the same data. The results of this second test confirm in each case the findings of the "*t*" test.

### III. Conclusions and Qualifications

What do these comparisons prove? Two conclusions seem warranted upon the basis of this particular evaluation.

1. The performance of the two groups on the Test of Economic Understanding and the factual data component of the Course Examination suggests that "The American Economy" television course was as successful as the lecture course in conveying a broad, general understanding of economics.

2. The lecture group seems to have achieved a higher level of understanding of specific economic concepts and of analytical tools than did the television group. Stated differently, if a firm grasp of economic concepts and the ability to apply the analytical tools of economics to problems and issues are held to be basic goals of the college principles course, "The American Economy" may not have been a perfect substitute for the typical college course. It is relevant to note at this juncture, however, that this second conclusion tends to impose an objective on "The American Economy" that is not entirely consistent with the goals set forth by the national teacher. Although the television course was "assumed to be the equivalent of one year's

TABLE 1—MEAN DIFFERENCES ON EXAMINATIONS AND THEIR STATISTICAL SIGNIFICANCE

Examination	Lecture Group Mean	Television Group Mean	Differences of Means	Computed " <i>t</i> "	Interpretation, " <i>t</i> " test	Probabilities (one-tail), Wilcoxon Test
Test of Economic Understanding (50 questions)	40.0	40.9	-0.9	-.8967	Not significant	Not significant
Factual (20 questions)	13.5	13.6	-0.1	-.1936	Not significant	Not significant
Conceptual (60 questions)	42.0	38.8	3.2	2.1309	> <i>t</i> at .05	Significant at .05 level
Problem-Analytical (40 questions)	23.7	17.4	6.3	4.3404	> <i>t</i> at .001	Significant at .005 level

work in economics for nonspecialists at a quality college . . . [and covered] the same broad ground found in most of the widely selling economics textbooks," the course was designed primarily for the secondary school social studies teacher and was intended "to go just beyond that minimum degree of economic literacy described in the National Task Force report" [1, p. 646]. In other words, this second conclusion is in no way inconsistent with the proposition that "The American Economy" was highly successful in fulfilling those objectives it set out to achieve.

There are several grounds upon which these conclusions and their significance might be challenged. It is perhaps useful to anticipate and evaluate these possible criticisms [4].

1. A first potential criticism is that the experiment does not embrace certain variables that might reasonably be expected to have a bearing upon the performance of the groups compared. There are two defenses for this criticism which, in our view, substantially diminish its relevance. First, as previously noted, the matched-pairs comparison took cumulative grade average, credit hours completed, and college affiliation into account. While other variables, e.g., outside employment, sex, family background, and type of student housing, might be argued to have some bearing upon the performance of the two groups, it can be countered that the cumulative grade average used in the comparisons can reasonably be expected to reflect these other variables. Second, if these allegedly neglected variables are of crucial importance, the relative homogeneity of the television and lecture groups would seem to constitute a *prima facie* case that such variables would influence the performances of both groups in the same direction.

2. A second criticism might be that this experiment has validity only within the confines of the University of Nebraska. However, the students at Nebraska are probably typical of students in the elementary economics course at a large number of American colleges and universities. Furthermore, there was nothing atypical about the control (lecture) group, and the course was quite standard as to subject matter, type of presentation, textbook and supplementary materials, etc. The class size (107) is undoubtedly less atypical than would have been the case a few years ago. In short, there is reason to believe that a similar experiment at any one of a large number of other institutions would have yielded similar results.

3. Objective examinations were employed in this comparison for two reasons: (a) to avoid the subjective element involved in the scoring of non-objective examinations and (b) to facilitate the sampling of student understanding of a wider range of subject matter. It can be argued that many basic educational goals may be ignored in the measurement of students' grasp of factual information and basic concepts and their ability to manipulate analytical tools through multiple-choice questions. It is true, of course, that the over-all "effectiveness" of any economics course is undoubtedly multidimensional. Which of the two groups acquired a more permanent interest in economics? How have the attitudes of the two groups toward value (policy) positions which they do not share been affected? With what impressions of

the importance of the social sciences as an area of scholarly endeavor have the two groups emerged from their respective courses? The present study admittedly does not shed light on these questions. But lacking empirical evidence, we see no *prima facie* reasons why either of the courses here compared would be of superior effectiveness in achieving these more nebulous goals. It is perhaps relevant here to recall Professor Coleman's comment that "We will have wasted an opportunity and precious resources if we let this [television] series pass without systematic study of its accomplishments and shortcomings" [1, p. 647]. We envision the present report to be only one aspect of this over-all evaluation.

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#### The Brown Shoe Case and the New Antimerger Policy: Comment

I should like to offer several comments on the recent Brown Shoe decision [1] which David Martin ably analyzed in the June 1963 issue of this *Review* [3].

1. The Brown decision ostensibly settles one of the thorniest issues in antitrust law: the issue of market definition in merger cases. In holding that relevant submarkets, product and geographic, may exist within the boundaries of the broad market, the Court apparently ruled that the market may be drawn more narrowly in a merger case than in a monopoly case. Unfortunately, the decision is not altogether clear on this point. After stating that submarkets are germane in a Section 7 case, the Court proceeded to argue that the relevant market must be drawn broadly enough to recognize competition where it exists and to correspond to economic reality. Obviously, a market drawn to include competitive products and suppliers traces out the true boundaries of a market, regardless of whether it is termed a broad market or a submarket.

Martin argues, however, that in the Brown case the Court did not accept the Sherman Act guidelines to market definition in merger cases. Since this is

a common interpretation, I shall address my comments to the question of whether the definition of the market should vary with the violation and anti-trust statute. Is it tenable, as several commentators have argued, to exclude substitute suppliers in merger cases under the Clayton Act, even though we include them in monopoly cases under the Sherman Act [7] [4] [2]?

The assumption underlying the submarket approach in merger cases is that the submarket is meaningful in itself, i.e., that the submarket marks off an important area of competition. The difficulty with this assumption is that one cannot determine whether this is true without examining the broader market, the market comprising those firms selling substitute products to a common group of buyers.<sup>1</sup> It is within the broad market, not the submarket, that the stuff of price determination and competitive rivalry is found. If the market definition in a monopoly case excludes some of the firms within this market, we cannot determine whether a defendant has insignificant or substantial monopoly power. If the market definition in merger cases excludes some substitute products or if the market is defined so narrowly that it includes only those firms which happen to be selling in a given area, we cannot reasonably know whether the merger will damage competition; for the submarket approach will restrict us to an examination of only a segment of the market and only a part of the forces which determine price and firm behavioral patterns. The merger of two companies which retail low-priced shoes in the same urban neighborhoods may or may not affect industry leadership, alter price, change competitive tactics, inconvenience buyers or suppliers, or trigger other mergers. And there is no way of knowing such things if we restrict ourselves to a product and geographic submarket.

Rightly or wrongly, the courts have sometimes taken a quantitative approach to merger legality, concentrating entirely on market-share statistics. Even on these terms, the market should not be defined differently in merger than in monopoly cases. Suppose there are two sets of firms, that each set has three firms, that each firm is equal in size, and that the products of set A are competitive with, but differentiated from, the products of set B. If one of the firms in set A acquires the other two firms in its set, the government could challenge the acquisition under either Section 2 of the Sherman Act or Section 7 of the Clayton Act. If substitute products are included, as they presumably would be in a monopoly-power case, the firm has achieved a 50 per cent share of the market; if substitute products are excluded, as the Brown decision implied they could be in a Clayton Act proceeding, the firm has achieved complete monopoly control. But obviously the probable damage to competition is the same regardless of how we define the market. Surely we do not multiply the actual or potential damage of the merger by choosing to prosecute under one law rather than another. Alternatively, if one of the firms in set A acquires two of the firms in set B (rather than set A), the government would not characterize the merger as conglomerate, realizing that this

<sup>1</sup>The broad market may, of course, exclude certain goods which are extremely poor substitutes because of quality or price differences. And some sellers of the relevant product may not be in the relevant geographic market because of transportation costs.

would weaken its case under the Clayton Act and destroy it under the Sherman Act. But this implies that the merger is significant in terms of actual or potential competitive impact precisely because the products of sets A and B are substitutes and in the same market.

Martin has argued that the judiciary has accepted the submarket approach to market definition in order to tighten the merger law. But this can be done, as the Brown case demonstrates, by lowering the size of the market share vulnerable under the law. There is, then, no need to narrow the market definition and artificially inflate the relative size of the merger. Nor is it a matter of indifference as to which way we turn the screw: the attempt to tighten the law by means of a submarket approach to market definition might have some unfortunate side effects.

A submarket approach in merger cases is not likely to increase the clarity, consistency, or acceptability of the law. What criteria are to be used in identifying a submarket? The six criteria enumerated by the Court in the Brown case are the same criteria which establish or identify the broad market. Some courts will accept at face value the market definition offered by the government, provided there is some supporting evidence; other courts, however, will demand that the relevant market must be drawn realistically, and the Brown decision will be cited as authority. Thus the definition of the market and the criteria used to identify it will vary from case to case, lending support to the argument that the antitrust laws are too capricious to be workable. And this argument will be bolstered by the allegation that the competitive impact of a merger is sometimes evaluated in a gerrymandered market. It is preferable, in this writer's opinion, to face the difficulties involved in identifying the true boundaries of the market than to use a submarket approach and run the risk of undermining the strong case for strict antimerger enforcement.

2. In finding the vertical aspect of the Brown-Kinney merger illegal, the Court considered three factors: the foreclosure effect, industry trends, and the competitive strength of the combination. Each of these three points will be briefly commented upon.

The Court did not attempt to analyze the competitive impact of the vertical merger at the manufacturing level; instead, it assumed as an article of faith that the permanent foreclosure of a market share which came to only 1.2 per cent of national sales would be inherently damaging to competition. Justice Harlan, in one of the separate opinions, expressed an opposite fear: that some of the independent retail stores would be prevented by the merger from dealing in Brown shoes. The possibility that the foreclosed manufacturers could sell to the foreclosed retailers was apparently not given any weight by the Court. The failure of the Court to go beyond the bare fact of market foreclosure testifies again to a point once made by Professor Mason: that the legal concept of monopoly emphasizes market exclusion [5]. It should come as no surprise, then, if small merger foreclosures are equated with monopolistic tendencies.

In judging the legality of the vertical merger at the manufacturing level, the

Court also considered the alleged industry trend toward concentration. It is clear that competition at some level may be threatened if only a few firms integrate into backward or forward markets, especially if these firms possess large shares at one of the industry levels. But it is not clear that a multifirm vertical integration movement will damage competition at any level: if 100 shoe manufacturers acquire all of the retail outlets, competition will not be restricted at either the manufacturing or the retailing level [6, p. 183]. Though the evidence in the *Brown* case did show that the number of manufacturers declined in the period 1947-54, it cannot be assumed that this was due, entirely or in large part, to the vertical merger movement in the industry; moreover, Justice Harlan admitted that the 4-firm, 8-firm, and 15-firm concentration ratios at the manufacturing level actually declined from 1947 to 1955.<sup>2</sup> Instead of arguing that the vertical merger movement in the industry threatens competition, the Court could just as easily have concluded that the vertical integration movement in the industry has worked to the disadvantage of the largest firms.

The Court also held that the vertical merger threatened competition at the retailing level. Justice Warren pointed out that a chain of vertically integrated outlets has lower costs than independent retailers because the wholesaler is eliminated and the chain units can purchase in large quantities. Justice Harlan argued that a vertically integrated chain can sell at a lower profit. Martin pointed out that these cost advantages were not given much weight by the Court. But the important point is that the Court failed to consider the attempt to lower costs as a legitimate purpose for merging; in fact the economic advantages of this particular merger were regarded by the Court as a threat to retail competition. With this stroke, the Court may have succeeded in closing the door on vertical mergers; for even if the market foreclosure is quite small, the merger can be condemned because of the adverse competitive effects at the forward market.

Martin concluded that the *Brown* decision does not make vertical mergers per se illegal but it "puts much less burden on the government than before" [3, p. 353]. If this view is correct (and this writer thinks it an understatement), it is important that we understand the costs of a tough law on vertical mergers. If there are genuine economies to be gained by vertical integration, business firms are under strong pressures to take advantage of them. This can be accomplished by the building of new assets or the acquisition of existing assets. In either case, the nonintegrated firms will eventually fall by the wayside. The pains of economic adjustment, however, may be more severe if these firms are gradually eliminated from the industry by the competitive pressures generated by new and more efficient capacity; for the sale of distressed assets to buyers inside or outside the industry is likely to be relatively disadvantageous

<sup>2</sup> According to the Bureau of Census studies of the 4-digit footwear industry, the 4-firm and 8-firm concentration ratios rose slightly from 1947 to 1954, while the 20-firm ratio remained constant. All three ratios were a shade lower in 1958 than in 1947. See [8, p. 27].

to the selling firms. A strict ban on vertical mergers may succeed only in increasing the social disutility of industrial reorganization without the compensating advantage of encouraging firms to achieve the most efficient structure.

If there is merit in the argument that mergers represent an efficient means of permitting firms to expand in order to achieve economies and a relatively painless way for inefficient firms to exit from the industry, then the new law on vertical mergers stands in need of modification. Provided the firm does not possess or is not acquiring a substantial market share, it should have the right to a defense that the merger will demonstrably lower costs. Such a modification of the law would retain for society the benefits of the merger device, without subverting society's efforts to prevent competitive damaging mergers; and it would take the sting out of the charge that the purpose of the Clayton Act is to protect producers from hard competition.

3. A close reading of the Court's approach to horizontal mergers in the Brown case suggests that the Supreme Court views a substantial increase in concentration, brought about by a merger involving a substantial share, as tantamount to a lessening of competition or tendency toward monopoly. Perhaps this is what Martin meant in arguing that under the Brown ruling horizontal mergers are to be governed by the incipency doctrine, whereas prior to 1950 they were governed by the more stringent Sherman Act test of illegality.

That an increase in industry concentration (rather than damage to competition) has become the gravamen of Section 7 is clear from a number of points in the Brown decision. The Court eschewed an analysis of market structure and conduct, viewed the instant merger against the backdrop of industry concentration trends, held (in another section of the decision) that the competitive vigor remaining after a merger is not relevant if oligopoly trends are present, argued that market shares as low as 5 per cent—at least in a fragmented industry—are substantial shares, and justified its over-all approach to merger legality on the ground that Congress had given a mandate to prevent tendencies toward concentration in their incipency. More significantly, the Court pointed out several times that there are three types of mergers which Congress wished to immunize from the law: mergers involving *de minimus* shares, failing companies, and small firms seeking to compete more effectively with the industry leaders. Presumably, all other mergers are illegal. Since it would be difficult to demonstrate on a priori grounds that all other mergers might damage competition, it is a fair inference that the focus of the law has been shifted from damage to competition to increases in concentration.

The above interpretation of the Brown decision forces me to question Martin's observations that market-share information will play a subsidiary role in future merger cases, that future courts may permit a merger involving a market share of, say, 5 per cent or more if industry competitors possess a larger share, and that the Brown decision confirms a rule of reason approach to merger legality. By emphasizing concentration, in the Brown case, the Court has pulled the rug from under the Pillsbury rule-of-reason doctrine and

replaced it with an approach that centers on market statistics, past and present.<sup>8</sup> If future courts scrupulously follow Brown, the frequency of the small-merger exemption may be small: even small mergers are now vulnerable under the law if industry trends are present, and it would take a most unimaginative antitrust prosecutor who could not find evidence of some kind of trend in the history of an industry.

In conclusion, the Brown decision crystallizes a question which long has plagued students of antitrust: how reliable are our predictive (analytical) judgments as to the probable competitive impact of a merger involving market shares of from, say, 2 to 20 per cent in industries having varying degrees of seller concentration, product differentiation, and entry barriers, as well as unpredictable firm behavioral patterns? My suspicion is that the variety of professional judgments concerning the impact of any given merger would be embarrassingly large. If I read Brown correctly, the Court has brushed aside the intricacies of the competitive impact question and has come down on the side of those who urge that antimerger policy should stress large numbers of seller alternatives and low seller concentration. And certainly this was what Congress had in mind in amending the Clayton Act in 1950. But even those who sympathize with this objective may be dismayed in the Brown ruling by the Court's confusing submarket approach to market definition and its overt hostility toward vertical merger. If the new look in antimerger policy is to have staying power, it must have a defensible economic underpinning.

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<sup>8</sup> The above conclusion should be qualified. The Court held that the competitive strength of the Brown-Kinney combine might threaten the survival of nonintegrated firms. If this "soft competition" approach to mergers becomes precedent, then market statistics can be dispensed with altogether.

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### The Brown Shoe Case and the New Antimerger Policy: Reply

I wish to reply to the first of the three points made by Professor Jones because his argument raises some fundamental questions about the scope and nature of economic theory and its usefulness in and relevance to the development of antitrust policy.

Jones argues that the Supreme Court's acceptance of the submarket approach in merger cases is undesirable, and he advocates the use of the same market concept in Clayton Act cases as in Sherman Act cases. But is it not quite consistent with acceptable economic analysis to use one market concept in evaluating the degree of competition or monopoly that has existed in the past and another market concept in forecasting the probable effect of a merger or acquisition on competition in the future? In the latter case one must not only draw inferences from incomplete information about what existed in the past but must also forecast future market boundaries. Even if unique, well-defined market boundaries existed in the past, we cannot know with certainty what they will be in the future. Fortunately, the Clayton Act standard is based on reasonable probabilities of future circumstances—market boundaries as well as market shares.

Aside from the distinctions between the nature of monopolization and Section 7 cases, however, I question Jones's basic assumption that "a market drawn to include competitive products and suppliers traces out the true boundaries of a market." In his first footnote Jones implicitly recognizes that substitutability is a matter of degree. He is willing to exclude from the broad market "extremely poor substitutes." But just what constitutes "extremely poor" is the question that gave rise to the submarket approach in the first place. The courts cannot relegate their criteria to an assumption in a footnote; they must reach findings of fact and conclusions of law relating to the question. Rather than forcing market realities into a dichotomy of substitutes on the one hand and extremely poor substitutes and nonsubstitutes on the other, the Supreme Court has adopted a concept that abstracts from reality to a lesser degree. In short, the Court quite properly rejected the notion that a unique boundary exists.

If the science of economics is to make a contribution to decision-making, whether private or public, economists must, it seems to me, avoid acting as if reality conforms to the simplifying assumptions of our models. I do not deny the usefulness of a theoretical model based on the assumption that there exists a "market comprising those firms selling substitute products to a common group of buyers." But that it exists in the shoe business or the electrical conductor wire and cable business is a fact that must be proven in court with legally acceptable evidence.

As Ross M. Robertson pointed out so clearly a decade ago,<sup>1</sup> the boundaries that Jones assumes to exist may not exist at all. Each good manufactured and offered for sale competes for buyers' dollars and, therefore, has substitutes.

<sup>1</sup> Ross M. Robertson, "On the Changing Apparatus of Competition," *Am. Econ. Rev.*, May 1954, 44, 51-62.

The substitutes have substitutes. The spatial aspect of competition alone gives rise to a problem of drawing a line around the firms that have a common group of customers. Unless Jones has made a value judgment on what constitutes "extremely poor" substitutability, his notion of the "broad market" is much broader than "flexible wrapping materials." It may even include the whole of economic activity in all the world. If it is less broad, it is because someone has made a value judgment and drawn a line. Whether drawn narrowly or broadly, can such a line be called "the true boundary"?

The recognition of the pervasiveness of substitutability and competition may lead to the conclusion that there is no monopoly problem. Obviously, no firm has the power to raise its prices beyond some upper limit without losing all its customers. But antitrust policy is concerned with protecting the public from *unlawful* restraints and monopolies. To catch violations in their incipency Congress has chosen to make unlawful all corporate acquisitions for which the reasonably probable effect is a substantial lessening of competition. In economic language we can state the criterion in these terms: Is it reasonably probable that the future effect of the acquisition will be to give the combined firm power to raise the price somewhat? That is, is there likely to be a limited range within which the price can be increased above the competitive level before the competition of substitutes reduces sales sufficiently that the firm finds it undesirable to raise prices further? A reduction of the number of firms selling a particular item may result in such power to raise prices somewhat. A merger between producers of substitutes may also reduce the danger of loss of sales to a particular substitute. If either possibility exists, the law is applicable. The relevant market in the former case is contained within the market relevant in the latter case.

Mr. Jones agrees with the desirability of tightening the merger law. He prefers that the government's burden be reduced by lowering the market share accepted as unlawful. Let us consider his example of six firms of equal size, three of which produce product A and three of which produce a substitute product B. Jones asks, if one of the firms acquires two others, what happens to its market share? His answer is that it has gone from one-sixth to one-half, irrespective of which two firms it acquires, and that this result is what the Court should examine in ascertaining the probable effect on competition in the market. I say that it makes a great deal of difference which firms have been acquired. If one accepts the market for products A and B together as one relevant market and also accepts the two submarkets as two additional relevant markets, then the effect of the merger on the market structure in all three markets depends on which firms were acquired. In Jones's first example—in which an A producer acquires the two other A producers—the acquiring firm's share of the A submarket goes from one-third to one, its share of the A plus B market goes from one-sixth to one-half, and its share of the B submarket remains at zero. In Jones's second case—in which an A firm acquires two B firms—the acquiring firm's share of the A submarket remains at one-third, its share of the broad market goes from one-sixth to one-half, and its share of the B submarket goes from zero to two-thirds. Is not all of this information relevant? Why throw it

away? Obviously, whether the probable impact will be to lessen competition substantially depends in part on how much the price of A can be varied before B is substituted for it, and vice versa. The probable impact also depends on how much the prices of A and B can be raised without some other substitute coming into consideration. Monopoly power in law as well as in economics is defined in terms of power to enhance price (not to infinity) and exclude entry. Owning potential entrants tends to give such power.

Perhaps the economic reasoning of the Court in *Brown Shoe* was really more sophisticated economically than Jones realized.

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### On a Debt-Income Model of Consumer Instalment Credit Growth: Comment

The purpose of this note is to argue that the growth of the ratio of instalment debt outstanding to income is not adequately explained by Alain Enthoven in his article in the December 1957 issue of this *Review*.<sup>1</sup> His life-cycle model in Section I is not under discussion here, and its validity is unaffected by the following arguments. It is here contended that his debt-income model is technically misleading, if not positively erroneous. His assumptions are so strong that further analysis of their consequences reveals that, instead of growing to an asymptote, the debt-income ratio is constant (or becomes so as soon as the assumptions of the model have taken effect, which will happen within about three years at most).

Enthoven assumes that borrowings in each year are a constant proportion of income, that repayment conditions are constant, and that income increases by a fixed proportion each year. Under these conditions it may be seen that (a) total debt at the end of each year is the sum of those fixed proportions of borrowings in that and earlier years which are still outstanding (since repayment conditions are constant). Therefore (b) total debt is the sum of fixed proportions of income over the past few years (since borrowings are proportionate to incomes). Therefore (c) total debt bears a fixed ratio to income in the given year (since the income in each previous year is a fixed proportion of income in the given year). Hence Enthoven's conclusion that the debt-income ratio tends to a limit is correct only in a trivial sense; in fact it always equals the "limit." If the borrowing-income ratio is not equal to its constant value before year 0, say, perhaps because instalment credit was only then legalized, the debt-income ratio does not reach its constant value until year  $l$ , where  $l$  is the number of years for which contracts run. But it clearly must be constant from then onwards since the borrowing, if any, up to year 0 is irrelevant to total debt once it has all been

<sup>1</sup> Alain Enthoven, "The Growth of Instalment Credit and the Future of Prosperity," *Am. Econ. Rev.*, Dec. 1957, 47, 913-29.

repaid. The following paragraph summarizes Enthoven's mathematical formulation and explains how the confusion arises.

Enthoven writes

$$(1) \quad Y(t) = Y(0)(1+r)^t,$$

where  $Y$  denotes income,  $r$  its constant relative rate of growth, and  $t$  the number of years which have elapsed since the base year. Then if  $B(t)$  and  $R(t)$  are new borrowing and repayments in year  $t$ ,

$$(2) \quad B(t) = bY(t)$$

$$(3) \quad R(t) = L[B(t-1), \dots, B(t-n)],$$

where  $b$  is constant and  $L$  represents some linear combination. By definition,

$$(4) \quad D(t) - D(t-1) = B(t) - R(t).$$

Using (2) and (3) in (4)

$$(5) \quad D(t) - D(t-1) = bY(t) - L[bY(t), \dots, bY(t-n)].$$

For the sake of definiteness Enthoven now assumes that repayments are complete by the end of the third year, so (5) gives

$$\begin{aligned} D(t) - D(t-1) &= a_1Y(t) + a_2Y(t-1) + a_3Y(t-2) \\ &= aY(t) \end{aligned}$$

by using (1). Hence, summing,

$$\begin{aligned} D(t) &= \sum_{n=1}^t aY(n) + D(0) \\ &= \frac{a(1+r)}{r} Y(0)[(1+r)^t - 1] + D(0). \end{aligned}$$

Hence follows his basic conclusion:

$$(6) \quad \frac{D(t)}{Y(t)} = \frac{a(1+r)}{r} - \left[ \frac{a(1+r)}{r} - \frac{D(0)}{Y(0)} \right] (1+r)^{-t},$$

and so appears to tend to  $a(1+r)/r$  as  $t$  tends to infinity.

The basic cause of this confusion is that the repayment conditions are not specified, so that the constants in (6) are not so general as they appear. Suppose that of every dollar borrowed in year  $t$ ,  $p_0$  is outstanding at the end of the year,  $p_1$  at the end of year  $(t+1)$ , and by the definiteness assumption, none at the end of year  $(t+2)$ . Then

$$\begin{aligned} D(t) &= p_0B(t) + p_1B(t-1) \\ &= Y(t) \left[ p_0b + \frac{p_1b}{(1+r)} \right] \\ (7) \quad &= cY(t), \end{aligned}$$

giving a fixed debt-income ratio of  $c$ , as argued verbally above. Then

$$\begin{aligned} D(t) - D(t-1) &= cY(t) - cY(t-1) \\ &= aY(t) \end{aligned}$$

as above, where  $a = rc/(1+r)$ . This may be obtained directly, as follows:

$$\begin{aligned} D(t) - D(t-1) &= B(t) - R(t) \\ &= bY(t) - (1-p_0)bY(t) - (p_0-p_1)bY(t-1) \\ &\quad - p_1bY(t-2) \\ &= Y(t) \left[ p_0b + \frac{p_1b}{(1+r)} \right] \frac{r}{1+r} \\ &= \frac{cr}{1+r} Y(t). \end{aligned}$$

Equations (6) and (7) are consistent, since  $D(0)/Y(0) = c = a(1+r)/r$ , so that the square bracket in (6) is zero and  $D(t)/Y(t)$  is equal to  $c$ .

The observed growth in the debt-income ratio, which continued strongly until the late 1950's at least, can only be explained by a breakdown in one or more of Enthoven's assumptions; in particular, the constancy of the borrowing-income ratio expressed in (2) is not found empirically before the mid-1950's, and even now the ratio shows some signs of further growth.

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### On a Debt-Income Model of Consumer Instalment Credit Growth: Reply

John and Ursula Hicks had a favorite saying for it: "Don't throw out the baby with the bath water." Mr. Oliver has found a little bath water, but he is wrong in suggesting that he has thrown out my baby. His point is correct, as far as it goes, but it is a minor technical point that does not affect the validity of my interpretation of the postwar growth of consumer instalment credit [3]. Although his note is useful, he has not established that my "debt-income model is technically misleading, if not positively erroneous."

As Mr. Oliver indicates, I did assume that income in the year  $t$ ,  $Y(t)$ , grows at a constant annual percentage rate,  $r$ , i.e.,

$$(1) \quad Y(t) = Y(0)(1+r)^t;$$

that new borrowing in the year  $t$ ,  $B(t)$ , is proportional to income in the same year, i.e.,

$$(2) \quad B(t) = bY(t);$$

and that repayments in the year  $t$ ,  $R(t)$ , are a linear combination of borrowings in the previous  $n$  years, i.e.,

$$(3) \quad R(t) = L[B(t-1), \dots, B(t-n)].$$

And, as Mr. Oliver indicates, these assumptions do imply that the debt-income ratio must become constant by the  $n$ th year. But assumptions (2) and (3) were made gratuitously, to provide a logical basis for the equation

$$(4) \quad D(t) - D(t-1) = aY(t),$$

which states that "the absolute increase in the stock of debt outstanding each year is proportional to a variable which grows at the same rate as income" [3, p. 923]. As shown in my article, equations (1), (2), and (3) together imply (4). But the reverse is not true. That is, (4) can be valid even under conditions in which (2) and (3) are not satisfied. Moreover, my model rested entirely on equations (1) and (4). Equations (2) and (3) did not reappear once they had served the purpose of providing a basis for (4). Therefore, since (2) and (3) are unnecessary, and have the overly strong and unwanted implications pointed out by Mr. Oliver, I shall apply Occam's Razor and abandon them. The basic assumptions of the model then are equations (1) and (4), and the whole analysis that follows them stands unharmed. This model, incidentally, is essentially the same as models used by Gurley and Shaw [4] and Domar [2] in discussions of the growth of other kinds of debt.<sup>1</sup>

How good is the assumption expressed by equation (4)? As I showed in 1957, equations (1) and (4) imply:

$$(5) \quad D(t) = \frac{a(1+r)}{r} Y(0)[(1+r)^t - 1] + D(0)$$

$$(6) \quad \frac{D(t)}{Y(t)} = \frac{a(1+r)}{r} - \left[ \frac{a(1+r)}{r} - \frac{D(0)}{Y(0)} \right] (1+r)^{-t}$$

$$(7) \quad \lim_{t \rightarrow \infty} \frac{D(t)}{Y(t)} = \frac{a(1+r)}{r}.$$

Fitting (1) to personal income for the years 1945 and 1963, one obtains an annual average growth rate of about 5.7 per cent. Fitting (5) to personal income and consumer instalment credit outstanding for the same years, one obtains a value of  $a$  of .0094.<sup>2</sup>

<sup>1</sup> If I were now able to rewrite the article in the light of Oliver's criticism, I would merely delete the lines between the equations numbered (3) and (6) on page 923—corresponding to equations (1) and (4) here—together with the attached footnote, and replace them by the words "Second, we assume"; and also delete the references to the assumptions expressed here by equations (2) and (3) earlier in the paper, while leaving everything that follows unchanged.

<sup>2</sup> The data for these and all other calculations in this discussion are taken from [6]. The income data are personal income from Table C-14, p. 224. The instalment credit data are from Table C-49, p. 267. The borrowings and repayments are from Table C-50, p. 268. The 1963 data are Council of Economic Advisers' estimates. All other income data are from the Department of Commerce. All other data on instalment credit are from the Board of Governors of the Federal Reserve System.

One finds a good deal of cyclical fluctuation if one calculates the actual ratio of  $D(t) - D(t-1)$  to  $Y(t)$ , i.e.,  $a$ , for each year in the postwar period. Although the three-year averages shown in Table I suggest that  $a$  has declined some in the past six years, the 1962 and 1963 values were above the average for the period as a whole. Considering the fact that the  $D(t) - D(t-1)$  series is the difference between two relatively large variables, one of which is subject to quite large cyclical variations, the parameter  $a$  appears to have been reasonably stable.

As I showed in the 1957 article and intend to show again later in this note, the model based on equations (1) and (4) does provide a good description of what actually has been taking place; that is, an asymptotic convergence of the debt-income ratio. However, the implications correctly

TABLE I—AVERAGE VALUES OF THE RATIOS OF CHANGE IN DEBT  
TO INCOME AND BORROWING TO INCOME\*

Years	$a = \frac{D(t) - D(t-1)}{Y(t)}$	$b = \frac{B(t)}{Y(t)}$
1945-63	.0091	—
1946-48	.0112	.063
1949-51	.0096	.091
1952-54	.0098	.108
1955-57	.0106	.122
1958-60	.0077	.121
1961-1963	.0081	.125
1962	.0102	.125
1963	.0122	.131

\* These are simple unweighted arithmetic averages.

drawn by Mr. Oliver from equations (1), (2), and (3) are quite clearly inconsistent with the data; since 1945, the debt-income ratio has increased steadily and has not become constant. Therefore, either or both the hypotheses represented by (2) and (3) must be abandoned. Mr. Oliver has made a useful contribution in pointing this out. But what is to replace them? This would be an interesting research project for someone with more time than I have. I must limit myself to a brief consideration of a few possibilities.

First, what is the evidence on equation (2)? As Mr. Oliver points out, the borrowing-income ratio is growing. In fact, using personal income and instalment credit extended, the ratio has more than doubled over the period under investigation, although it has changed little since 1955. This is shown in Table I. It reflects the removal of restrictions on this kind of borrowing in 1946 and 1947, the increasing number of people using instalment credit, and the penetration of instalment credit into new areas. It may also reflect what is, in effect, refinancing of existing debt by further use of instalment credit.

Second, what is the evidence on equation (3)? My present circumstances do not permit the detailed sort of econometric investigation one would like to do. For example, one should attempt to fit a different function to each of the major types of instalment credit (automobile paper, other consumer goods paper, repair and modernization loans, and personal loans). However, one rough check shows that total repayments on all instalment credit each year from 1948 to 1963 have stayed very close to the average of borrowing in that year and the two preceding years. The variations about this average do not show any discernable trend.<sup>3</sup> This suggests that there hasn't been any strong change in the pattern of repayments, but confirmation of this conjecture would require more detailed analysis.

I have, for several years, wanted to go back and examine the post-1956 data to see whether the pattern of growth of consumer instalment debt has been consistent with my model. I am grateful to Mr. Oliver for providing the occasion for doing this. In making such an examination, it is important to bear in mind that the purpose of the model was not to predict income or the debt-income ratio. It is not entirely clear that one can expect reliably accurate prediction from any econometric model; surely not from a simple two-parameter model in a complex multivariate situation. The purpose of the model was to show the logical compatibility of a growth rate of instalment debt substantially in excess of the growth rate of income with the continued steady growth of income. Moreover, after allowing for cyclical fluctuations, the growth of the debt-income ratio between 1945 and 1956 did display the pattern of asymptotic convergence predicted by the model.

Between 1956 and 1963, the average annual growth rate of income has been lower than in the preceding eleven years. Fitting  $(1+r)^t$  to the end points, one obtains about 6.2 per cent for 1945-56 and about 4.8 per cent for 1956-63. If one had had to make predictions in 1956 of the debt-income ratio in the early 1960's, one possible procedure would have been to use the value of  $a$  derived from the 1945-56 period and to make conditional predictions based on alternative assumptions about the rate of growth of income. The trouble with this is that  $a$  is not necessarily independent of  $r$ . In fact, in my original derivation of  $a$ , the one attacked by Mr. Oliver,  $a$  and  $r$  were positively related, so that a decline in  $r$ , *ceteris paribus*, would entail a decline in  $a$ . Mr. Oliver has forced me, at least temporarily, to be agnostic about the relationship of  $a$  and  $r$ . But I neither need to assume nor would I feel justified in assuming that they are unrelated, especially in view of the fact that  $a$  also fell after 1956.

An alternative procedure would be simply to extrapolate from 1956 onward using the model with parameters derived from the 1945-56 data, and to compare the extrapolated debt-income ratios with the actual ratios. This comparison is shown in Table II. The fit is not too bad when one considers the simplicity of the model. For example, by 1963 the extrapolated

<sup>3</sup> The average ratio of  $R(t)$  to  $.33[B(t) + B(t-1) + B(t-2)]$  in the years 1948 to 1963 is 1.008. With the exception of two years in which the ratio reaches 1.083 and 1.089, the greatest deviation from the average is less than .03.

debt-income ratio is about 12.5 per cent while the actual ratio reached 11.6 per cent.<sup>4</sup>

Thus, in fact, predictions of the debt-income ratio generated in this way turned out to be high despite the fall in  $r$  which, if  $\alpha$  were constant, would have the effect of increasing the debt-income ratio. The reason for this can be found in the decline of  $\alpha$  after 1956. Nevertheless, Table II shows quite clearly that, allowing for cyclical fluctuations, the debt-income ratio did follow the pattern of asymptotic convergence suggested by the debt-income growth model.

TABLE II—ACTUAL AND EXTRAPOLATED GROWTH OF INCOME  
AND INSTALMENT CREDIT, 1956-1963\*

	Actual	Extrapolated	Actual	Extrapolated	Actual	Extrapolated
	$Y(t)$	$Y(t)$	$D(t)$	$D(t)$	$D(t)$	$D(t)$
	\$10 <sup>3</sup>	\$10 <sup>3</sup>	\$10 <sup>3</sup>	\$10 <sup>3</sup>	$\frac{D(t)}{Y(t)}$	$\frac{D(t)}{Y(t)}$
1945	171.2	171.2	2,462	2,462	.0144	.0144
1946	179.3	181.9	4,172	4,385	.0233	.0241
1947	191.6	193.2	6,695	6,442	.0349	.0333
1948	210.4	205.2	8,996	8,620	.0428	.0420
1949	208.3	218.0	11,590	10,933	.0556	.0502
1950	228.5	231.6	14,703	13,391	.0643	.0578
1951	256.7	246.0	15,294	16,002	.0596	.0650
1952	273.1	261.4	19,403	18,776	.0710	.0718
1953	288.3	277.7	23,005	21,722	.0763	.0782
1954	289.8	295.0	23,568	24,852	.0813	.0842
1955	310.2	313.3	28,906	28,177	.0932	.0899
1956	332.9	332.9	31,720	31,720	.0953	.0953
1957	351.4	353.6	33,867	35,461	.0964	.1003
1958	360.3	375.7	33,642	39,447	.0934	.1050
1959	383.9	399.1	39,245	43,680	.1022	.1094
1960	401.3	423.9	42,832	48,178	.1067	.1137
1961	417.4	450.3	43,527	52,957	.1043	.1176
1962	442.1	478.4	48,034	58,032	.1086	.1213
1963	463.0	508.2	53,675	63,424	.1159	.1248

\* Extrapolation based on debt-income model fitted to 1945 and 1956 data.

In discussing an analysis by Gilbert Burck and Sanford Parker [1], I included the following footnote in the 1957 article [3, p. 927, n. 21]:

Burck and Parker . . . used an incorrect method of extrapolation of the rate of increase of the debt-income ratio: ". . . instalment debt outstanding has grown from 5 per cent of consumers' disposable money

<sup>4</sup> The values for income, debt, and therefore  $\alpha$  and  $r$  used here are slightly different from those shown on page 926, Table III, of my 1957 article. The reason for this is that, in the interim, for reasons best known to the Department of Commerce and Oskar Morgenstern [5], the data for the years 1945-56 have been revised. However, the change is too small to have any noticeable effect on this analysis. I have used the new data to achieve consistency with the subsequent years.

income in 1948 to 10.9 per cent in 1955. If this rate of increase were to continue for another seven years, instalment debt outstanding would have to rise to about 17 per cent of disposable income in 1962." Apparently they extrapolated linearly the change over the previous seven years. If this is what they understand by "the rate of increase of instalment debt," it is not surprising that they predict that it must slow down, for this "rate," if extrapolated, would lead eventually to an infinite ratio of debt to income no matter how small a positive increase took place in the ratio during the base period. In terms of personal income and instalment debt, their figures would be a ratio of debt to income of .0431 in 1948 and a ratio of .0948 in 1955 (see Table II, above), or an increase of .0517. Extrapolating linearly for another 7 years by adding the change over the past 7 to the 1955 ratio, one obtains .1465 as their "prediction" for 1962. The authors make it clear that they do not believe that the economy could support such an in-

TABLE III—LIMITING DEBT-INCOME RATIOS IMPLIED BY  
THE DATA OF SELECTED SETS OF YEARS

Personal Income and Instalment Debt	$a$	$r$	$\lim \frac{D}{Y}$
1945-56 (1957 Data)	.0107	.0602	.188
1945-56 (1964 Data)	.0106	.0623	.181
1956-63 (1964 Data)	.0078	.0482	.169
1945-63 (1964 Data)	.0094	.0568	.176

crease. On the basis of equation (9), the extrapolated debt-income ratio for 1962 is .1239.

In fact, as indicated in Table II, the 1962 ratio turned out to be 10.9 per cent. Thus, my extrapolation was clearly much closer to the mark than the one implied by Burck and Parker's model.

Alternatively we can fit the model to the end points of the 1945-63 period, and compare it with the previous results. This is done in Table III.

These calculations suggest that the 1956-63 experience has been consistent with the conclusions reached on the basis of the 1945-56 data. In particular, the limiting debt-income ratio has remained around 18 per cent. Looking ahead, the rate of convergence can be expected to decline. If personal income continues to grow between now and 1970 at the 1945-63 average rate, and if the forces that have generated the growth of debt in those years continue to operate unaltered (assumptions, not predictions), by the end of 1970, the ratio of consumer instalment debt to personal income should reach about 13.5 per cent.

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**Economic Forecasting When the Subject of the Forecast  
Is Influenced by the Forecast: Correction**

In the September 1963 issue of the *Review*, the first instead of the second version of a communication by Emile Grunberg and Franco Modigliani was mistakenly published, "Economic Forecasting When the Subject of the Forecast Is Influenced by the Forecast: Comment." In his "Reply," Murray C. Kemp replied to the unpublished second version. Fortunately, only a few of his remarks and references require clarification.

In particular, Kemp's two sentences on page 739, lines 8-13, refer to the following statement by Grunberg and Modigliani (which should be substituted for their statement on page 737, lines 9-13):

In short we suggest that, before we can reach sensible conclusions on this complex issue, we need to learn to construct models whose behavior equations are more directly tied to optimizing behavior of the individual units, and which take seriously into account rationality in the formation of expectations.

Further, the paragraph of Kemp's, beginning on page 739, line 25, referred to the following footnote by Grunberg and Modigliani (which should be inserted after the word "possible" in line 14, page 734):

Kemp's use of the terms "accurate" and "perfectly accurate" seems not very felicitous. Our reasons for preferring in our paper the term "correct" is that "accuracy" (and *a fortiori* "perfect accuracy") does not have a very precise meaning. Indeed, "accuracy" is better viewed as a relation than as a property. Surely, perfect accuracy does not exist even in astrophysics. We defined "correct" as what might be termed "acceptably accurate": a prediction where at any time  $t$  the observed value  $x_t$  falls within an interval between  $x^* + n$  and  $x^* - n$ , where  $n$  is an arbitrary variable depending on the purpose of the prediction and thus ultimately subjective.

In addition, the second version contained the following footnote, not referred to by Kemp, which should be inserted after the word "discontinuities," page 736, line 20:

Needless to say, the shape of the dashed portion of the  $R$  function in our graph is purely illustrative. It is conceivable that the  $R$  function might actually reach the upper boundary for some value of  $y^*$  less than the upper bound of  $y$ , and before intersecting the requirement function. But thereafter, since it cannot pierce the upper boundary, it must either run along it or fall again below it. In either case, as long as it is continuous, it must intersect or touch the requirement function at least once, if not earlier then at least when  $y^*$  equals the upper bound of  $y$ . Similar considerations apply, *mutatis mutandis*, to the lower bound.

## BOOK REVIEWS

### General Economics; Methodology

*Die Grundannahmen der Preistheorie. Eine Kritik an den Ausgangssätzen der mikro-ökonomischen Modellbildung.* By GERHARD KADE. Berlin and Frankfurt am Main: Verlag Franz Vahlen, 1962. Pp. 176. DM 18.

This book by Dr. Gerhard Kade is a small volume of 176 tightly printed pages. It is a *Habilitationsschrift* which was published on recommendation of the Faculty of Economics and Social Sciences at the Free University in West Berlin.

The subject matter is neoclassical economic theory, defined as all those economic theories which adopt the market model of perfect competition. This comprises a good part of contemporary economic theory. To get into sharper focus the problems with which the author is concerned, a few preliminary observations may be made on the nature of theories in social sciences in general and economics in particular.

Economics as a social science is an empirical science (*Erfahrungswissenschaft*). Economic events—similar to events in other social or natural sciences—are part of the real world. In an effort to understand these events, theories were developed, at first intuitive ones, later axiomatic ones. It is generally agreed that, once it becomes possible to axiomatize the contents of a field of knowledge—which depends, of course, on how well such a field is studied and understood—it may lend itself to mathematical treatment. If the latter is the case, the possibility exists that, in a deductive way, new and so far undetected relationships may be discovered. This may then lead to considerable new insights as was the case for example in modern physics. However, in any empirical field, the preconditions for a later fruitful mathematical deductive analysis—no matter how refined and powerful it may be—are good axioms or good basic assumptions to start with. These axioms are statements about events in the real world and the better they describe these, the better they are. Here Kade's problem begins.

He scrutinizes carefully what are the underlying axioms of neoclassical price theory, or better, neoclassical decision making. He singles out three fundamental ones which suffice to explain the working of the system as conventionally stated. These are (a) the information axiom, (b) the axiom of rationality, and (c) the axiom of reaction. These axioms, which are basic for the economic content of the neoclassical theory, are found seriously deficient. They do not properly account for what is observed in reality. The reason for this inadequacy is partly owing to the fact that, when these axioms were formulated—at and before the time that economics was mathematized—the relationship between alternative economic choices and resulting decisions was poorly understood. In addition, these axioms include traceable notions of earlier times as to what should be the order of economic things (here the author lists, e.g., apologetic efforts of interested groups), the role of the state in economic

affairs, and an optimum which the system is supposed to grind out. In any case, whatever the deficiencies of the axioms were, they have come down to us and pervade present neoclassical price theory.

What does the author consider the most important deficiencies? We may quickly list them. First, there is the assumption of full information or transparency of all markets. Second, there is the assumption that decision-making units try to maximize economic gains and that doing this will not only lead to an "optimal" solution for each individual but also for society as a whole. Chances are that a "maximum" in a mathematical sense is not a true "economic optimum." Whether it is actually an economic "optimum" depends on the assumed underlying economic behavior. Different assumptions as to economic behavior will lead to different optima. Third, there is the assumption of the existence of smooth and differentiable functions which are difficult to reconcile with known indivisibility of many goods and factors of production. In short, the desire to use an elegant mathematical apparatus (something the author refers to as "*Analogiefreudigkeit*," which means a joy derived from the fact that the same methods of analysis can be applied as in the natural sciences) led often to the uncritical acceptance of simple, but, *helas*, to a large extent irrelevant axioms. Consequently, neoclassical theory, though logically consistent the author argues, is largely empty in an economic sense. An implication is that economic policy decisions based on such a theory must be viewed with utmost suspicion. This seems to be the essence of the book.

It is not necessary to delve further into Kade's objections to neoclassical theory. They are well known, though—this must be admitted—often conveniently ignored. It is unfortunately very often the case nowadays that the elegance of a mathematical model and its logical consistency often seem to be more important than its economic content. Yet one should not be too pessimistic. In time new axioms will be, no doubt, forthcoming and they will better describe what is going on in the real world than those we inherited from the eighteenth and nineteenth centuries. The appearance of game theory may be cited as an example of a possible fruitful breakthrough leading to the creation of new axioms of economic behavior.

The merit of Kade's book is the emphasis which he puts on the sharp distinction between the economic content of an economic model which depends on underlying basic assumptions and its tautological mathematical structure. Concern about these problems is certainly very important, yet in the opinion of this reviewer there is no need for a new *Methodenstreit* about what should rank higher in economics, efforts to improve axioms or developing mathematical deductive methods. Both are essential. If there is at present an imbalance, as the author asserts, one may guess that it is owing to the fact that mathematization of economics may have occurred at too early a stage historically. Contrary to some natural sciences, too little was evidently known about human economic behavior to permit meaningful axiomatization. Whether the use of mathematics in economics has been derogative to efforts to obtain better axioms, and thus has handicapped the development of economic theory, is a very difficult question. This reviewer would be very reluctant to accept the author's final conclusion that this was inevitable and actually so.

Kade wrote a stimulating, if not easy to read, book and, though in the treatment of his subject the diagnosis is much longer than the cure, it should be rewarding to those who are interested in basic problems of economic theory.

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**Price and Allocation Theory; Income and Employment Theory;  
Related Empirical Studies; History of Economic Thought**

*Approaches to Dynamic Investment Planning.* By STEPHEN A. MARGLIN.  
Amsterdam: North-Holland Publishing Company, 1963. Pp. x, 162.  
\$5.00.

This concisely written monograph is concerned with the following problem in the planning of public investment: Suppose that benefits from undertaking a project increase with calendar time. If the cost of construction remains constant (or increases less rapidly than benefits), the social value of the project may vary with the time at which construction begins. The problem for investment planning is to determine the optimal time for undertaking construction of the project.

Marglin begins with the analysis of a simple but elegant formal model. Construction costs occur at a single point in time while benefits occur as a perpetual stream beginning at the time of construction. The value of the rate of benefits is the product of a component depending only on calendar time and a component depending only on time elapsed since construction. The first component may be identified with price (for "small" projects) and the second with quantity of benefits.

The criterion for social value of a project is the present value of net benefits. The discount rate is taken as given. It represents "... the community's *marginal* rate of substitution between present and future consumption ..." (p. 143). In the absence of budget constraints, this criterion is clearly an appropriate one. At the most fundamental level investment decisions represent the selection of alternative paths for consumption; in the case of public investment, selection among projects represents choice among alternative paths for consumption faced by the community.

Marglin contrasts selection of time for construction by maximization of present value with construction of a project whenever its present value becomes non-negative. The latter rule, called by Marglin the "Sophisticated Static Rule," may lead to premature construction of projects. The principal conclusion of this part of the analysis is that "durable investments must be planned within a framework that includes the dynamic question of the choice of construction time as an integral part of the investment decision" (p. 31).

From the initial framework just described, Marglin goes on to consider investment planning subject to budget constraints. In the most detailed model discussed by Marglin there are  $m$  projects and  $T$  construction periods. The present value of benefits for each project is taken to be a function of construction outlays on that project in each of the  $T$  construction periods. The total construction outlays on all projects in a given period must be less than or equal

to the construction budget provided for that period. The budget for each period is taken as fixed in the sense that construction expenditures in a given period may not exceed the budgeted level. No carry-over of funds from one construction period to another is permitted. However, unspent funds are credited to the investment plan at a price which is taken to reflect the social value of these funds in the private sector.

The critical assumption in Marglin's formulation of the problem of investment planning subject to budget constraints is the following: "we simply assume the existence of a set of techniques for finding the optimal dynamic plan for an individual project in isolation, and address ourselves only to the extra problems posed by budget constraints" (p. 73). The techniques to which Marglin refers involve the calculation of the present value of benefits for each project. As in the problem of investment planning without budget constraints, the discount rate is taken as given.

The criterion for the social value of an investment plan is "to find the allocation of the budgets which maximize the sum of project net present values and the net present value of slack [unspent funds] subject to the condition that the sum of project outlays in each period not exceed the period's budget . . ." (p. 101). In computing the value of Marglin's criterion for any particular investment plan, the procedure is to calculate the present value of net benefits for each project; both benefits and construction outlays are discounted at the given social rate of discount. The present values for each of the projects are then added together to obtain a total present value for the investment plan. An optimal plan is one which maximizes total present value for all projects together with the present value of unspent funds subject to the budget constraints.

The crucial assumption implicit in Marglin's procedure is that an appropriate rate of discount for the evaluation of individual projects (or, for that matter, for devising an optimal dynamic plan for an individual project) can be taken as given independently of the construction budgets available in each of the construction periods. There is one set of circumstances in which this assumption is consistent with selection of an optimal path for community consumption. This is that *none* of the budget constraints is "effective," that is, that funds provided by the construction budget for each period are greater than or exactly equal to the construction outlays for that period which would be optimal *in the absence of* budget constraints.

The problem of investment planning without budget constraints is not completely uninteresting. However, for the application which figures most prominently in Marglin's discussion—water-resources planning—the most interesting problems appear to involve budget constraints which are effective. In this case no "set of techniques for finding the optimal dynamic plan for an individual project in isolation" exists.

There is a further problem in the application of a present value criterion like that employed by Marglin even where budget constraints are absent. Except for the trivial and uninteresting situation in which the community's marginal rate of substitution between consumption in any two periods is independent of the levels of consumption in either of the periods (and all

other periods), the assumption of a given rate of discount requires in essence that the optimal solution of the investment problem be known in advance of computation of this solution by maximization of present value.<sup>1</sup> Stated this baldly, the assumption of a given rate of discount is palpably absurd. A procedure like that advocated by Marglin cannot provide an appropriate basis for investment planning in either public or private sectors.

A major portion of Marglin's book is devoted to the consideration of investment plans which are optimal by his criterion. In general, the "optimal" plan corresponds to the solution of a nonlinear-programming problem. Marglin presents what amounts to a survey of methods for computing solutions to concave programming problems as discussed by such authors as Arrow and Hurwicz. In view of the requirement that the optimal solution be known in advance, implicit in the definition of the criterion for optimality, these computational details are of little economic interest.

From the point of view of water-resources planning, Marglin's chief contribution in this monograph is his elegant and lucid analysis of the choice of a time for construction in the absence of budget constraints. Despite the defects of Marglin's approach to investment planning subject to budget constraints and the inadequacy of his proposals for computation of investment plans, the monograph may provide useful insights into the institutional framework within which an economically sound approach to these problems must be formulated.

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<sup>1</sup> This point has been made very forcefully by J. Hirshleifer in his already-classic article, "On the Theory of Optimal Investment Decision," *Jour. of Pol. Econ.*, Aug. 1958, p. 340.

*The Growth Multiplier and a General Theory of Economic Growth.* By VIKAS MISHRA. New York: Asia Publishing House, Taplinger Distributor, 1962. Pp. 164. \$5.00.

If one has recently acquired some fresh ideas about a theory, quite as a matter of course one wants to bring it to the attention of others. A reliable means to this end is to use the expression "A General Theory of . . ." in the title. But this has its liabilities, for even Keynes' *General Theory* turned out to be not quite as "general" as he thought.

The title of the present book is somewhat misleading and pretentious. The book may be regarded basically as a descriptive analysis of what I might call a dynamic input-output model, though the author neither uses the term "input-output" nor gives any mathematical formulation.

The book consists of two parts: Part I deals with the analytical apparatus; Part II takes up economic evolution in the expansive time-horizon. In Part I the author discusses the basic concepts and definitions and presents an analytical apparatus which he uses to discuss the way the process of

economic growth could start and develop within the framework of this system. An attempt is made to incorporate in a single formulation the explanations of fluctuations in economic activity, the transition of an underdeveloped subsistence economy to a growing economy and sustained growth equilibrium. In Part II he shows to what extent the analytical apparatus can be applied to explain both the present and future evolution of an economic society. The central concept the author uses in his formulation of growth theory is the growth multiplier.

The growth multiplier is defined as "a moving summation process of the various investment multipliers in an economy where an investment multiplier is conceived of as an investment *potential* function of a specific investment *current*" (p. 100). By investment current, Vikas Mishra means autonomous investment and by investment potential, induced investment. Thus the investment multiplier is the ratio of induced investment to autonomous investment. But it is *not* the Kahn-Keynes type of the investment multiplier ( $dY/dI$ ). In his formulation the total investment potential can be divided into the direct investment effect arising from the Hirschman type of linkage effect and also into the indirect investment effect arising from the familiar multiplier and accelerator interaction. A set of such investment multipliers—under various conditions, with varying volumes, spreads, and intensities—will establish an investment multiplier complex which, in the process of interaction with the economic activity complex, brings about the growth multiplier. The initial investment current may start from any sector, but how crucial a sector is must be evaluated in terms of the proportionate magnitude of its contribution to the total national product and also of the relatively large prospects of technological change (or innovation) in that sector. In terms of the transition to the phase of self-sustained growth, the entry of the crucial sector into the orbit of interaction process is a precondition for the take-off stage in economic growth.

The process of economic growth is viewed as comprising two distinct phases, that is in his terms "Release" (Take-Off) and "Equilibrium" (Sustained Growth). The Release phase is a sharp rise in the rate of investment current and also investment potential, whereas the Equilibrium phase treats them as relatively stable. For him, Cyclical disequilibrium and Trend disequilibrium can be explained via the direct and indirect investment effects.

To this reviewer it is not entirely clear what is meant by the growth multiplier. Since Mishra defines the growth multiplier as a moving summation, it seems to me that it may be considered as a summation of "dynamic multipliers" of all sectors. At any rate the present book is too general to draw any specific conclusions about the way an economy can develop and achieve balanced growth. My admiration of this work would have been greater if the author had presented his model of economic growth as a system of equations, at least in the footnotes or in the appendix.

RYUZO SATO

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*Keynesianism—Retrospect and Prospect: A Critical Restatement of Basic Economic Principles.* By W. H. HUTT. Chicago: Henry Regnary Company. Pp. xi, 447. \$7.50.

In this book Professor Hutt endeavors comprehensively to examine and "refute" the Keynesian system. After briefly stating the Keynesian thesis and its consequences, as he understands them, he begins with several valuable chapters on the nature of money and the functioning of the pricing system. In Chapter 8, however, a highly individual definition of income is introduced, and for the remainder of the book Hutt's criticism, much of which is valid and valuable, is needlessly obscured by an almost Keynesian determination to make the reader learn a new language, which is about 50 per cent tautology. In this review I shall concentrate upon the substance of Hutt's argument, as I understand it.

What is Hutt really shooting at? He takes as his basic target (p. 33) a quotation from Harrod: "If a certain level of interest is established which is inconsistent with full activity, no flexibility or mobility in other parts of the system will get the system to move to full activity." In other words, Keynesian unemployment equilibrium does *not* depend on sticky prices and wages. This proposition of Harrod's (in its nondefinitional sense) Hutt has no difficulty in demolishing in his Chapter 9. He *seems* to use the standard refutations of the "wealth" effect, and repercussions upon the marginal efficiency of capital, pointed out by Haberler, and in my *Keynesian System*, and, as far as that goes, by Keynes himself in Chapter 19 of the *General Theory*, but the most original part of his emphasis is to point out that *relative* price changes, by indicating and releasing new wants and new investment outlets, will favorably affect the MEC schedule. As in my *Keynesian System*, though not as carefully or explicitly, he uses spontaneous shifts in wants and methods to question the idea of any necessary capital glut or state of "full investment." He shares my belief that any actual state of capital "glut" is the product of an unfriendly institutional environment, not physical law.

It would appear from all this that Hutt is a hard-shell, sound-money, and price-flexibility man who would never increase  $M$  or certainly not  $MV$ . But somehow this is not altogether the case. In an aside (pp. 125-26) reminiscent of some of Keynes' qualifications, he seems to admit that an expansion of  $MV$  to "remedy a *purposeless* deflation" is all right, and that deflations have sometimes been "misconceived or ill advised." So we do have to exercise some practical judgment after all! This should have induced more charity toward the "right wing" Keynesians among whom he numbers the reviewer but they come in for special virulence. In this connection I should explain that by the "undogmatic disciples of Keynes" (p. 435), whom he says I neglect to mention, I meant Gottfried Haberler and John Jewkes. I can see no reason why he should quote Haberler with approval throughout, while discharging considerable indirect artillery in my direction, when Haberler's position and mine, as he must know, are practically identical. If this be "Keynesianism," I must plead guilty, of course. But let me pass on to such differences of substance as I think still exist between Hutt's views and mine.

Both Hutt and I, as I see it, believe that economic progress is essentially open-ended. But we both also believe that, through the inevitable discontinuities of a continual advance into novelty, fluctuations sometimes occur. Hutt, *most* of the time, talks as if he thought price and interest-rate flexibility *alone* could always contain these fluctuations within a socially tolerable degree of fluctuation. Now with this position I disagree, and I refuse to rule out entirely the use, in appropriate circumstances, of deficit finance. Hutt merely *asserts* that such a policy must always lead to inflation. He does not prove it. Just because easy money is not the cure-all that the streamlined Keynesians now teach it to be, does not mean that it is always bad. Indeed we have seen that even Hutt weakens a bit in his all-out dogmatism. Unemployment, like fever, is a product of many different forces, and because a medicine does not work in one case does not prove it is always useless.

But deficit finance and easy money are certainly, today, products that have been oversold. I am distressed that Hutt should have couched his, frequently, most valuable deflation of their claims in language so vague and so intemperate as to lose him much of his audience. He frequently misstates Keynes, and only restrictions of space keep me from stating some of his errors. But Hutt may have something on his side. When someone writes a careful, temperate criticism of Keynes, like my *Keynesian System*, it often gets shrugged off—unread—as “saying nothing new.” Maybe the criticism has to be shrill to be heard! If so, this is a deep reflection upon current U.S. scholarship.

DAVID MCCORD WRIGHT

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*Price Theory and Its Uses.* By DONALD S. WATSON. Boston: Houghton Mifflin Co., 1963. Pp. xiii, 431. \$6.50.

In *Price Theory and Its Uses*, Donald S. Watson organizes his material in a customary way. The theory of demand comes first, with chapters on the demand concept, price elasticity, neoclassical utility, modern utility, indifference curves, and selected extensions (income elasticity, cross-elasticity, durables, expectations, market structures). The theory of the firm follows, with chapters about profit maximization, production functions, optimal combinations, cost functions, and linear programming. Competitive pricing comes next, with chapters about supply curves, short-run equilibrium, long-run equilibrium, general equilibrium, policy implications, and disequilibrium. Monopoly pricing follows, with chapters headed monopoly price and price discrimination. Next is imperfect competition, divided into monopolistic competition, oligopoly models, oligopoly pricing, and nonprice competition. Watson concludes with incomes as prices, devoting one chapter to factor pricing and one to wages.

How do you judge a theory text? Substantively, I hope you want it to accomplish the following, among other things.

It presents the intersection of demand and supply, not as a deterministic outcome, but as an estimator of the expectations of quantity, average price,

successful traders, and gains from trade—and as an estimator that, in the two tests so far made, has proved biased and subject to large error even with atomism. It recognizes that these expectations may be affected by search and transaction costs and by the transaction system (auction, etc.). It sees that a testable theory of value would predict which system arises where and produce a probability function for quantity, etc., with each. It shows that parallel indifference curves would mean not only that the income effect is zero, but that alternative dollar measures of a participant's gain from trade converge, and that these dollars can be summed over all participants to provide a measure of exchange efficiency.

It reveals, even to undergraduates, that equilibria may not be unique, that determinancy is not settled by counting unknowns and equations, and that stability in a market depends on much more than the slopes of its demand and supply. In any event, it always distinguishes between equilibrium and actual values, and, recognizing that disequilibria are pervasive, it explains that what really needs to be predicted are the time paths of variables.

It discloses that the explanatory variables in predictive demand and supply functions include previous prices (and therefore commitments and expectations); that responses to prices, advertising, etc., are subject to distributed lags; that individual demands cannot simply be summed unless they are independent; and that a curve portraying simultaneous alternatives does not indicate reversible paths over time or its elasticity relate to price changes. It does not confuse elasticities and slopes or label all of a nonhomogeneous curve elastic.

It specifies that demand is long-run, not if it shows pounds bought during a decade instead of a week, but if it shows pounds per week (or decade) that would sell at each price after it (and incomes, etc.) were constant for a decade. By a short-run function it means not what happens soon but what would happen if no start were made on certain time-consuming readjustments.

Indifference curves are permitted to touch axes, especially if one represents money. It is mentioned that the curves neglect indivisibilities and monotony and that they are assumed to be independent of the budget line—thereby neglecting conspicuous consumption, inference of quality from price, and expectational problems. It is explained that the height of a budget line depends on more than current income and that its slope equals the negative inverse of the price ratio. The algebraic derivation of consumer equilibrium uses not a specified utility function but any increasing function of the function.

It emphasizes that profit maximization is only one of a number of plausible axioms and indeed is an ambiguous one in a world with uncertainty and multiple interest rates. It explains that learning and equating  $MC$  and  $MR$  are time-consuming and expensive—and anyway may not maximize stockholders' wealth when there are discontinuities or unmet second-order and global conditions or when the producer must precommit himself or worry about entry or reputation. It recognizes that the more concentrated side usually admin-

isters prices, not quantities, and explains why. It shows that a budget maximum or an interest charge on additional outlays converts  $MRP = MFC$ , etc., into limiting cases.

It recognizes disagreement about whether returns to scale eventually diminish. It mentions rising at-plant input costs and internalization of external benefits. It distinguishes between plant and firm, between types of integration, and between instantaneous rate of output and hours of operation per year. It scallops or steps  $MC$  when output is increased by setting to work another group of machines, another shift, or another plant. It does not say 2.5 ( $K + L$ ) exhibits diminishing marginal products. It distinguishes discretionary fixed costs (e.g., insurance) and wholly fixed costs (e.g., bond interest). In counting basic solutions to linear-programming problems, it includes corner points on the axes.

Its conception of industry performance includes not only long-run equilibrium scale and price-cost relations but also progressiveness, product suitability, unethical practices, etc. It does not regard variety as opposite to standardization (e.g., of clothing sizes) and equivalent to advertising or say that nonprice and price competition function the same way.

It subdivides oligopoly, perhaps using Bain's types of concentration curves, does not say that what is true of two sellers extends to three or more, does not equate numbers with competitiveness, and does not say the antitrust laws maintain many firms per industry. It reconciles an assertion that oligopoly prices are rigid with an (erroneous) assertion that a kinked demand curve makes price increases likely when demand grows, and also with an assertion that price leadership (which irons out the kink) is common. It does not, after Oxenfeldt's analysis, repeat that barometric leadership merely establishes the prices that would be set by competition. It indicates the obstacles to joint maximization. It explains bilateral monopoly equilibrium and indicates its importance for bargaining associations and antitrust.

It does not equate pure competition with efficiency. It does not extol  $P = MC$  regardless of margins elsewhere. It recognizes that distance, delays, ignorance, and immobility can change every conclusion. It indicates that easy entry may produce cycles of entry and exit. It does not say that no incentive to enter exists when profits are small for the group. It distinguishes between liquidation of a company and departure of its capacity. It does not emphasize constant demand (and zero profits) to one farmer and varying prices to his industry. It explains how prices change if no one has any bargaining power. It mentions the surprisingly useful theory of price and production over space.

It recognizes that theories consist of axioms and theorems, together with a purview of circumstances wherein the implications of the axioms are deduced. It emphasizes that theorems (e.g., the substitution effect for a "consumer" is negative) are established logically—and therefore may be false empirically. It understands that a theory, however approximate, is scientific only if it is tested against facts and progressively modified to enhance predictive power, and therefore descriptive or normative usefulness.

Some of these desiderata are still fond hopes for the profession and are, consequently, not helpful in choosing among textbooks. On none of them, however, does Watson's book measure well. Teachers who now assign a text such as Baumol's *Economic Theory* would find Watson's work less comprehensive, less intensive, less rigorous—and longer.

On the other hand, teachers who want to avoid derivatives and who now use a careful but simple text such as Leftwich's *Price System* in their "intermediate" theory course might possibly find Watson's book a serious contender. It has broader coverage, together with attempted applications, occasional mathematical notes, careful printing, and helpful diagrams, summaries, exercises, and references.

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**Economic History; Economic Development; National Economies**  
*Structural Interdependence and Economic Development—Proceedings of an International Conference on Input-Output Techniques, Geneva, September, 1961.* Edited by T. BARNÁ WITH W. I. ABRAHAM AND Z. KENESSEY  
New York: St. Martin's Press, 1963. Pp. x, 365. \$15.00.

*Input-Output Tables—Their Compilation and Use.* Scientific Conference on Statistical Problems, Budapest, 1-5 June, 1961, Branch A. Edited by O. LUKÁCS, GY. CUKOR, P. HAVAS, AND Z. ROMÁN; translated by L. Várady.  
Budapest: Publishing House of the Hungarian Academy of Sciences, 1962. Pp. 292. \$8.50.

The most fascinating aspect of input-output analysis and its kindred branch of economics, mathematical programming, lies in the fact that concepts and problems form a common universe of discourse across Western and Collectivist economies. This is increasingly so as the interest in Western I-O analysis has swung toward development planning, as the first volume above signifies.

The volume edited by T. Barna reports on the third of periodic major Western conferences on I-O work. This 1961 Geneva conference was preceded by the 1951 meeting at Driebergen, where theory was dominant, while in the second, in 1954 at Varenna, computational procedures were important. The volume edited by O. Lukács, reporting on a Budapest conference held almost simultaneously, is also the third in line, the previous Warsaw and Berlin meetings having been devoted to I-O and socialist planning. Though there is great commonalty of ideas and problems across the Iron Curtain, there was little interchange of personalities at these conferences; of the 17 papers at Geneva, two were by economists from the Collectivist countries, and a single Western contribution appears among 21 papers in the Budapest volume.

Before embarking on a detailed survey, the general impressions left by these volumes may be noted. First, the enormous growth in the 1950's in work of an I-O nature—conference attendances given by Barna in the Introduction are one guide, and another is the great range of countries mentioned through-

out both volumes. Second, while the first I-O work began in Soviet Russia in 1923-24 (and Leontief published a discussion on this work in 1925), it seems the West has now gathered pace, and its work and discussion of many technical problems in I-O are now more advanced (the sputnik effect again). This is evidenced, looking at work done, by the fact that I-O tables for most of the Eastern European countries other than Russia were not in preparation until the end of the 1950's, while, of course, they were under way in the United States and in several European and Commonwealth countries in the 1940's (see the useful table in the Budapest volume, p. 49). Still on the relative rate of advance, when we turn to technical problems, it is noteworthy that many of the difficulties for future consideration noted by Kenessey in the Budapest volume are actively and adequately discussed in the Geneva papers. The third general impression is the increased interest in I-O for planning (in the West, that is), but the decline in interest in both areas of the world in computational problems (due partly to computers, partly to greater division of labor—economists leaving computation to computer departments). Fourthly, there is now an easy marriage between I-O and programming in its various forms, authors in both volumes slipping between one approach and another and considering "mixed" techniques.

Neither of these volumes can be appreciated without prior knowledge of I-O analysis and, at least, *linear* programming. The knowledge that a reader requires is essentially the following. First, the different degrees of "openness" that an I-O system can possess with regard to household operations, foreign trade, government operations; e.g., some or all of household supply of labor and purchase of products can be endogenously included. Several disputes in the volumes turn on this issue, e.g., in the first volume, that between Fox, Sengupta, and Day. Second, the problems of coefficients: aggregation, average versus marginal values, actual versus forecast values, and particularly changes over time. The time-change problem clearly assumes great importance when attention is focused on economic development, because growth through technical change is change in the parameters, which technically it is highly convenient to assume constant. Third, the essential difference between I-O and L-P, which can be understood from a childish example: in the "production" of car mileage using two factors, gas, and engine wear, I-O corresponds to a one-gear car, L-P to a choice of gear-ratios (each using different combinations of gas and engine wear per mile). Thus only in L-P does the question arise of choosing a gear to maximize mileage for given supplies of gas and engine or minimize inputs to achieve a stated mileage. And only in L-P, when the optimum gear is determined "centrally," does the question arise of using prices to guide "decentralized" car-drivers to choose the appropriate gear. Fourth, the basic distinction between current or flow-flow relations and capital stock-flow relations (dynamic I-O tables) must be understood.

Thus forearmed, we can look at each volume in more detail. Barna's Introduction mainly discusses the broad questions already covered here. No collection of Western I-O papers would be complete without a contribution from Chenery, and he begins with a demonstration (founded on an earlier article

in this *Review*) of how interindustry use of product grows more than proportionately to total production as per capita income increases. As Ghosh, later on in the volume, reminds us, we need to remember the effect of classification here. If the economy does consist of "islands" of production, you get zero or great interindustry flows according to whether sectors coincide with the islands or cut across them. Chenery's graph is partly to refute a view that I-O is inappropriate in less-developed economies—characterized by a paper of this reviewer—a misunderstood paper (also used by Eleish later) which in fact considers the *best* use in an underdeveloped country for extremely scarce statistical resources (in fact, *one* statistician in Tanganyika in 1956; compare the entourage there today!). Chenery is very good on a suitable mix of I-O and L-P for development planning and on the inadequacy (because of no recognition of interdependence) of the long *QJ.E.* discussion on Investment Criteria for Less-Developed Countries.

The remainder of Part I (Models of Economic Development) contains a paper by three Polish economists and papers by Mather of India, Fox, Sengupta, and Day. Each touches on several issues, but the dominating themes (in order) are the problems of change of technical coefficients in the growth process, independent "islands" (U.S. agriculture?) which need not be integrated in the I-O scheme, and prices and allocation (when programming is introduced).

Part II of the Geneva book is devoted to a relatively new area, regional I-O, a little off the line of the main theme of the book, underdevelopment and development planning, especially as all the papers—by Isard and Smolensky, Leontief and Strout, Hirsch, and a note by Leibling—refer solely to the United States. Still, they do show what a useful addition regional I-O can be to national planning to tackle regional underdevelopment, though applied to less-developed countries, "regional" activities would probably need to be drawn tribally or racially. The Leontief paper is much more technical than the one by Isard and, among other things, is a great exercise in "subscription."

Part III opens with a long essay by Nemchinov (the Russian Chenery, I gather from the Budapest book). This is about Soviet statistics, Soviet planning, Marxist theory, and so on; very little really about I-O. Presumably it would have been difficult for the editor to do anything about this, and we do have to remember that the Collectivist economists conceive I-O as a small cog in a much wider scheme of planning and control than we do. The French contribution, from Delange, in contrast, is confined to the French official I-O tables and deals not at all with French planning, where perhaps a little on the role of I-O therein would have been interesting. Eleish and Bruno, on Egypt and Israel respectively, give very good accounts of the special problems of I-O in less-developed countries. A special feature of the first of this pair of papers is the discussion of the different treatment of competitive imports and noncompetitive imports in I-O analysis used in programming industrialization, and of the second, a test of development-plan forecasts with subsequent actual happenings. Balboa gives a useful account of E.C.L.A. work on I-O in selected Latin American countries.

The final section, Part IV, deals with statistical problems: the Japanese contribution demonstrates how the labor coefficient varies considerably with size of firm, causing difficulty in aggregating over same-product sectors in economies with traditional and modern production methods. Anne Carter deals with the inconsistency of using capital-coefficients implying new techniques simultaneously with extrapolations of existing flow coefficients implying old techniques. Sevaldson and Watambe separately deal with changes in coefficients at an advanced level. The volume concludes with an interesting discussion of the pros and cons of a U.N. standardized scheme for I-O collection similar to the System of National Accounts (the base is already laid with this and the U.N. International Standard Industrial Classification of All Economic Activities). Added to the existing great variety within the endogenous part of the I-O tables will be a steady loss of the current uniformity in final demand columns and primary input rows as these are broken up for endogenous inclusion (e.g., labor inputs by expenditure patterns, savings habits, family size, social class?). In a word, standardization would permit international comparisons and encourage international discourse, but may limit specific national usefulness and would stultify creative change (the price of the S.N.A.!).

The Budapest volume is a more systematic account of the construction and use of tables in most of the Eastern European countries, but it does seem only on the verge of many technical problems tackled squarely in the Geneva book. I shall try to draw out the essential differences in I-O in the Collectivist economies compared with others. I-O is much more of a small piece of wider planning machinery in Eastern Europe, as already mentioned. Basic I-O statistics are better in Collectivist economies since firms are sympathetic to the needs of State planning (a claim made at Budapest). Prices used for values in I-O are not market prices but administered ones, so there arises the possibility of calculated "shadow" prices (if programming is added to I-O) actually being put into use in the Collectivist economy. Greater use of engineering or planned coefficients rather than actual ones occurs. The construction of the table in Collectivist economies is somewhat different from Western methods to accord with Marxist theory, e.g., the distinction in the endogenous part of the table between productive and nonproductive sectors; primary inputs classified so that their remuneration corresponds to the "wage-fund," "surplus product," etc. Note the significance of all these differences for the world-standardization discussion.

I am inclined to say that the Geneva volume is for I-O students, the Budapest one for students of Marxist economies. It is difficult to allow for one's own outlook: the Budapest volume has been adequately translated into English but it still reads like a foreign language to someone not steeped in Marxist economic terminology. Perhaps a Soviet economist would say comparable things about the Geneva book, but it is undeniable that the only *explicit* propaganda appears in the Nemchinov paper in Barna's volume and at several places in the Budapest volume. In spite of this, I-O retains its pre-eminent place as that part of economics with the widest political and geographi-

cal spectrum—a World Government would know where to begin its economic statistics.

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*Economic Development: Past and Present.* By RICHARD T. GILL. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. vii, 120. \$3.95.

This book is one of nine volumes included in *The Foundations of Modern Economics* series, designed to serve the text requirements of the basic economics course. The stated purpose of the series, which is under the editorship of Otto Eckstein of Harvard, is to provide more flexibility in the choice of topics included in a first-year course through brief, self-contained volumes written by specialists in each field. This approach constitutes another assault on the firmly entrenched introductory tome in economics, reflecting perhaps a growing disenchantment with the overstuffed conventional text.

This volume under review presents a meaty introduction to the field of economic development for students and the general reader. It covers in six chapters the "General Factors in Economic Development" (population, resources, capital); "Underdevelopment, Growth and Getting Started" (Rostow's stages, classical theory, vicious circles, big push, etc.); "Beginning of Growth in Economically Advanced Countries" (impact of the Industrial Revolution); "The Growth of the American Economy"; "The Problems of the Underdeveloped Countries," (meaning of, demand for, obstacles to, required scale of development); "Alternative Approaches—India and China." Emphasis is on historical description and review of institutional factors contributing to the widening gulf in international living standards. The problems of getting started, the evolution of technology and its impact on the late-comers, including the United States, are ably treated. Comparative statistics, charts, and tables support the exposition. The last two chapters, where one would expect the perspectives and modern tools to be applied, do not quite live up, in terms of analysis, to the build-up of the first four.

Some familiar concepts and historical themes might have been sacrificed in favor of dealing in depth with capital-output ratios, absorptive capacity, marginal savings, shadow prices, underemployment, *n*-achievement, and other need and aspiration criteria. Most of these terms in fact, even if implied in some of the discussions, are absent from the vocabulary of the text. Particularly where they have quantitative significance, these concepts would have provided a logical foundation—supplementing the historical and descriptive material—for a more problem-oriented and analytical approach to the case examples of India and China in the last chapter. How do the Indian and Chinese planners calculate specific capital-output, savings, and capital-import ratios required for generating per capita income gains to meet the demands of self-sustained growth and regional and international comparisons? Why have domestic savings ratios in India remained so unimpressive, and what devices can the planners suggest for increasing them, as well as reve-

nues from domestic taxation, so as to reduce the heavy dependence on foreign capital? What are the eclectic issues which complicate the population variable in the growth equation? Analysis of basic operational factors gives the student some appreciation of the concrete tools and dynamic planning methodology currently being applied to gauge and forecast development requirements.<sup>1</sup>

The author also stresses the usual "capital poor" status of the underdeveloped areas, but more might have been said to show that they are poor in markets, innovation, entrepreneurs, administration, and motivation. In the reviewer's experience, the capital-shortage thesis needs qualification where capital availability is a lesser need than locating worth-while projects and organizing and motivating the local manpower to launch and administer them. In some areas, ten or more aid-giving countries and agencies, and even indigenous and foreign private capital, are seeking outlets simultaneously. The capital is there, but the proper feasibility groundwork is inadequate. Nor is it a question of absorptive capacity only, for this is expandable. More probing of these bottlenecks would have added depth and currency to the discussions.

A condensed coverage cannot be expected to please all of the diverse preferences of pedagogues, who will, in any case, supplement as they see fit. The omissions elaborated will not be missed by the general reader. Some may even prefer to add their own problems and analytics. It is more important for an introduction that the major perspectives and theses are here to inform and challenge. In this respect, this well-written treatise succeeds commendably.

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<sup>1</sup> The level prescribed for this book precludes application of Harrod-Domar, Cobb-Douglas, or Schumpeter-type growth models or functions, yet the student needs to supplement the static knowledge with the dynamics of such model building.

*Histoire économique: XIX<sup>e</sup> et XX<sup>e</sup> siècles.* 2 Vols. By J.-A. LESOURD AND C. GÉRARD. Paris: Armand Colin, 1963. Pp. 292; 372. F 19.80; F 24.

The appearance of a new introductory textbook in a foreign language is not often of direct concern to U.S. economists. But in the field of economic history there are so few books which are both modern and sufficiently broad to serve as introductory texts that such linguistic provincialism is inappropriate. The present book, covering the period from 1750 to the present, should prove useful both to teachers and to students in first courses in economic history who can read French.

The authors, J.-A. Lesourd and C. Gérard, are, respectively, teacher of history at the University at Nancy and teacher of history and geography in classes preparatory to the *grandes écoles*. Much of the most interesting French work in economic history has been done by historians and geographers, and I feel that the book gains, on balance, from following this tradition rather than that of political economy.

A major virtue of this text is its form. It is attractive, easy to read, and convenient to use. There is an abundance of charts, tables, graphs, maps, and dia-

grams, usually appropriate and often imaginative, some of which serve to illustrate concepts as well as to present factual material. In addition there are appendices on the presentation of statistical material, on weights and measures, and general and chapter bibliographies. Few non-French works are mentioned, showing that provincialism is not limited to Anglo-Saxon authors.

Lesourd and Gérard have chosen to be comprehensive rather than selective in their coverage, relying on pithy generalizations and *caveats*, as well as occasional forays in depth as defense against the perils of superficiality. On the whole I think that they have succeeded well. The extended introduction touches almost every field of economics insofar as it can shed light on the history of the nineteenth and twentieth centuries. The chapter dealing with the history of economic doctrine emphasizes social philosophers rather than theorists (the chronology omits Wicksell, Marshall, Böhm-Bawerk, and Schumpeter, *inter alia*, but includes French writers whose intellectual importance is at least open to doubt). The treatment of institutions (firms, banks, monetary systems) is particularly good, whereas the chapters on prices and cycles are marred by ambiguity and flaws in the data. The authors use the concept of "real prices," due to Fourastié, with prices weighted by average hourly money wages, thus biasing price series downward for the nineteenth century. This in turn leads them to characterize the period as one of downward long-term trend (in their treatment of cycles), with no clear distinction made between movements in prices and changes in the level of activity.

In the chronological part of the book, the authors have chosen to present a rather detailed picture of the world economy at certain times (1750, 1850, 1913), and to treat the intervening periods sector by sector. This approach provides a nice balance, without undue concentration on particular countries in the "snapshots," and yet opportunity to give more detailed accounts of particular aspects of French, European, or American experience.

In the last part of the book the story is brought up to the present time, in this reviewer's opinion, with distinctly less success. Here the source materials are incomparably richer, and the authors are not able to carry off the herculean task of digesting so much scholarship and analysis. Economic errors and unsupported or questionable arguments become more frequent, while the treatment of factual material is more along the lines of economic geography and relies considerably on raw statistics. Nor is the sketchy coverage of problems and theories of underdevelopment adequate either as a substitute for, or a complement to, work on that subject proper.

As regards the historical discussion, I would argue with Lesourd and Gérard on a few specific points. In their treatment of French agriculture they imply that the extension of acreage planted in wheat during the nineteenth century represented progress and rationalization (p. 298). In fact, it was the result of protection and local autarkic efforts. The considerable work done, in the United States and elsewhere, on the relationship between French economic retardation and entrepreneurial attitudes (the family firm, the role of banks in industry, etc.) is largely ignored. As regards England, the mid-century period of high-farming is not mentioned. Nor is the failure of British industry to progress in new lines given its due in accounting for the relative decline of Great Britain toward the end of the century.

This is an interim synthesis, as the authors and publishers point out. It suffers in spots from hasty writing, from occasional specious clarity in formulating vague concepts, from a slight predilection for numbers at any cost, even when adequate data are not available, and from touches of implicit Marxism. But it is a useful, readable, and courageous effort. In fact, the authors have accomplished something of a *tour de force* in combining a well-written, concise, and coherent text with the presentation of a large body of factual material, much of it not easily available to the student. A later version, incorporating data and analysis becoming available as the result of current research, and ending in 1918 (or 1939), would be of considerable help to students and teachers interested in economic history in the context of economic growth, theory, and quantitative methods.

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*Profile of Michigan—Economic Trends and Paradoxes.* By STEPHEN P. SOBOTKA WITH T. A. DOMENCICH. New York: Free Press of Glencoe; London: Collier-Macmillan, 1963. Pp. xvii, 174. \$4.95.

This book is a monumental example of the misuse of economic theory, statistics, and mathematics. It might well be read by every graduate student as an example of how not to do economic research.

The starting point for Stephen Sobotka's analysis is the apparent paradox of unemployment amid plenty in Michigan: high wages and high income per capita are accompanied by higher rates of unemployment than in nearby industrial states. At least at the time the study was done these conditions prevailed. Sobotka argues that the only reasonable explanation for this paradox is that high wages have caused business firms to substitute other factors of production for labor, thereby causing high rates of unemployment. Calculation of "elasticities of substitution for labor" shows that in the transportation-equipment industry, which is very important in Michigan, it is relatively easy to substitute against labor. These calculations indicate that a 1 per cent increase in relative wages in the transportation-equipment industry in Michigan results in a 2 per cent decline in employment in the industry. The policy conclusion is obvious: Sobotka advocates "public policies that will erode the power of unions to raise the wages of their members and thus cause other competent workers to be unemployed."

The uniqueness of Sobotka's research lies not in the argument he makes, which is almost as old as economics itself, but in his efforts to quantify the substitution against labor and compute elasticities of substitution by industry. The procedure is described in Chapter 4, which had been published independently, and in an article by Minasian, who apparently worked with Sobotka on the project. It is based on the Arrow-Solow production function.<sup>1</sup>

<sup>1</sup> Stephen P. Sobotka, "Michigan's Employment Problem: The Substitution Against Labor," *Jour. Business*, April 1961, 34, 119-28; Jora R. Minasian, "Elasticities of Substitution and Constant-Output Demand Curves for Labor," *Jour. Pol. Econ.*, June 1961, 69, 261-70; Kenneth J. Arrow, Hollis B. Chenery, Bagicha Minhas, and Robert M. Solow, "Capital-Labor Substitution and Economic Efficiency," *Rev. Econ. Stat.*, Aug. 1961, 43, 225-50.

It is possible to analyze Sobotka's method, but only by going beyond Chapter 4 to Minasian's article. At no point in the book are the critical mathematical-statistical procedures described and the crucial calculations given. This is quite inexcusable, for the unsuspecting reader is asked to accept on faith methods and calculations which close examination shows to be wrong.

The first mistake is an economic one. The "elasticity of substitution" on which the whole analysis rests turns out to be only a mathematically inverted measure of labor productivity. In Sobotka's words:

The elasticity of substitution was computed by a series of regression equations in which labor input, in either physical or money terms, was the dependent variable and average hourly earnings the independent variable. *To allow for interstate differences in scale of operations, the labor inputs were deflated by value added* (p. 60, my italics).

The usual measure of labor productivity is:

$$\frac{\text{value-added } (V)}{\text{labor input } (L)}.$$

When it is regressed against wage rates, a positive relationship appears. The higher the wage rate, the higher is productivity ( $V/L$ ). What Sobotka did was to reverse the relationship to:

$$\frac{\text{labor input } (L)}{\text{value-added } (V)},$$

and, *mirabile dictu*, got a negative relationship. Instead of recognizing it for what it was—a mathematically inverted measure of productivity—he thought he had found a measure of the elasticity of substitution against labor.

Analysis of Minasian's article, in which the method is described in more detail, confirms this point. Minasian's equation (2) was the basis of the calculation of "elasticities of substitution":

$$\frac{W \cdot L}{S} = A W^{(1+n)}$$

where

$W$  = price of labor

$L$  = quantity of labor

$S$  = value-added

$n$  = elasticity of demand for labor with respect to its price

$A$  = a constant

Dividing through by  $W$ , it becomes:

$$\frac{L}{S} = A W^{(n)}.$$

Solving for  $W$ , it becomes:

$$W^n = A^{-1} \left( \frac{S}{L} \right)^{-1}$$

which is equivalent to

$$W = A^{-1/n} \left( \frac{S}{L} \right)^{-1/n}$$

This equation tells us, in graphic terms, that there is a straight-line relationship between wage rates and productivity ( $A$  is a constant), and curvature is introduced by substitution against labor ( $n$  is an exponent). The assumptions, inherent in the mathematical formula used, are highly unrealistic in themselves. However, if  $n = -1$ , the equation becomes:

$$W = A \left( \frac{S}{L} \right)$$

which is nothing more than saying that wage rates are functionally related to labor productivity.

Since  $n$  is Sobotka's "elasticity of substitution against labor," it is vitally important for his argument to show that his figure for the transportation-equipment industry is significantly different from  $-1$ . Otherwise, the relationship between wages and employment is all explainable by the productivity of labor and none of it by substitution against labor.

Here Sobotka (and/or Minasian) made a second mistake, this time in statistical technique. Using data for the transportation-equipment industry from 18 states for a single year, the elasticity of substitution for labor was calculated at  $-2.04$ , with a standard error of  $.49$  and an  $r^2$  of  $.22$ . According to my calculations this elasticity of substitution is just barely significantly different from  $-1$  at the 5 per cent level. But it would not be so with only 17 observations. A question should immediately be raised about the homogeneity of the 18 observations. Are they from the same universe? Are they observations of the same family of phenomena? For the purposes of this study they are not. The transportation-equipment industry in Michigan produces automobiles primarily; in California, aircraft; in another state it might be ships or railroad equipment. Within the automobile industry itself, design and tool and die operations are heavily concentrated in Michigan, while in other states assembly operations predominate. In a case like this, when the statistical test shows borderline significance at best and the data are of doubtful homogeneity, the proper technique is to conclude that statistical significance has not been proved. The figure for elasticity of substitution for labor cannot be considered significantly different from  $-1$  when a close look is taken at the nature of the observations on which it is based. The proper conclusion to be drawn from Sobotka's mathematics and statistics is that high wage rates in Michigan are associated with high levels of labor productivity.

This does not mean that high wage rates in Michigan have been of no

importance in causing employers to substitute against labor. But it does mean that Sobotka has (a) failed to prove that they have been and (b) failed in his effort to quantify the effect.

The study has other faults. The average levels of unemployment shown for Michigan are high primarily because of conditions in 1956-58 and not because of consistently high levels throughout the period studied. Although the last data analyzed are for 1958, the author continually writes as if the trends shown still persisted at the time of publication. One would hardly suspect that in 1963 the rate of unemployment in Michigan was well below the national average and below that of the surrounding states.

Uncritical reliance on statistical techniques has led Sobotka to ignore some of the basic causes of Michigan's economic problems: the tendency of any geographically concentrated industry to decentralize in an expanding economy; the effect of the Supreme Court's basing-point decision on location of metal fabricating plants; the large decline in defense contracts in Michigan as national defense shifted to missiles; the excessively high labor participation rates in Michigan in 1953-55; the self-generating and competitive nature of automation once it has begun in an industry. All of these factors were discussed in the literature on the Michigan economy at the time Sobotka was working on the problem, and they should not have been ignored.

Sobotka's fiasco is a classic example of a methodological trap that is always ready to snare the unwary economist. Mathematical and statistical techniques can become an end in themselves, obscuring the underlying economic reality which should be the central focus of attention. This is what happened to Sobotka, with disastrous results.

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### **Statistical Methods; Econometrics; Social Accounting**

*On the Accuracy of Economic Observations.* 2nd ed., revised. By OSKAR MORGENSTERN. Princeton: Princeton University Press, 1963. Pp. xiv, 322. \$6.50.

The first edition of this book, which appeared in 1950, will be familiar to many readers. The present edition is substantially enlarged and brought up to date. Part I discusses the nature of economic data, detailing at some length both the sorts of failings to which economic data are subject and the sorts of misuse to which they are commonly put. Part II consists of seven chapters, each of which discusses a particular type of statistics, namely foreign trade, prices, mining, agriculture, employment and unemployment, national income, and growth and growth rates. Finally, there is a short chapter on the orientation of future research.

In his concluding chapter Professor Morgenstern is careful to state that "there was no intention to make a chamber of horrors, nor to belittle the efforts of the many good workers to whom our data are due." Unfortunately, an atmosphere of hopelessness does pervade the book. To this reviewer, how-

ever, the state of economic statistics does not seem to be quite as black as it is pictured here.

The types of error with which Morgenstern is concerned run the gamut from failure of the data to measure the concept they are supposed to (often because the concept is itself imprecisely defined), through mishandling of statistical techniques by the makers of the statistics, to deliberate falsification, and finally, such factors as machine errors in the processing of data. His concept is thus far broader than what is usually thought of as statistical error.

Morgenstern's conclusion, with regard to all of the types of data examined, is that margins of error are extremely wide, and that errors are not likely to be offsetting. Even biases in the data can not be relied on: for instance, changes in national income are pictured as little, if any, more reliable than the absolute levels of national income. His methods of arriving at these conclusions are rather hard to pin down; for the most part, they seem to come down to looking at a set of data to see if it looks reasonable—and what is reasonable varies from page to page. The BLS is chided for never revising its figures, but the OBE's revisions are used against it as evidence of error. He regards the greater frequency of revision in postwar as compared to prewar data as evidence that the statistics are not getting any better; but later he views with alarm the fact that figures ten years old are still being revised.

It is in his prognosis of the future that Morgenstern's pessimism is most acute. In discussing what can be done about the present parlous state of affairs, he relies mainly upon a hope that things will get better by-and-by, as the level of economic education and sophistication of people in general rises. His concrete proposals appear to be three: (1) discard data based on lies; (2) prevent government agencies such as the Council of Economic Advisers "from presenting to the public economic statistics as if they were free from fault"; and (3) "insist that economic statistics only be published together with an estimate of their error." He does not, however, tell us how to achieve these ends. Nor is it entirely clear how far he really means to push them. Few economists would disagree with him when he argues that changes of one- or two-tenths of a per cent in price indexes have no meaning; on the other hand, few would follow as far as his statement that quarterly national income figures are "grotesque." Does he really mean that every time the Council of Economic Advisers prepares a speech for the President, they must either use such language as "the gross national product rose \$10 billion (plus or minus \$10 billion)" or else say nothing at all about what is happening to economic activity? He writes rather sadly that the answer to critiques such as his is always, "Yes, but the data we have are better than nothing at all"—and I do not think that he really means to disagree with this.

It is Morgenstern's third recommendation that is likely to give the average producer of economic statistics the most qualms, and at the same time most seriously to mislead the unwary or unsophisticated user of data. To illustrate from the discussion of national income statistics, Morgenstern quotes (as if they applied to present-day OBE national income accounts) some remarks made by Simon Kuznets in his 1942 book relating to data of an earlier period compiled by the National Bureau, to the effect that the national income was

subject to an error of 10 to 20 per cent. Since 10 to 20 per cent of \$500 billion is \$50 to \$100 billion and the observed annual changes in gross national product run much less than this, Morgenstern regards any discussion of even the direction of annual change, much less its magnitude, as idle. It is just here that the danger of attaching simple-minded estimates of errors lies. Despite Morgenstern's dismissal, the estimates *are* subject to the same general biases from year to year, and the errors to be expected in the change figures are less than those in the absolute figures. No one really cares what the error in the absolute magnitude of the gross national product is, because no one has yet thought up a way to use this figure in isolation. What really would be needed, to satisfy the need Morgenstern feels, would be the computation of estimates of error for all of the possible combinations and comparisons which anyone might wish to use. The statistical agencies are obviously justified in arguing that any such performance is beyond the limits of common sense, and that the only practical solution is for the user to familiarize himself with the sources and methods at least enough to make his own judgment about the validity of the data he is using in the particular use he wishes to make of them. Admittedly, many users will not make the necessary effort, and there will be much misuse of the data—but this is likely to be much less harmful than the assignment of outrageously large error figures to all economic statistics.

NANCY D. RUGGLES

*Yale University*

*Linear Programming and Extensions.* By GEORGE B. DANTZIG. Princeton: Princeton University Press, 1963. Pp. xvi, 625. \$11.50.

It is now more than a decade since George Dantzig began the series of RAND papers that have culminated in this monumental treatise. The book covers the field authoritatively and has something for everybody: proofs for the mathematician, model formulation for the operations-research practitioner, computational techniques for the electronic-computer specialist, and network theory for Hungarians.

To an economist who is convinced that linear programming was really invented by Ricardo, this volume is bound to be annoying. Dantzig's interpretation of the simplex multipliers as competitive prices does not appear until Chapter 12—well after he has covered simplex computations, duality theorems, parametric programming, and given two alternative proofs that the simplex algorithm terminates in a finite number of steps. Incidentally, he succeeds in covering most of this ground without the use of matrix algebra.

No matter how blasé the economist's attitude toward computing techniques, he cannot afford to deny himself the pleasure and the educational experience of Chapter 23, where Dantzig explains the decomposition principle, his joint discovery with Philip Wolfe. Unlike the first Dantzig and Wolfe paper on the subject, this one is as much fun as a conversation between Pooh and Piglet. More than one coffee-hour will be whiled away in guessing at the identity of the renowned economist-consultant, F. M. Dalks. Maybe some of those coffee-hours will also be spent in debating just why the decomposition

principle is guaranteed to be convergent—even though it can be interpreted as decentralized planning with incomplete information at the center. What is the essential ingredient here that leads to convergence? With countercurrent flows of price *and* quantity information, it may be that D. & W. have invented the kind of friction that Dorfman-Samuelson-Solow had in mind when they remarked, "In competitive markets, as in pendulums, a little friction may be needed for stability."

Dantzig's book deserves to be widely read. The publishers are to be commended for having kept the price of this volume within reasonable limits.

ALAN S. MANNE

*Stanford University*

*Nichtlineare Programmierung.* By H. P. KÜNZI AND W. KRELLE WITH W. OETTLI. Berlin, Goettingen, Heidelberg: Springer Verlag, 1962. Pp. xiii, 221. DM 38.

Linear programming is concerned with the maximization (or minimization) of a linear function under conditions which are linear inequalities. These methods have been successfully used by economists in both theoretical and applied economics. But it is evident that the restriction to linear relationships cannot be considered anything but a very rough first approximation. Generalizations of the methods of linear programming for nonlinear functions meet formidable mathematical difficulties.

This is an excellent monograph on nonlinear programming. It is self-contained, and the first chapter includes the necessary mathematical theorems (matrices, vectors, linear equation systems, projections, quadratic forms, convexity). In the second chapter we have a short introduction to linear programming, where duality and the simplex method are stressed. The third chapter presents convex programming and the Kuhn-Tucker Theorem, which is of utmost importance in nonlinear programming.

The remaining chapters deal explicitly with theories and numerical methods proposed by various authors in the field. In Chapter 4 we have quadratic programming. The methods by Hildreth and D'Esopo are presented in the fifth chapter. This method uses the duality theorem.

The method due to Theil and van de Panne utilizes the complete description method (Chapter 6). Chapter 7 brings the method of Beale, which applied the somewhat generalized simplex method. Chapter 8 presents the method of Wolfe, which also uses the simplex algorithm, as do the method due to Barankin and Dorfman (Chapter 9) and the method due to Frank and Wolfe (Chapter 10).

Entirely different are the method of gradients (Chapter 11) and the method of projected gradients due to Rosen (Chapter 12). The degenerate case is also treated here. The famous multiplex method of Frisch (Chapter 13) and the method of permissible directions due to Zoutendijk (Chapter 14) are special cases of the gradient method. The capacity method of Houthakker (Chapter 15) is applicable to strictly convex quadratic programs under somewhat restrictive conditions.

The mathematical theory underlying these procedures is clearly presented,

and all methods are illustrated with the help of numerical examples. A very complete bibliography surveys the literature in the field (pp. 213-18).

This outstanding monograph can be recommended to economists and workers in operations research interested in the field of nonlinear programming.

GERHARD TINTNER

*University of Southern California*

*Introduction to the Theory of Games.* By EWALD BURGER. Translated from German by John E. Freund. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. vi, 202. \$10.00; text, \$6.00.

This is a very useful textbook or reference book for game-theory courses trying to provide rigorous proofs for the most important mathematical theorems without presupposing very extensive mathematical preparation. (But it does require some mathematical sophistication from the student or else some elaboration by the instructor of the rather concise mathematical reasoning.) As the author himself points out in the Preface, the book contains very little heuristic justification for the assumptions and definitions used; instead it concentrates on the formal mathematics. But for this very reason it is a very suitable supplement to Luce and Raiffa's *Games and Decisions*, which is very good in heuristic explanations for game-theoretical concepts but gives few rigorous mathematical theorems and proofs.

Ewald Burger's book discusses noncooperative games (the theory of equilibrium points, with some economic applications), two-person zero-sum games (including a section on infinite games), and cooperative games (including the Shapley value). Particularly useful is an appendix on Sperner's Lemma, with elementary proofs for the Brouwer and Kakutani Fixed Point Theorems based on this Lemma (though there are a few misprints in the proofs).

The discussion is clear, rigorous, and to the point. The English translation is competently done but, like many other mathematical texts, it uses grammatical constructions too involved for easy understanding. Some of the long sentences could have been broken up with a gain in readability, even if this had meant somewhat greater length.

In the opinion of this reviewer, in case of a second edition the size of the book could be usefully increased by including additional material. For instance, it would be helpful to discuss a few more computation methods for finding solutions and equilibrium points; the section on infinite games could be expanded to advantage; discussion of the Nash-Zeuthen model could be included, etc. But even as it is this volume is a very welcome addition to textbooks in game theory.

JOHN C. HARSANYI

*Wayne State University*

*Linear Regression and Its Application to Economics.* By ZDZISLAW HELLWIG; translated from the Polish by J. Stadler. New York: Macmillan, Pergamon Press, 1963. Pp. xii, 238. \$5.00.

Dr. Hellwig, a Polish econometrician and senior lecturer in mathematics and mathematical economics at the University of Wrocław, has written a manual which will interest both economic statisticians and students of in-

tellectual developments in Eastern Europe. Simple linear regression, because of its mathematical "complexity" and computational difficulty, has not been utilized to any great extent by Poland's applied economists. Thus, in addition to presenting a fairly standard treatment of simple linear regression, Hellwig introduces a number of techniques which substantially reduce the computations normally required in regression analysis.

One-fifth of the volume is devoted to demonstrating the importance of simple linear regression to policy-makers. The result is a somewhat haphazard introduction to standard microeconomic theory, placed in a stochastic framework. Hellwig argues that regression analysis can be useful to the State in the setting of equilibrium prices and norms of production, and in judging the effects of alternate policies on equity. Because the author does not introduce multiple regression explicitly, the discussion of the application of regression analysis to economic research is much more confusing than is necessary. While some of the material may be of interest to scholars concerned with the influence of conventional economic theory on Polish economic thought, the reader interested in the applications of simple linear regression to economic policy can omit this section.

The discussion of statistical methodology provides a useful summary of much of the standard literature, a report of some of Hellwig's own research, and a large number of computational illustrations drawn from the Polish economy. Among the topics covered are specification of the simple linear model, hypothesis testing and estimation of parameters, tests of bivariate normality and the linearity of the regression function, transformation of curvilinear regression, and the use of regression analysis in the estimation of trends. While the discussion is often sophisticated, there is a great deal of variation in quality. The author sometimes neglects to make assumptions explicit. In the section on hypothesis testing, for example, where the reader is assumed to be familiar with elementary set theory and orthogonal transformations, the level of significance is defined as "the probability of an event that we consider as practically impossible" (p. 147). The discussion proceeds without any further explication of the theory of hypothesis testing.

The section of most interest to empirical workers is Hellwig's description of the computationally simple two-point method (or the method of grouping) for estimating the parameters of regression functions. While these estimators are less efficient than least-squares estimators (although they are consistent and unbiased), they are very useful in situations where electric calculators are not available or where a quick estimate of the parameters is required. The two-point method has also been widely developed for "errors in variables" models, and as a result there is a large body of literature dealing with the problem of the optimal grouping of the sample.<sup>1</sup> This literature is largely ignored by Hellwig. However, his discussion serves as a good introduction to the two-point method.

The major difficulty with this short volume is that it attempts to serve too many purposes: it is at once an introduction to microeconomics and statis-

<sup>1</sup> A. Madansky, "The Fitting of Straight Lines When Both Variables are Subject to Error," *Jour. Am. Stat. Assn.*, March 1959, 54, 173.

tics, a source book for the research worker, and a compilation of some of the author's papers. In addition, the lack of an index greatly restricts its usefulness to research workers. The book does serve as a reminder of the availability of simple computational techniques which have been largely ignored by economists.

BERNARD SAFFRAN

*University of California, Berkeley*

*Statistics for Economics.* By WILLIAM I. GREENWALD. Columbus: Charles E. Merrill Books, 1963. Pp. xviii, 366. \$8.75.

This is a textbook on statistics, one aimed at the student of economics who would become acquainted with the "application of (statistical) methods and techniques" to problems encountered in economic research. It is a self-proclaimed cookbook which places emphasis on a wide selection of recipes rather than on principles of nutrition. Consistent with these objectives, it does not attempt or claim originality or innovation in statistical methodology.

One may quarrel with the basic intent of teaching the technical manipulations of empirical research apart from the often quite subtle logical foundations upon which interpretation is based. But, as this is not the first textbook to exhibit this tendency, this review will attempt to evaluate the text on its own terms.

While a cookbook may justifiably concentrate on the "how" of statistical methods at the expense of "why" or "when," it should not neglect other obligations. It should provide its purchaser with a useful reference for consultation after his statistics course is long past and largely forgotten. In this regard it should possess an extensive index, a selective bibliography for specific topics at the end of each chapter, perhaps a glossary of terms, symbols and formulae, and certainly an appendix containing tables of statistical functions and logarithms. *Statistics for Economics* contains none of these things. Its index is three pages plus seven entries long; it has neither bibliography nor footnote references to sources of more complete or authoritative information; there is no glossary and only five 10-entry "samples" from various tabulated statistical functions.

At 366 pages this book is much shorter than other candidates for "Cookbook of the Decade." This implies that either depth or breadth must be sacrificed. In his preface the author admits that he made a "judgmental choice" among possible topics, but the reviewer is impressed rather by the lack of selection displayed in the body of the text. The inevitable consequence of superficial explanation and analysis is equally evident. Moreover, six pages of "prime" space in the first chapter are devoted to the tired old subject of significant digits and the associated rules of thumb for truncating sums, products, etc. This is a topic that is virtually ignored, and deservedly so, by more encyclopaedic texts. The general problem of propagation and cumulation of errors in complex numerical manipulations is, of course, important; it is also very complicated. But it is clear that the errors cannot be avoided by a ruthless application of the ancient rules of thumb. In fact, re-

peated application of such rules in a series of calculations is likely to ensure failure of the computational checks the author also recommends.

The first 40 per cent of the book "covers" general statistics, running the gamut from tabular presentation to simultaneous equation estimation. The treatment is necessarily thin, but it is suprisingly weak precisely where a cook-book should excel. This text does not stress efficient calculation procedures, it rarely presents illustrative calculations in terms of "real" data, and it does not provide problems for student practice.

The next 50 per cent is devoted to methods of analyzing economic time series. The conventional topics of index numbers, trend-seasonal-cycle-random decomposition, and National Bureau cycle analysis are presented along with the more exotic harmonic analysis and autoregressive models. This section is endowed with more illustrative applications than the preceding section but, to compensate, motivation for the various techniques is supplied even more sparingly.

A short final section is given over to empirical forecasting. It presents a classification of types of forecasting methods (but gives no examples), provides some very general remarks about the inscrutability of the future, and describes the formulation and use of indexes of diffusion.

Professor Greenwald has properly avoided the role of inventor in statistical methodology but he has been more adventurous in English usage. His choice of verb for forming or specifying an hypothesis is invariably *hypothesize*. While this may annoy the reader, he reads on to see what happens when a verb is needed for taking or forming an expectation. (This, too, ends in disappointment since the need never arises.)

There are other minor defects but it is questionable whether the text would be much improved by remedying them. Sentences such as "Therefore to say that something is random is to say that it is probable and not haphazard since a haphazard sample can contain elements of regularity" (p. 75) are unnecessarily obscure. A suggested treatment for time series plagued with autocorrelation is to "Employ multiple regression and correlation in which time is included as a third coinciding *or lagged* variable" (p. 292, italics mine). These examples are extreme but not isolated.

To summarize, this text compares very unfavorably with others on the market in a number of important respects. It is superficial rather than selective and it is completely lacking in several desirable features. Its strong points are not sufficient to balance the faults and make it a reasonable alternative to available texts.

HAROLD W. WATTS

*University of Wisconsin*

### Business Fluctuations

*The Chinese Inflation, 1937-1949.* By SHUN-HSIN CHOU. New York: Columbia University Press, 1963. Pp. xiii, 319.

The second major study to be published in the United States in recent years about the inflation in China from the onset of the Sino-Japanese War

in 1937 to the advent of Communist rule in 1949, this book warrants high praise on a number of bases. The subject has been thoroughly researched and has been analyzed with great care; judicious and skillful application of the tools of economic theory and economic history are in frequent evidence throughout. The generally tight writing and organization of the book reflect much perspiration by the author and positive professional inspiration of Shun-Hsin Chou and the numerous eminent individuals cited as having encouraged or assisted him.

There are eight substantive chapters. Following a brief introduction to the subject and consideration of "The Pattern of the Chinese Inflation," four chapters deal with inflationary pressures in the government, private, and international sectors. These deal with such familiar matters as government receipts and expenditures, private consumption and investment, foreign exchange and trade policy, foreign aid, the balance of payments, etc. The last three substantive chapters deal with "Bank Credit, Velocity, and Inflation," "The Redistributive Effects of the Chinese Inflation," and "A Comparison of the Chinese and Other Inflations," (Germany, Greece, and Hungary). The volume ends with an "Epilogue" and five appendices of a statistical and theoretical character.

Chou's conclusions resemble those of Chang Kia-ngau whose book on *The Inflationary Spiral, The Experience in China, 1939-1950* was published in 1958. This should not be entirely unexpected for Chou was associated with the Central Bank of China when it was under Chang's governorship, assisted Chang with his study, and in turn was assisted with his own book by some of the same economists and bankers who had contributed in various ways to Chang's. Chou finds that between 1937 and 1949 the increasing rates of increase in price and note issues reflected the ability of the inflation to perpetuate itself once the process was started. He also found, as one would expect, that the increasing amplitudes of the fluctuations in them were indicative of the inherent instability of the inflationary process; and also, not surprisingly, that the rising ratios of price changes to the increases in the note issues reflected the major role which the increase in the velocity of money circulation played in intensifying the inflation.

With the admitted advantage of hindsight, Chou states that the following monetary and fiscal measures would have contributed to the reduction of the intensity of the inflation in the short run: (1) budgeting of the government's outlay of domestic currency on a monthly basis rather than on an annual basis; and (2) the acquisition of the purchasing power required for the budgeted expenditures via tax collections, sales of government gold, sales of foreign exchange held by the government, sales of government properties, borrowing from the domestic market, and other "appropriate" measures. However, Chou concedes that while such a program would have contributed to reduction of the intensity of the inflation in the short run, it would not have offered a fundamental solution to the problem. The problem ultimately could have been solved, he says, only by a drastic expansion in China's production, the development of an effective means of increasing government revenue, and/or the curtailment of government activities. In the postwar years, he says,

"the Nationalist government had neither the strength nor the necessary environment effectively to pursue these objectives. Only peace . . . and an effective large scale foreign-aid program could have changed the situation. Up to 1949, neither was in existence" (p. 274).

This is a variation on a major theme in Chang's book to the effect that Nationalist China was a helpless victim of overwhelming circumstances; that had China somehow been able to halt the inflation, the Nationalists would not have collapsed in 1949, and ultimately not only would the Communists have been defeated but economic conditions would have been materially improved. Chang in his study puts it in stronger terms than does Chou: "Many historical forces contributed to the collapse of the Nationalist government . . . but the direct and immediate cause which overshadowed all other factors was undoubtedly the inflation" (p. 363). In my judgment, such views illustrate the extent to which excellent economists can get so engrossed with the trees of their subject that they are unable to recognize the rest of the forest. Specifically, Chou and Chang, as well as others, appear to have spent so much time analyzing the inflation as an influence on events and attitudes in China from 1937 to 1949 that they have not considered the extent to which the inflation was in turn a reflection and an influence of those same events and attitudes. In short, they do not seem to be as impressed as I am by the interaction of other economic and noneconomic factors on one hand and the inflation on the other.

In closing, it needs to be reaffirmed that, despite this difference of opinion, Chou's book is an excellent piece of work. Very competently done, it will be of great interest and use to economic historians, monetary and banking specialists, and China hands for many years to come.

SIDNEY KLEIN

*Rutgers, The State University*

### **Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit**

*A Study in Monetary Analysis.* By ASSAR LINDBECK. Stockholm: Almqvist & Wicksell, 1963. Pp. 306. SKr. 30.

Mr. Lindbeck's book can be described as consisting of three main sections, with the first two closely related and the third hovering over but not quite touching the other two. It is, in essence, an attempt to fuse the works of Patinkin, Bent Hansen, and Gurley-Shaw. On this account alone, it deserves attention as an ambitious work which was many years in the writing. It is meticulously, and at times tediously, thought out. It never quite gets off the ground. For the most part, it burrows deeply into the analytical complexities of its logical structure, pausing time and again to examine each particle of grain displaced along the way. And when the going gets rough, Lindbeck thoughtfully simplifies the model or examines only one or two of a seemingly infinite range of possibilities, leaving the rest to the reader's imagination or want of it. Despite the deadliness of its style, it is a book well worth reading.

and it should appeal both to the pure theorist and to the policy-oriented economist.

The first part of the book (Chapters 1 to 3) is concerned with some basic methodological problems and the microeconomic theories of the household and the firm. The influence of Patinkin is unmistakable and fully acknowledged. Indeed, Chapters 2 and 3 can be described as "Patinkin plus uncertainty." For example, in the theory of the household, using a two-period analysis, we are concerned with a household's decision to consume, save, hold various types of assets (money and government and private consols), and its choice between work and leisure. The analysis is, of course, based on preference theory and the absence of a money illusion. A preference function of 16 arguments is set up and, subject to a set of conventional budgetary restraints, the optimum position of the household is arrived at. Within this model, Lindbeck traces the effects of a change in the volume of asset holdings under conditions of uncertainty where the Keynesian precautionary and speculative motives for holding cash balances are specifically introduced. In Lindbeck's words, "The motives for holding assets are still stronger . . . if uncertainty is assumed not only for the *timing* of future payments and receipts, but also for the *values* of expectational entities—such as prices, wage rates, interest rates, and income" (p. 23). With this extension of the Patinkin model, liquidity effects are then introduced for a range of assets of different degrees of marketability. There are no surprises or startling conclusions in the chapter, but it does serve to clear the air and tidy things up a bit for the analysis to follow.

Chapter 3 applies the same approach to the theory of the firm. A preference function is set up with profits, cash balances, government securities, and debt as arguments. It is assumed that firms have an aversion for debt, with the firm seeking an optimum position with regard to profits and debt. The behavior function, therefore, has as its arguments relative prices, profit tax rates, and, in real terms, initial asset holdings (including debt), interest payments on assets and debts of the preceding period, and interest rates. In short, the traditional notion of profit maximization is expanded to include other goals as well. Within this format Lindbeck has a valuable analysis of the willingness of firms to borrow and the role of internal funds in decisions to invest. The firm, in other words, is assumed to attach a positive utility to profits and financial assets and a negative utility to debt, i.e., the firm will seek to realize its optimum position with respect to profits, liquidity, and debt. The supply of funds is represented as a step function as the firm moves along the curve from the use of its own internal funds (at an assumed opportunity cost) to alternate sources of credit at progressively higher interest rates. It is here that Lindbeck first introduces the distinction between credit availability and interest rate effects. This distinction plays an important role later in the book and a discussion of it will be postponed for the moment.

Having set up his meticulously constructed microeconomic models of the household and the firm, Lindbeck is now in the position to play games. But he is faced with a formidable problem in Chapter 4. He has 14 behavior

functions—five for the household and nine for the firm—with, all told, an incredible number of independent variables. Says Lindbeck, in a classic example of understatement, “A presentation of the properties of all these behavior functions by an analysis of the sign, and possibly the magnitude, of every partial derivative of each function would make extraordinarily boring reading” (p. 99). A much simplified model is then introduced with six markets (consumer and capital goods, money, government and private bonds, and labor), and two types of firms (“a consumer good producer that does not supply capital goods, and a capital good producer that does not supply consumer goods and does not demand (additional) capital goods”). Employing the technique of comparative statistics with the microbehavioral functions of the abbreviated model, a demand-supply analysis is used to gauge the direct effects of monetary and fiscal policy. Under direct effects, Lindbeck examines the impact of taxation on household income and on profits and the consequences of open-market operations. Autonomous changes in government and private behavior are also brought in to trace the channels through which the direct effects will flow. For the analysis of the indirect effects, the model is further simplified with the functions restated in aggregate terms. The resulting macromodel is then used to analyze the indirect effects of monetary and fiscal policy. The effects of certain parameter changes are considered and an attempt is made to determine, as far as possible, if a new equilibrium situation will be restored automatically. If the system proves to be dynamically unstable, then the role of government is brought in to counteract or eliminate the destabilizing factors. The parameter shifts examined are (1) a shift by consumers from cash balances to consumer goods, (2) a reduction of personal income taxes, (3) open-market purchases by the central bank, (4) a shift of the consumption function at the expense of either money balances or of lending, (5) an increase in the propensity to invest along similar lines, and (6) changes in productivity.

The most serious criticism that can be made of this chapter, and of the book as a whole, is the assumption that time lags in the policy variables simply do not exist. Lindbeck is much too absorbed with the mechanics of his general-equilibrium model to worry about the possibility that many of his policy variables may actually turn out to be perverse in their effects because of the lags involved. He is, of course, aware of this problem but dismisses it in one paragraph with the statement that “there is at present no generally accepted opinion about the length of these time lags, and about how the length varies between different business situations” (p. 167). One wonders if a lagged partial-equilibrium model would not have been more to the point.

Another shortcoming of Chapter 4 concerns Lindbeck's two definitions of an equilibrium situation: (1) where excess demand is zero in all markets of the model, and (2) where there is no tendency for prices to change. The second definition does not coincide with the first so long as zero excess demand is not a necessary and sufficient condition for price stability. Lindbeck's sympathies lie with the first definition, but as a concession to the “real” world he assumes that though prices may be sticky downwards, wage rates are

subject to a ratchet effect. It would have been more interesting, however, if a ratchet effect were assumed also for commodity prices and the second definition were then given its full due. In this event, the analysis of the role of government within the second definition would have been more relevant.

We turn now to the most important part of Lindbeck's book. Chapter 6 has to do with bank and nonbank financial intermediaries, and Chapter 7 with the availability of credit doctrine both in its European and U.S. versions. In Chapter 6, and as a welcome change after so much abuse, the tattered and battle-scarred colors of Gurley and Shaw flap briskly in Lindbeck's breeze. Much of this chapter is devoted to a careful and systematic analysis of the *credit* creation multiplier (as distinct from the *money* creation multiplier). Lindbeck sides with Gurley and defends him from his critics. He argues that nonbank financial intermediaries *are* "creators" of loanable funds and dismisses the arguments of Culbertson, Smith, and Aschheim. Perhaps an extended quote from Lindbeck would be in order (pp. 207-8).

(I)t has been (explicitly or implicitly) assumed . . . that the answer to this question is of relevance for the problem of the "desirability," from the point of view of the efficiency of monetary policy, of extending the system of cash reserve requirements to cover non-monetary intermediaries and not just commercial banks. The answer to the question who is a "creator of loanable funds" and who is not, depends, of course, on how creation of loanable funds is defined. In the traditional loanable funds theory, supply of loanable funds has been defined as saving + reduction in money balances ("dishoarding") + money creation. If "creation" of loanable funds is identified with "supply" of loanable funds, in the aforementioned sense, all types of behavior units can create loanable funds. . . . (However, a difference between commercial banks and non-monetary intermediaries, in the respect under discussion, is that the latter can create loanable funds only by dishoarding, whereas commercial banks can create loanable funds also by money creation. Some of the authors who have argued that non-monetary intermediaries cannot create loanable funds seem to have forgotten that intermediaries can dishoard, in the sense of increasing credit supply by reducing money balances.) Moreover when induced changes in liabilities of intermediaries are considered, even these kinds of credit institutes may, as we have seen, perform "multiple credit expansion," even though their liabilities are not usually used as a means of payment. Even in this sense, intermediaries can "create credit."

Within his model, Lindbeck considers the potentially destabilizing effects of intermediaries and their expansionary asset effect on the economy "(1) by shifts between money and private loans, (2) by shifts between government securities and private loans, and (3) by shifts between money and government securities." Next he considers control over the supply of *credit* via liquid-asset ratios, required cash reserves for intermediaries, and other selective controls. This chapter is well worked out and should serve to clear up many of the substantive and semantic confusions usually found in the treatment of financial intermediaries.

Chapter 7 is also well thought out and informative. Its discussion of recent

developments in monetary theory is excellent. Lindbeck begins by distinguishing between the U.S. and U.K.-Swedish versions of the availability of credit doctrine. The U.S. doctrine, as developed by Roosa and Sproul, emphasized the increased sensitivity of the financial sector to small interest rate changes and placed virtually all of its faith on the locking-in effect. Proponents of the availability of credit doctrine in the United Kingdom and Sweden, on the other hand, were more agnostic about the efficacy of interest rate changes and placed a greater reliance on liquid asset ratios, reserve requirements for intermediaries, and direct controls over the credit advances of all types of credit institutions. In short, the U.S. version of the availability of credit doctrine favored open market operations and discount rate policy to control the supply of *money* whereas most European adherents leaned more toward control over the supply of *credit* with a mix of direct as well as indirect financial controls. In effect, the difference between the two versions turned on the relative strength of the so-called locking-in effect. Concern in Europe was with the *composition* as well as the *volume* of loanable funds and this led them to "a much more interventionist political philosophy" than the Federal Reserve Bank of New York (not to mention the more conservative Board of Governors) would ever be willing to entertain.

By far the most valuable part of Chapter 7 is Lindbeck's distinction between the interest rate and availability of credit effects. From the lender's point of view, limitations in the availability of credit are explained in terms of the stickiness of interest rates because of (1) institutional rigidities, (2) good-will considerations, and (3) personal discrimination.<sup>1</sup> Two subcategories are developed where the supply of credit curve is of the conventional type and another where credit risks become so great that the supply of credit becomes perfectly inelastic. Lindbeck then distinguishes between a restriction in the availability of credit because of credit rationing (i.e., the amount by which demand exceeds supply) and a change in credit standards involving a leftward shift of the supply of credit curve. The former is largely a matter of personal discrimination whereas the latter is compatible with the allocation of credit via the market mechanism. From the borrower's point of view a step function of borrowed funds is introduced to describe the alternate sources of credit available to a borrower at increasingly higher interest rates. A tight-money policy results in an upward shift of the supply curve with the horizontal shortening of each step representing the decrease in credit availability (the credit availability effect) and the vertical displacement of

<sup>1</sup> In the United States a fourth reason can be given for the stickiness of interest rates, namely, *political*. The public debt and the size of consumer instalment debt would be the complicating factors. If 4 per cent can raise the ire of Congressman Wright Patman, 7 or 8 per cent should bring down the entire Congress! Lindbeck comes close to arguing in favor of an interest rate *ceiling*. Coupled with the familiar idea of an interest rate *floor*, the interest-rate-ceiling concept could be used to counter those arguments that talk about a theoretical or "effective" interest rate which would do the job. If the effective interest rate lay outside the institutionally feasible range of interest rate variation, owing to some insensitivity to small interest rate changes, the discussion and disagreements concerning the effectiveness of conventional monetary policy would be more meaningful and considerably more to the point. It is strange that this notion of an interest rate ceiling has been in large part ignored.

the supply curve representing the interest rate effect. Lindbeck, contrary to some writers on this subject, holds the view that both effects must be considered simultaneously. Whether or not one rather than the other predominates depends on whether a borrower is limited in the number of alternate credit sources available to him and whether he is already standing on the highest step with nowhere else to go. The remainder of the chapter explores selective credit controls and the arguments against large interest rate variations—most of which Lindbeck dismisses as exaggerated except possibly for the discriminatory impact of monetary policy on the smaller economic units, the effect of gross monetary policy measures on the distribution of income, and the possible effects of high interest rates on the rate of economic growth.

Chapters 6 and 7 make up well over 40 per cent of the book and are most probably an expansion of a much-mentioned earlier monograph by the author.<sup>2</sup> They have, however, a rather slight connection with the theoretical core of the book. Lindbeck has not, in my view, succeeded in playing the role of an eclectic by synthesizing Patinkin, Bent Hansen, and Gurley-Shaw—particularly with regard to the last. Yet it is a book that deserves to be taken seriously both from the theoretical and policy points of view.

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<sup>2</sup> Assar Lindbeck, *The "New" Theory of Credit Control in the United States*, 2nd ed., Stockholm Economic Studies, pamphlet series no. 1, Stockholm, 1962.

*Monetary Management*. Prepared for the Commission on Money and Credit.

By FRANK M. TAMAGNA, WARREN L. SMITH, CLARK WARBURTON, MICHAEL D. REAGAN, CHARLES P. KINDLEBERGER, AND ROBERT Z. ALIBER. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. 472. \$6.50.

In 1961 the Commission on Money and Credit published its now familiar recommendations concerning U.S. "monetary, credit, and fiscal policies and the instruments and institutions through which they operate." In forming its views the Commission was aided by a research staff and by a large number of studies, reports, and working papers prepared by various trade associations, by the Federal Reserve and the Treasury, and by individual scholars. In 1962 the Commission began to publish selected reports and studies from among those prepared for it. *Monetary Management*, the first collection of studies by individual scholars to appear (in 1963), contains six contributions. Two of these approach book length; the other four are articles or working papers.

Frank M. Tamagna's 174-page study, "Processes and Instruments of Monetary Policy: A Comparative Analysis," discusses monetary policy formulation and implementation with particular attention to the functions, powers, and influence of foreign central banks. The discussion is arranged by functional topics and draws upon experience in most countries of Western Europe as well as in Argentina, Australia, Japan, New Mexico, and New Zealand. According to the author the study is based on a variety of sources such as laws, statutes and regulations, central bank ordinances, instructions and letters, annual reports, monthly bulletins, speeches, special reports and studies prepared on the occasion of conferences and committee considerations, on

a perusal of general bibliography in the field, and on personal experience with central banking in various countries.

This study is a rich source of information on central bank policies and practice viewed against the backdrop of diverse financial institutions and markets in different countries. Tamagna points out some common themes in this diversity: the dependence of central bank power on the competence, personal authority, and tenure of officials and staff rather than upon legalistic rights, the need for cooperation and coordination between central bank and Treasury operations, and the growing influence of central bankers in national monetary and fiscal affairs in the decade of the 1950's. On the side of diversity are the many variations of practice in open market operations, discount policy, and the use of reserve requirements, as well as variations in responsibility of the central bank for selective credit controls, in the effectiveness of moral suasion, and in central bank regulation of nonbank financial institutions. On this last point Tamagna offers the important observation that any definition of "banking" and "nonbanking" institutions may be highly conditional on particular institutional arrangements and customs and therefore meaningful only for particular countries at a particular time.

Tamagna has not been fully successful in organizing his formidable mass of primary material and giving it focus. His study is almost exclusively a descriptive catalogue of practice. He offers little analysis of the potential application of foreign practice to U.S. problems and makes no recommendation for such application. Thus his study lacks direct relevance to the U.S. scene. As a source for the student of foreign central banking practice, this study can be recommended as a useful survey, but it is too compressed, too descriptive, and possibly inappropriately organized (by topic rather than country) to serve the needs of more scholarly investigation. This shortcoming is reinforced by the complete absence of bibliography and footnote citation.

Warren L. Smith's discussion of "Reserve Requirements in the American Monetary System" is the other major study (141 pp.) contained in the volume. In a clear and forceful style Smith examines a surprising range of topics which turn out to relate to reserve requirements. This range can be indicated by citing some of Smith's conclusions. Traditional reserve requirements against demand deposits should be identical for all banks. On balance the velocity reserve plan is undesirable. Changes in the level of reserve requirements as a countercyclical measure are likely to be awkward and inflexible and thus inferior to open market operations. It might be desirable to take away from the Board of Governors the power to change reserve requirements in normal times. The *level* of reserve requirements has implications with respect to the structure and distribution of government (and private) debt and, in particular, to the proportion of government debt held by commercial banks and the central bank with the money supply held constant. This in turn influences significantly both bank earnings and the net interest cost of the government debt to the Treasury (allowing for the return of interest earnings to the Treasury by the Federal Reserve).

Smith examines a variety of secondary (government security or cash) reserve plans. After noting the problems of transition to such plans he con-

cludes "... one might guess that a requirement amounting to 25 or 30 per cent of demand deposits would produce a substantial stiffening of the impact of monetary policy on bank loans" (p. 271). He analyzes the implications of 100 per cent reserves and doubts that they would improve on the effectiveness of present arrangements for credit control. He is skeptical concerning the need for reserve requirements on time deposits and the liabilities of nonbank financial intermediaries for the purpose of monetary stabilization. He emphasizes that savings banks and savings and loan associations would be deprived of their intermediary function if required to meet reserve requirements identical to those for member-bank demand deposits. He would eliminate reserve requirements against time and savings deposits in commercial banks.

Smith's analysis is supported by a large variety of carefully constructed exercises in the algebra of reserve-deposit multipliers and by illustrative numerical projections of the implications of various reserve plans for such matters as the asset structure of the banking system, bank earnings, and the like. His contribution lies less in the novelty of his material than in the effective manner in which he combines established theory, empirical information, and professional judgment to illuminate policy issues and reach specific and unambiguous recommendations. In the art of applying economic analysis to issues in monetary policy Smith has few peers in the profession today.

Four article-length essays complete the volume. Clark Warburton examines "Nonmember Banks and the Effectiveness of Monetary Policy." He finds the behavior of nonmember banks similar to that of country member banks, which they resemble in size and economic environment. For this reason and because of their relative unimportance in the aggregate of bank assets, Warburton concludes that they do not presently hamper the effectiveness of monetary policy. Should they come to constitute an impediment to monetary policy, Warburton would, in effect, require them to become members.

In "The Internal Structure of the Federal Reserve: A Political Analysis" Michael D. Reagan scrutinizes the organizational structure of the System to assess its implications for such matters as goal formation, interest-group representation in the decision process, and the consistency of its organization with the public (Reagan means "political") nature of the System's functions. Reagan's discussion is fresh and stimulating. He regards the present organization as anachronistic in relation to the public responsibilities that evolution has brought the Board. He favors a more centralized organization, more directly responsible to the President of the United States, and less identified in form (there is no substantive issue) with commercial banking interests.

In one of the remaining papers Charles P. Kindleberger reviews the pros and cons of flexible exchange rates and recommends against a flexible dollar rate on the grounds that "there would be a loss for the world international monetary mechanism." In the other, Robert Z. Aliber discusses "The Adequacy of International Liquidity" and evaluates various proposals to increase it.

Since its appearance in 1961 the *Report* of the Commission on Money and Credit has sometimes been criticized for lack of scholarly originality and

for possible failure by the Commission to rely sufficiently on the professional studies supplied to it by its staff and consultants. On the basis of the observable relationship between the essays in *Monetary Management* and Commission recommendations in relevant areas, the second of these criticisms appears unwarranted. Study of the Commission *Report* reveals substantial correlation between these professional studies and Commission recommendations on such matters as organization and powers of the Federal Reserve System, extension of System membership to nonmember banks, policy toward controls over financial intermediaries, reserve requirements, preservation of the fixed-dollar price of gold, and the development of new forms of international cooperation to meet the growing need for international liquidity. Moreover, the Commission *Report* reflects adequately the spirit of these particular background studies which are more in the nature of policy-oriented applications of accepted professional concepts and available information than they are pioneering studies in the realm of basic social science research. The objective of the Commission was to bring professional competence and experienced judgment to bear on monetary and credit issues and not to sponsor original and basic research. On the evidence of these essays the Commission kept faith with the professional advice which it received.

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*Federal Credit Programs. A Series of Research Studies Prepared for the Commission on Money and Credit.* By STEWART JOHNSON AND OTHERS. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. viii, 614. \$6.50.

The 1961 *Report of the Commission on Money and Credit* has generally drawn faint praise in the economic journals, primarily for failing to bring forth any bold new monetary-fiscal concept or institutional proposal. Whatever the merits of this criticism, economists have tended to overlook the pioneering work done by the CMC in the fields of federal credit programs and public regulation of private financial institutions. Here the CMC espoused new approaches for delimiting federal credit operations and for restructuring U.S. financial markets. Its recommendations of principles to guide the federal government's credit activities and to lighten the incubus of governmental regulation of private financial firms so that they would engage in wider interinstitutional competition are of great potential importance.

The volume on *Federal Credit Programs* contains seven research reports that, along with those in a companion volume *Federal Credit Agencies*, helped to shape the CMC philosophy of the role of such programs in the economy. In the present volume Stewart Johnson provides a valuable compilation of facts and statistics on all federal lending and loan insurance programs over the period 1929 through 1958. Warren A. Law presents a study of the aggregate impact of such programs on the entire economy. James W. McKie offers a thoughtful essay on criteria for federal credit assistance, including a treatment of "credit gaps." D. Gale Johnson analyzes the role of federal credit agencies in the agricultural sector. James Gillies performs the same task

for the housing sector. Robert C. Turner and Ross M. Robertson study the sources of funds available to federal lending agencies. J. Fred Weston analyzes the technical components of the programs, including credit standards, interest rates, costs, and nonprice terms.

Each of these studies offers new analysis and fresh insight into its subject. Because a brief review must of necessity be selective, I shall comment only upon the two essays of McKie and Law, which are probably of most general interest to economists because they deal, respectively, with the basic principles justifying federal credit assistance and with the aggregative economic effects of such assistance.

McKie investigated the meaning of "credit gaps," the filling of which has been the primary reason offered for the entry of the federal government into the business of supplying credit. Such gaps exist only in markets which are imperfect by reason of poor organization, ignorance, or ineffective competition (often due to perverse public regulation). They give rise to differences in costs of funds to users greater than can be attributed to differences in risks, administrative costs, or liquidity. True "credit gaps" are investment opportunities whose prospective private benefits would exceed their costs but for the aforesaid barriers. They are to be differentiated from situations in which alleged social benefits exceed private benefits, which are candidates for public subsidies.

Government can properly step in to fill a credit gap when it can realize special economies of scale or can shoulder the burden of surmounting threshold barriers to entry which would endanger the survival of a private enterprise before it could break through to full-scale economies of operation. When government detects a true credit gap—and they are rarer than commonly believed—it can engage in direct lending, lend through an intermediary, insure or guarantee private loans, or institute antitrust or other regulation to stimulate private lending. Direct lending is usually indicated when it is complementary to some existing federal activity; indirect lending when the gap is caused by a threshold barrier; loan insurance when large numbers of similar risks can be economically aggregated; and regulation where competition is ineffective but can be made to work.

McKie's analytical framework is useful in guiding federal action, although more than one of these conditions is present in many credit markets, so that his criteria do not point unambiguously to a single choice. He is premature in branding the Small Business Investment Company—one of the few financial innovations of the postwar era—as unsuccessful in closing the small-business capital financing gap.

Law's careful study of the aggregate impacts of federal credit programs on the economy summarizes extensive recent literature and offers original analysis. It extends and in some respects conceptually improves the pioneering analysis of Saulnier, Halcrow, and Jacoby (hereafter cited as S-H-J).<sup>1</sup> Law was unable to make much progress with such thorny problems in measuring economic impacts as the choice between gross and net flows of federal credit

<sup>1</sup> *Federal Lending and Loan Insurance* (Princeton: Princeton University Press, 1958).

or between direct loans and loan insurance. He defines federal credit simply as the total of direct loans disbursed plus the federally insured or guaranteed parts of private loans. In theory, federal credit makes a real net addition to the GNP only if it generates new income and is not a transfer payment, does not replace private credit, and is derived from funds that would not otherwise have been spent on final products. In practice, it is impossible to determine whether these conditions are met. Law cut the Gordian knot by assuming arbitrarily that *all* federal credit was income-generating and not refinancing of existing debt, the entire net credit flow constituted a net addition to GNP, and funds injected into federal programs by the Treasury were not diverted from potential borrowers so long as there were excess reserves in the banking system. The last assumption is especially heroic; but it is difficult to formulate a better one.

On the key issue of impact of federal housing credit programs upon the postwar supply and price of housing, Law sides with Break who held that the primary effect was an increase in real supply. S-H-J argued to the contrary, that it mainly served to increase housing costs and prices. Clearly, both effects were necessarily present; the issue of their relative importance will never be finally settled.

In considering whether federal credit programs contributed to economic stability, the author rejects the criterion applied by S-H-J, who observed whether the flow of credit increased in years when GNP declined and vice versa (also counting as countercyclical an increase during an initial year of business revival). He used the alternative—and probably superior—criterion of credit increase in years when unemployment exceeded 5 per cent and credit decrease in years when it fell below this level. However, his finding of countercyclical influence in 16 of the 29 years in the period 1929-58 was not materially different from the S-H-J finding of a 15/24 ratio of successes during 1929-53. Both studies confirm the conclusion that any contribution of federal credit programs to economic stability—either at full or less than full employment—has been slight and accidental, and that their postwar influence has been consistently inflationary. The author takes an unwarrantedly dim view of the possibilities of improving the countercyclical effects of credit programs, however, particularly in view of the possibilities of countercyclical shifts in *sources* of funds. Heretofore, there has not been a deliberate “federal credit policy”—only a series of unrelated programs. An agency is needed to coordinate all federal credit programs and to correlate their actions with those of the Federal Reserve and Treasury.

Law found substantial elements of subsidy in the programs, many of which failed to cover costs of funds to the Treasury, losses, administrative costs, and appropriate premiums for risk. (He applied the Krutilla-Eckstein “opportunity cost” criterion of the social cost of federal loans, which was 5-6 per cent per annum during 1955 versus an average market rate paid by the Treasury of 2.85 per cent.) However, some programs did stimulate competition or eliminate divergencies between private and social returns on investment, which justified some subsidization. Federal credit—particularly guarantees—can also be used as a substitute for federal spending. With the benefit of hind-

sight, one can see that a wider application of this form of assistance would have been desirable in the U.S. foreign economic aid programs and in other spheres.

Were these painstaking studies used by the CMC in reaching the recommendations contained in its *Report*? Having served as a member of the Research Advisory Board of the CMC, which mediated between the Commission and its professional researchers, this reviewer would answer in the affirmative. A reading of Chapter 7 of the CMC *Report* clearly reveals a conceptual framework that has much in common with that described above, and the "seven broad guides" put forth by the CMC embody many of the advisers' ideas. In the field of federal credit there was an effective marriage of academic theory and practical experience, although this may not have been true of all aspects of the CMC's work.

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*Asset Prices in Economic Analysis.* By SAMUEL B. CHASE, JR. Berkeley and Los Angeles: University of California Press, 1963. Pp. xii, 153. \$5.00.

*Asset Prices in Economic Analysis* is three things, and, hence, must be judged by a triple standard. It is a doctoral dissertation, a treatise on the theory of finance, and a book written in good English. It deserves to be widely read as a fine example of each of these.

It is a brief doctoral dissertation presenting a clear argument in less than 125 pages of text (if allowance is made for the diagrams). It is an exercise in pure logic rather than in computerology. As such, it might well be made compulsory reading for all Ph.D. candidates in economics who have reached the stage where they must choose their dissertation subject and decide on its form.

The theory of finance has attracted the attention of many writers. Despite this widespread interest, one economist's description of one set of ideas might well be applied to the general body of financial analysis. It is "woefully inadequate, unverified, and incapable of bearing the heavy burden that is placed upon it."<sup>1</sup> It is easy to account for this sorry state of our theory. With a few notable exceptions (e.g., Boulding, Goldsmith, and, to a lesser extent, Gurley and Shaw), most writers have centered their attention on relatively limited areas of the economy's asset and liability structure. A synthesis of our views into a conspectus of the role of balance sheets as determinants of economic decisions is now required. Samuel B. Chase, Jr., attempts a conspectus of the structure of assets, and one must agree with him that although his "analysis is highly abstract," it "provides a useful framework for analyzing government financial policies, including central banking, debt management, taxation and budget policy."

He starts from the proposition that "People like to hold assets." However, whereas most analyses move from a discussion of the demand for money to a consideration of the demand for "liquid assets" and on through a range of

<sup>1</sup> Allan H. Meltzer, Testimony to the U.S. House Banking and Currency Committee, as reported in the *New York Times*, Feb. 12, 1964, p. 45.

assets to the demand for and supply of physical assets, Chase proceeds directly to a discussion of the problems associated with physical assets. That is, he starts from a discussion of assets available under conditions of increasing cost in terms of consumption foregone. From this point, he proceeds to a discussion of the factors affecting the price of assets and of the effects of these prices on the demand for and supply of both assets and consumption goods, while always relating these conditions to the demand for money.

Perhaps the outstanding contribution of this work is the discussion which runs through it of the relation between the price, eventually in terms of consumption goods, of a particular asset or group of assets and an individual's wealth. The "wealth effect" of a change in asset prices is a major determinant of the demand for assets. That is, he deals extensively with the knotty problems of unrealized capital gains and losses as determinants of investment and consumption demands.

In general, the analysis is abstract and schematic rather than specific. It deals with the conditions which will determine an individual's distribution of his total wealth among different types of asset, usually in terms of a distribution among real assets, whose prospective prices and earnings are uncertain to different degrees. It is not difficult to transpose these conditions into a description of the factors determining the demands for several types of real asset and different types of financial asset. However, here Chase provides guides which the statistician may use, rather than specific prescriptions.

The abstract quality of the analysis becomes strikingly evident towards the end of the book when the effect of a change in the supply of one type of financial asset—government bonds—on asset and consumption markets is considered. The factors that will determine the relative prices of assets and consumption goods are specified but, in the absence of specific data on the cross elasticities of demand for consumption goods, physical assets, bonds, and cash, an indeterminate conclusion is reached. Yet the discussion of government debt operations serves to outline the fundamental differences between those who believe that "money matters" and those who believe that it is merely a veil. In his final pages, Chase provides a skeletal outline of his approach to fiscal theory and compares it with a simplified Keynesian approach. The essential difference between the two views of the role of a government in the economy is that the Keynesians limit their attention to current flows, ignoring stock effects, while Chase reminds us that any government deficit or surplus permanently changes the community's holdings of bonds and money. These changes alter the community's money wealth and hence shift all future demand curves for assets and consumption.

At this point in the development of our financial theory, a discussion of asset prices at a high level of abstraction is a valuable contribution; but it is not a real conspectus of the role of balance sheets as determinants of economic decisions. It is noticeable that the words "balance sheet" and "liabilities" are missing from the index. In the first place, Chase probably overstates the importance of an analysis limited to assets. Every individual and firm has an inherent asset in the form of borrowing power. If one accepts this view, the issue of liabilities can be treated as the sale of part of this borrowing power. In the abstract, this view is acceptable, yet, if the argument is too

abstract, it may provide misleading guides to policy. A comparative study of "Liabilities in Economic Analysis" might well contribute to our progress towards a proper theory of finance.

In the second place, Chase too readily accepts the view that inherited wealth plus current income provide a homogeneous pool which may be distributed between consumption and investment, with unlimited possibilities to change the composition of asset holdings. Just as "people like to hold assets," they like to keep their assets intact, even in the face of income and price changes which would suggest their dissipation. Further, they like to leave the structure of their assets intact even when shifts in relative prices suggest that they should change the composition of their holdings. Similarly, a sudden rise in incomes will lead quite rationally to splurging which would seem inappropriate on the basis of abstract analysis. These apparently irrational rational forces are among the facts of life which warrant investigation.

In the third place, as Chase states in his preface, "a perfectly competitive pricing system is postulated." He deals only briefly with trading costs and the effects of imperfect markets. In particular, it is, in effect, assumed that borrowing and lending rates of interest are equal. Our world might well be a better place if it were perfectly competitive. Unfortunately, it is not.

These intrusions from "the real world" may well be irrelevant comments. Chase does not pretend to provide a conspectus of financial theory. He has attempted a limited, highly abstract essay. In doing so, he has provided valuable guides which should help to lead us out of our current confused state and towards a satisfactory synthesis of our views.

As a book, this work is a delight to read. In many respects, it moves like a good novel rather than a monograph. At numerous points, the reader is tempted to think "But Chase has forgotten . . .," only to find the point in question discussed on the next page. For those of us who yearn for a return to the literary elegance of the nineteenth century rather than the computer print-outs that pass for economic literature today, many of Chase's sentences are limpid oases. Finally, the University of California Press is to be congratulated for presenting this pleasant essay in a pleasing format.

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*From Marshall to Keynes—An Essay on the Monetary Theory of the Cambridge School.* By EPRIME ESHAG. Oxford: Basil Blackwell, 1963. Pp. xxiv, 144. 21s.

A principal aim of this essay on the monetary theory of the Cambridge school is to explain and critically evaluate Alfred Marshall's contribution to monetary theory and policy. In carrying out this task, Mr. Eshag compares Marshall's views with those of the classical economists, with the views of his contemporary neoclassical writers, and with the views of those who followed him at Cambridge. This volume therefore provides both a very broad overview of the development of monetary theory since Ricardo and a more detailed history of monetary theory as it developed in Cambridge from Marshall to Keynes.

The five chapters in this volume take up in turn Marshall's contribution

to monetary theory and policy. The first three chapters are devoted to monetary theory and summarize the analysis of the laws that determine the internal value of money, the external value of money (the theory of exchanges), and the theory of interest. The last two chapters on monetary policy deal with the role of money in the economy including the influence of price changes on trade and production, the analysis of the credit cycle, and the use of alternative monetary instruments to achieve given policy objectives. Eshag's presentation points up the significance of Marshall's views by comparing them with those of his predecessors and contemporaries and by tracing the impact of Marshall's work on the development of monetary theory at Cambridge.

One of the most interesting findings that emerges from Eshag's thorough study of the literature is that, although Marshall contributed many important refinements to classical monetary theory, he did not succeed in constructing a monetary theory capable of explaining fluctuations in income and employment. In large measure Marshall's failure resulted from his primary concern with long-run statics and from his propensity to employ a long-run analysis with an implicit assumption of full employment; he abandoned this framework (emphasizing long-run statics) only when dealing with what he took to be the exceptional case of financial crisis. Thus, even when Marshall turned to short-run problems he analyzed the "credit cycle" but never really attempted to explain the phenomenon of the "trade cycle" or of persistent unemployment. In consequence, his short-run analysis, perhaps too heavily influenced by his preoccupation with the long run, concerned itself with the adverse effects of rapid price changes. Although Marshall does occasionally mention possible changes in output and employment, his analysis of the "credit cycle" focuses primarily on speculative activities induced by changes in the price level; and the greatest evil of these cycles is the financial crisis which develops when credit is contracted at the turning point of a speculative peak and which, in turn, disorganizes trade and brings *temporary* hardship to both entrepreneurs and workers. Marshall did not develop a theory that explains the determinants of aggregate demand and the effect of changes in aggregate demand on employment and output. Eshag's main conclusion is that Marshall's framework was inadequate, his tools ineffective, and his conclusions often misleading. In fairness to Marshall it should be mentioned that a monetary theory based on the analysis of credit cycles was perhaps less inappropriate in the years prior to the 1880's than, say, in the 1930's.

The major contribution of Marshall's successors was to focus monetary theory on short-run problems, to remove the assumption of full employment implicit in the long-run analysis and, most important, to develop an analytical structure that could explain short-run or cyclical fluctuations in employment and output. The monetary problems in Europe during and after World War I resulted from the abandonment of the gold standard, from currency inflations, from the loss of foreign gold reserves (to the United States), and from the loss of productive capacity. And after the Great Depression of the early 1930's, unemployment became the overriding question of policy. Pigou, Lav-

ington, Keynes, Hawtrey, and Robertson, the Cambridge writers considered in this book, made significant contributions in extending the Marshallian analysis and in developing a Cambridge monetary theory in the course of their work on these problems. But the most important innovation was, of course, Keynes' *General Theory* which explicitly considered the possibility of underemployment equilibrium in "normal" (noncrisis) conditions and which laid the basis for the development of a macroeconomic theory in which monetary changes could affect the level of employment and output.

Keynes was primarily interested in developing a theory of employment and a monetary theory of the "trade cycle," and he was not concerned with the analysis of the "credit cycle." Perhaps to emphasize this shift in objectives he felt it necessary to differentiate his product so sharply from Marshall's, in spite of the fact that his method of analysis is Marshallian and many of the ideas he used in the *General Theory* may be found in Marshall. Thus, as Eshag points out in Chapter 4, liquidity preference, the marginal efficiency of capital, and the propensity to consume, three of the important building blocks of the Keynesian savings-investment analysis, have a close affinity to Marshallian propositions. Liquidity preference is related to Marshall's analysis of the "balancing of advantages" in holding money. The marginal efficiency of capital is related to Marshall's analysis of "quasi-rent" and the importance he attached to "confidence" as a determinant of business investment. The propensity to consume and multiplier analysis may be traced to Marshallian views (admittedly the connection here is much weaker) on the "interdependence of firms" and to Marshall's analysis of cumulative processes of expansions and contractions. And even the notion of changes in "effective demand" may be found in rudimentary form in Marshall.

Considering the extent to which the individual building blocks of his analysis were owed to the writings of his predecessors, Keynes appears to be overstating his case against the classical and neoclassical economists when he writes that "the great puzzle of effective demand with which Malthus had wrestled vanished from economic literature." On the other hand, while many of the specific ideas used by Keynes may be traced to Marshall and others, the *General Theory* did constitute a revolutionary departure from received doctrine in developing a theory of aggregate demand, a theory of income determination, a simple aggregative model of the economy, and a general theory of underemployment equilibrium. It is for this reason that Eshag concludes that:

... he [Keynes] was nevertheless, essentially right in claiming that the classical and neo-classical economists, including Marshall, had generally neglected the function of aggregate demand and had built their analysis on the general assumptions of the equality of aggregate demand and supply and of the full employment of labor and equipment in normal (non-crisis) conditions (p. 106).

This is a work of careful scholarship and should be extremely useful to those interested in the history of monetary theory. It contains an excellent exposition of Marshall's monetary theory and traces in detail the evolution

of the post-Marshallian ideas from which Keynes' *General Theory* emerged. It suffers from two minor, though irritating, blemishes: Occasional statements by the author suggest that he is not familiar with the most recent literature of monetary economics (this may be due to the fact that the research for this volume was done some time ago); and the author occasionally allows himself the luxury of stating a personal opinion as if it were an obvious fact. Fortunately, these obiter dicta occur very infrequently and do not detract from the basic merits of the book.

DAVID I. FAND

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*The Economics of the Credit Union.* By JOHN T. CROTEAU. Detroit: Wayne State University Press, 1963. Pp. xx, 182. \$5.95.

This book is based on studies by the author of various aspects of credit union activity that were commissioned by the National Credit Union Management Conference and presented and discussed at their annual meetings from 1957 through 1962. These studies are the basis for Chapters 2 through 7, which deal with credit union reserves, liquidity, growth, investment yields, and efficiency. The author has added an opening chapter on "The Economic Dimension of Credit Unions" and a final chapter summarizing the conclusions drawn from the studies.

There is much useful and interesting material in this volume, at least from the standpoint of one who is relatively unfamiliar with credit union activity. Chapter 1 raises some interesting theoretical questions. After providing a concise and readable summary of what credit unions do and the legal and institutional constraints under which they operate, John T. Croteau raises the issue of how economic analysis can be applied to the credit union. Here he stresses the cooperative nature of the credit union, which means it is not a profit-maximizing firm, but one "which exists only to further the economic and social aims of its membership . . ." (p. 6). But credit union members include "savers" (those whose borrowings are small relative to their shares) and "borrowers" (those whose share holdings are small relative to their borrowing). The credit union, then, if it is to be an extension of the interests of the membership, may contain the seeds of internal conflict—"savers" want high dividends, which imply high lending rates, and "borrowers" want low borrowing rates, which imply low dividend rates. Croteau points up this problem but does not attempt to resolve it, except to counsel "reasonable compromise (to) arrive at an acceptable course of action" (p. 10). It would seem that economic theory could play some role in analysis of this problem. The credit union (or its directors) may be thought of as elected officials, seeking to choose that combination of dividend rates and interest rates which will maximize the social welfare function of the members. The application of welfare criteria might or might not be fruitful for this problem, but it holds possibilities. As a practical matter Croteau suggests that the conflict is probably won by "savers," since directors and committee members are usually severely

restricted in their borrowing from the credit union. This may lead to restrictions by the directors on the growth of the credit union, since as the credit union grows it becomes more difficult to maintain the ratio of high-yielding personal loans to total assets, and earnings rates fall. This may be accomplished, Croteau suggests, by share limitations or setting aside a high proportion of earnings in the form of reserves and keeping the dividend rate low.

Croteau notes that the "dividend" to members is higher than the stated rate, since most credit unions provide life-savings insurance up to some maximum. This means the credit union has less cash to distribute as a dividend (the insurance is treated as an expense), but it really provides a tax-free dividend in the form of the insurance premiums paid for the member.

In the detailed studies of credit union policies in Chapters 2-7, Croteau does a workmanlike job in developing several arguments with the use of data drawn from 100-300 large credit unions. He suggests in Chapter 2 that approximately three-fourths of the annual addition to reserves for bad loans of these credit unions is a capital reserve since it does not reflect loss experience, and that this may be a way of obtaining interest-free capital and hiding it from members. He argues that legal bad-loan reserve requirements of credit unions provide ample protection against bad losses, and that any excess should be used to increase dividends. In discussing credit union liquidity he develops the argument that the rapid recent growth of credit unions has obscured the cyclical and seasonal liquidity problem, and that should it slow down, more attention will have to be given to individual credit union liquidity. The chapter on "Credit Union Growth" suggests the trend in the growth pattern of credit unions is following a Gompertz curve, with a declining rate of growth, which, however, seems a bit daring as a projection since it is based on annual data for a limited period (1952-57).

Croteau then looks at the growth and balance-sheet characteristics of 175 large credit unions classified by type of credit union, i.e., as to the type of "common bond" (of employment or residence). He finds that the growth of central credit unions was much higher over the period (1954-58) considered than that of other types. Service types of credit unions also had rates of growth well above the average, while credit unions in the "manufacturing and processing" category had rates of growth considerably below the average. The effect of the business cycle is evident in year-to-year growth of certain types of credit unions; e.g., credit unions in steel and auto industries show absolute declines or very small growth in the recession year 1958.

The chapter on "Financial Efficiency in Large Credit Unions" deserves mention. He uses data for large credit unions to show (1) that there are apparently economies of scale in operating expenses among credit unions; (2) dividend payments as a percentage of net earnings tend to rise with the size of the credit union; and (3) net income from operations per \$1000 of assets *falls* as size increases. The third result stems from the fact that the expense economies of large size are evidently more than offset by a lower ratio of personal loans to earning assets; larger credit unions apparently have had diffi-

culty in placing a large enough proportion of their assets in high-yielding personal loans.

The Croteau book can be read with profit by anyone interested in the problems and characteristics of the credit union.

DAVID J. OTT

*Southern Methodist University*

*La banca central en América Latina.* By FRANK TAMAGNA. Mexico City: Centro de Estudios Monetarios Latinoamericanos, 1963. Pp. 603. \$5.00.

This long-awaited work is a curious performance. The position of its author would seem to assure a normal product of U.S. scholarship, with the minor foible that its first publication was in Spanish under the aegis of an organization of Latin American central banks. Instead, the reader finds himself going through almost 400 pages of description and analysis without a single footnote to identify anything, and ten voluminous tables without any indication of the source of their data. No bibliography is to be found in the book, and no index is provided. (No index in the U.S. sense, that is; there is an "Índice general," but that is just a table of contents.) It is apparent that this volume has little in common with what the author would have published in, say, an American university press. The nature and extent of the difference must be sought in the book's history.

The idea of publishing a general study of Latin American central banking originated with the founding of the Center of Latin American Monetary Studies (CEMLA) some ten years ago. Financial support was obtained from the Agency for International Development, and Professor Frank Tamagna of American University was chosen to make the study. He began work in 1958, utilizing the facilities of CEMLA and making personal visits to the countries involved. A draft of the preliminary study was then circulated to the various central banks and was also gone over in detail by a distinguished committee composed of Ignacio Copote Lizarralde, Ernesto Fernández Hurtado, George Garvy, Manuel Noriega Morales, Louis Rasminsky, Javier Márquez, and Alberto Ferrari. The corrections, suggestions, and clarifications put forward by the banks and the special committee were then incorporated in the final draft, with the active participation of the CEMLA staff. This way of putting the book together may account for its resemblance in format to the *Report of the Commission on Money and Credit*, though even that report contained a few footnotes.

The committee approach involves certain difficulties. To take just one example, this is what is said about the imposition of limits on the growth of finance companies in Mexico:

Recently the uneasiness about excessive liquidity in the market, in consequence of the attraction exerted for short-term funds by financial institutions other than banks, led the Banco de México to meet the problem in another way, limiting monthly expansion of short-term debt by way of guarantees extended by finance companies and banks on their own bonds and mortgage certificates in order to transfer the resources and operations to a medium-term market (p. 148).

As it stands, the paragraph is, of course, meaningless. Apparently the original thought was to describe the growth limit of 1 per cent a month which the Banco de México imposed in 1960 on finance companies' term deposits. Then someone wanted to add a note about banks (which are not subject to that growth limit). The references to bonds and mortgage certificates seem to be the remnants of a longer passage discussing the desire of the central bank to stimulate the development of a capital market. The committee approach has its hazards.

The book is organized into ten chapters, each followed by a long table. (The last one goes on for 51 pages.) After a general introduction and an historical sketch, the reader finds a discussion of the evolution of monetary instruments, the formulation of monetary policy, discounts and advances, security transactions, regulation of legal reserves, direct controls, the development function of central banks, and the role of central banks in maintaining economic stability. Each chapter and its accompanying table are filled with details of the activities of each central bank. These data were distilled, in the first instance, from the primary sources available: banking legislation, regulations, annual reports, bulletins, circulars, and announcements of the central banks. The task was arduous; the material is characterized by spottiness, incompleteness, irregularity of appearance, and occasional confusion. Great credit must be given for the completion of this task. But it is difficult to see why, once it was done, the result was not documented. The economist interested in Latin American banking will get much from the book as it stands; he would have been able to get a good deal more if the path of research had been blazed by authenticating footnotes and bibliographic references. The materials digested in the present study were surely no more diffuse and recalcitrant than those employed by Sir John Clapham in his history of the Bank of England. His careful and luminous documentation might have been taken as a model for the present work.

In the case of the tables, a curious anomaly appears. Although the tables are voluminous, taking up a total of 224 pages, they ought to be much more so to accomplish their purpose. At present they serve as a kind of reader's guide to the material contained in each chapter. The data set forth are not quite self-explanatory; to be understood they must be fleshed out by a careful reading of the text of each chapter. In the text, however, the supplementary facts are presented in a catalogue fashion better suited to exposition in a table. Indeed, a step in the obvious direction has already been taken—in the listing of changes in legal reserves by the central banks of Mexico and Argentina. The sketchy information contained in Table VII is amplified by special tables inserted in the text. The indicated solution would appear to be the construction of much more detailed tables, with a corresponding reduction of the purely descriptive portions of the text.

A great many figures are cited in the text, on an *ex cathedra* basis, but no statistical tables are included. It may be said that, given the availability of *International Financial Statistics* of the International Monetary Fund, no such inclusion is necessary. That consideration would be more persuasive if

any cross-referencing to published statistical materials appeared in the text. Without it, the exposition is deprived of much clarity.

The monetary policy of Latin American central banks presents several special features. Economic development is, of course, emphasized at every turn. In addition, a great deal of attention is given to the differences between goods and services. The use of funds to produce services is labeled "unproductive"; to produce goods, "productive." This distinction goes back to one of Adam Smith's more fatuous concepts, one which is still causing a good deal of mischief in the Soviet Union. This peculiarity is mentioned in the present work but not discussed. The general concern with economic development is, however, given a whole chapter, with particular attention to the network of relationships between central banks and the many special development organizations. Other chapters spell out the efforts of the authorities to develop a capital market and their endless battle to maintain a measure of economic stability.

None of the cavils made should obscure the substantial merits of the book. It assembles and presents in coherent form a large amount of detailed information about the organization and operation of Latin American central banks. Given the mutability of these banks, the information is necessarily obsolescent as soon as it appears; so the sponsoring institution promises that a new edition will appear periodically. It is to be hoped that the next edition will not only bring the facts up to date but will supply the additional material which specialists will be hoping to see.

I. H. OTTO

*New York University*

*Federal Reserve Policy Reappraised, 1951-1959.* By DANIEL S. AHEARN.  
New York and London: Columbia University Press, 1963. Pp. xii, 376.  
\$10.00.

This is not a good or useful book for a number of reasons. First, the level of the analysis is low. Much of the book is given over to judicious assessment of plausible and counterplausible arguments. The author picks and chooses according to his taste. Second, it reveals, for all to see, the lack of systematic analysis that often characterizes discussions of monetary policy. So many views are represented and so many economists are quoted that one must conclude that the problem is quite general, though perhaps not universal. Third, relatively strong conclusions follow from extremely weak foundations. The author makes no effort to draw monetary policy conclusions from validated monetary theory. Indeed, monetary or economic theory have no important role in the discussion.

Daniel Ahearn's stated purpose is to investigate "whether the views of monetary policy held by the Federal Reserve, and the action taken . . . , significantly impaired the monetary authorities' ability to influence the money supply, interest rates, the availability of credit, and financial institutions and markets generally." In a short opening section, the "Roosa doctrine" is contrasted to the Board's orthodox and traditional approach. These are presented as rival conceptions of the monetary process. The former is "imaginative"

though a bit "overoptimistic"; in contrast, the Board is unimaginative and fearful of the consequences of interfering in the money market.

The meaning of the difference between Washington and New York, as seen by Ahearn, becomes clearer in the second section, roughly half of the book. This section is mainly an intermittent polemic against "bills only." The author discusses open-market operations, variations in reserve requirements, use of the discount rate, and the failure to use selective controls. In the author's view, three main defects in policy are of particular importance: (1) adherence to "bills only," (2) relatively little use of fiat changes in reserve requirements, and (3) failure to use selective controls.

Adherence to "bills only" is said to have made monetary policy less effective. I never found a clear definition of effectiveness. At times it seems to refer to the relation of policy action to money supply, interest rates, "liquidity," etc. At other times, effectiveness has something to do with the relation of money, interest rates, or liquidity to the pace of economic activity. Apparently, it is all very vague. But it doesn't matter much. Short-term rates are not closely related to long-term rates (pp. 103 ff.), and long-term rates are more "important" (p. 99), so the Federal Reserve could not have done much to affect investment or the financial intermediaries by operating in bills. The author sometimes talks as if there is no relation between short- and long-term yields on governments. "Leaving the long-term markets 'on their own' meant that movements in Treasury bond yields largely reflected movements in yields of private securities such as corporate bonds. When private demand for credit rose, pushing up corporate bond yields, Treasury bond yields were dragged up too by a sort of arbitrage process" (p. 332).

But arbitrage between short- and long-term markets does not seem to occur. Ahearn's discussion proceeds without any mention of an hypothesis concerning the term structure of interest rates. When the two markets are linked (pp. 108-9), it is through "liquidity." "Excess liquidity" is created by saturating the short-term market with "funds" that later "seep into all other sectors of the market." But the excess liquidity has to be absorbed, and this takes time. The result is an impairment of the ability of the Federal Reserve to restrict credit later and to prevent a gold outflow.

Ahearn's treatment of discounting has equally serious shortcomings. He considers a number of alternatives to the present arrangements and concludes that discount rates should be loosely tied to the Treasury bill rate. His criticism of proposals by Tobin and Friedman, in particular, makes no mention of the Federal funds market or of cost and yields on particular methods of reserve adjustments. At times, he accepts the "reluctance" view of bank borrowing (p. 342); at other places (p. 133), he argues that the removal of the Federal Reserve's administrative restrictions would lead to "overborrowing" and the "potential for inflationary enlargement of the reserve base would be enormous."

The discussion of selective controls leads to the conclusion that the Federal Reserve should have standby authority. The argument that monetary controls should interfere little with private decisions is dismissed quickly. Central banking is a process of interference with private decisions. Besides,

"consumer credit is now insensitive to general credit controls and mortgage credit could be . . ." (p. 195). At several places, Ahearn insists that such controls are a supplement, not a substitute for general monetary controls. But his evaluation of their use proceeds as if there were no other policies. For example, he uncritically accepts Saulnier's judgment that by 1949 Regulation W had a dampening effect on the rate of purchase of consumer durables. No mention is made of the consistently deflationary monetary policy pursued by the Federal Reserve during that recession.

The most fascinating assertion in the book is that the Federal Reserve placed "almost exclusive emphasis in 1951-1959 on the money supply as the means through which monetary policy should work" (p. 261). Unless one is as badly confused about the difference between money and bank earning assets (credit) as the Federal Reserve, this is a difficult assertion to support. But it is a major conclusion of the study, since the author would like to see greater attention paid to interest rates, and other variables.

How does he explain the pro-cyclical behavior of the money supply, if the Federal Reserve concentrated attention on controlling money? Part of his answer is that critics have ignored demands for "credit" which, he claims, generate cyclical behavior in the supply of money. Besides, important parts of the money supply behaved countercyclically. The use of his policies would have speeded the reaction to policy, in the author's view.

It is customary to close by recommending the book to someone. In this case it is difficult. The price is high, and the quality is low. But if Ahearn is right and consumers are insensitive to interest rates, perhaps they will turn out to be insensitive to price and quality also.

ALLAN H. MELTZER

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*Money and Banking: Analysis and Policy.* By CHARLES R. WHITTLESEY, ARTHUR M. FREEDMAN, AND EDWARD S. HERMAN. New York: Macmillan Company; London: Collier-Macmillan, 1963. Pp. xxii, 617. \$8.50.

This new version of Whittlesey's *Principles and Practices of Money and Banking*, most recently published in 1954, parallels other well-regarded elementary texts in terms of length, topics, depth, and organization. Its seven parts range from an introductory discussion of institutional aspects of money and credit, commercial and central banking through analysis of simple classical and Keynesian models, to an examination of financial policy and international finance. There are many charts and tables and a highly selective reference list appended to each of the 30 chapters. Despite a few shortcomings, on the whole the book is well worth careful consideration for a beginning course in money and banking.

An excellent description of credit instruments, including an easy-to-follow arithmetic of price-yield relationships, introduces the standard discussion of banking. The authors distinguish between primary deposits which arise from increases in cash assets of the banking system and derivative deposits which result from individual banks acquiring earning assets for cash.

They explain how deposit balances function as a means of payment—shifting from bank to bank as payments are made. The real bills doctrine—labeled “traditional”—is disposed of with such dispatch that uninformed readers might conclude the doctrine has only historical importance.

The authors define the banking system's potential lending power in the manner of *The Wall Street Journal* as the difference between excess and borrowed reserves. They do not develop this theme analytically, though they return to it in their historical survey of stabilization policies. Instead they pursue the standard analysis of deposit determination as a function of cash reserves, required reserve ratios, and cash flows. This analysis is weakened because the authors fail to focus on the bank demand for cash assets as it affects deposits. The tools for this analysis were available in the very satisfactory discussion of bank portfolio management, the money market, and flow of funds. An appraisal of the role of the reserve period in deposit determination also might have helped the analysis. Member banks have reserve periods of one week if they are reserve city banks and two weeks otherwise (not half months as the authors assert). Reserve requirements need not be satisfied continuously but only on an average basis over a reserve period. Because banks subject to such rules are able to adjust over the reserve period to any gains or losses in reserves, average excess reserves can be kept low and relatively invariant from one reserve period to another—a matter of considerable importance for monetary control.

The analysis of the change in deposits associated with a change in reserves when there is a cash flow is incorrect. This error is common to most current money and banking textbooks. Let  $D$ =deposits;  $R$ =cash reserves;  $r$ =required reserve ratio;  $C$ =currency outside banks; and  $c=C/D$ . No excess reserves are held. The cash flow accompanying deposit expansion is  $c\Delta D$ , and the associated increase in required reserves is  $r\Delta D$ . The authors argue that  $\Delta R=r\Delta D+c\Delta D$  and thus:

$$\Delta D = \frac{\Delta R}{c + r}.$$

This is wrong. If  $R$  really refers to cash reserves, and these are assumed given exogenously, the change in required reserves of  $r\Delta D$  must equal the increase in total reserves,  $\Delta R$ . Hence:

$$\Delta D = \frac{\Delta R}{r} \quad \text{and} \quad \Delta C = \frac{c\Delta R}{r}.$$

The authors' answer is correct only if  $R$  refers not to cash reserves of banks, but to high-powered money—the sum of cash reserves and currency held by the public.

Monetary theory is introduced so as to give the impression that the quantity theory is useful in explaining price levels while Keynesian analysis is designed to explain employment and output. No such theoretical dichotomy need exist. The text has a long section of national income accounting and elementary principles of national income determination, and

short but good nonmathematical discussions of the term structure of interest rates and the interaction of the multiplier and the accelerator. It gives only perfunctory treatment to the vital question of lags in the effect of stabilization policies. It puts little emphasis on expectations in interest-rate determination. Such an omission permits the authors to conclude that money-supply increases would unequivocally reduce market rates of interest. This conclusion is by no means true if one takes account of expected inflation. Such expectations explain why countries with rapidly increasing money stocks and price levels also tend to have relatively high nominal interest rates.

The authors review fiscal and monetary as well as discretionary and automatic policy mixtures; surprisingly they support discretionary general fiscal and monetary policies but hold countercyclical debt-management policy to be ineffectual. They write extensively about liquidity and demonstrate the plurality of meanings associated with that term. They treat inflation, market structure in banking, and the role of banks in effecting international payments briefly but effectively. The book largely satisfies its claim of wedding analysis and policy, although it omits, perhaps justifiably, several major policy arguments of recent years. After all, why should beginning students be expected to grapple with questions the professionals seldom answer alike? Then again, why shouldn't they?

WILLIAM G. DEWALD

*University of Chicago*

*Evolución de las instituciones financieras en México.* By O. ERNEST MOORE. Mexico City: Centro de Estudios Monetarios Latinoamericanos, 1963. Pp. 413. \$4.00.

*Las sociedades financieras privadas en México.* By ANTONIO CAMPOS ANDA-PIA. Mexico City: Centro de Estudios Monetarios Latinoamericanos, 1963. Pp. 237. \$2.00.

These two useful but unsatisfactory books are symbiotic in more ways than one. Both were published at about the same time in the same series ("Estudios") by the Center of Latin American Monetary Studies (CEMLA); both treat of Mexican financial institutions; and one uses the other as a source. Taken together, they provide the nonspecialist his first full-length portrait of the machinery of Mexican finance.

Mexico's central bank, the Banco de México, with 22.2 per cent of the financial resources of the banking system, supervises the private banks (45 per cent) and the government banks (32.8 per cent). In addition to having the bulk of the financial power situated in the public sector, the Banco de México has substantial police powers which it does not hesitate to use. Its task is made difficult, however, by the persistent fiscal deficits, which have usually been monetized, and by the traditional free exchange of the peso, which lets capital flee abroad at the least alarm.

The government banks exist in great variety:

Banco Nacional de Crédito Agrícola (Farming)  
Banco Nacional de Crédito Ejidal (Communal Farming)  
Banco Nacional de Comercio Exterior (Foreign Trade)  
Financiera Nacional Azucarera (Sugar)  
Nacional Financiera (Infrastructure)  
Banco Nacional Hipotecario Urbano y de Obras Públicas (Public Works)  
Banco Nacional de Fomento Cooperativo (Cooperatives)  
Banco de Pequeño Comercio del Distrito Federal (Small Business)  
Banco Nacional del Ejército y la Armada (Army and Navy)  
Banco Nacional Cinematográfico (Motion Pictures)  
Banco Nacional de Transportes (Transport)

as well as a quantity of special funds, commissions, and facilities. The interesting question of why the Mexican government thought it best to set up all these assorted banks instead of just one is not quite answered by O. Ernest Moore. He does mention that the authorities studied the matter in the 1920's and decided to adopt the European approach, but that is hardly a penetrating answer. Similar superficiality mars his account of the growth and development of each bank. As a handbook containing most of the more important data from the official reports of the Banco de México, Nacional Financiera, etc., the compendium is most useful. Its weakness lies in the fact that little more than official handouts can be found. In consequence, the data for one bank are presented quite differently from those of the next. Some of the sources and uses tables balance; some do not. Occasionally two different sources and uses tables are presented for the same bank and year, neither of which satisfies the requirements for a proper statement. Some of the official reports reproduced uncritically are misleading, as when government subsidies in the form of stock purchases are described as "internally generated funds" ("fondos propios"). And the tables of profits displayed for, say, the Banco Nacional de Comercio Exterior vanish without a mention when it comes to a perennially unprofitable bank like the Banco Nacional de Crédito Ejidal. Surely, to an economist, the losses of the one are just as interesting as the profits of the other. What needs to be done is to sift the data, develop the missing bits and pieces, and then present the result in a uniform and coherent form.

The private banking sector is made up of commercial banks, savings banks, mortgage banks, trust companies, finance companies, and home loan banks. Ever since 1950 the commercial banks have been losing ground to the finance companies. Of the private banking sector's resources, the commercial banks held only 44.4 per cent in 1960, down from 62.8 per cent in 1950, while the finance companies grew from 12.7 per cent to 30.3 per cent in the same period. A good part of the change represents reaction to increasingly severe government control. When the Banco de México was founded in 1925, the required reserves were only 10 per cent and applied only to commercial

banks. Now the required reserves are 100 per cent on some deposits (increased holdings of foreign currency) and 75 per cent on most. Part of the required reserves must be deposited with the Banco de México, part invested in government bonds, and the rest dedicated to prescribed loans and investments. Antonio Campos Andapia is quite laudatory about this process of imposing "minimum coefficients of liquidity" on the banks. As no ready market for Mexican government bonds exists, the fact that they constitute over 13 per cent of commercial bank assets can hardly be said to increase their liquidity.

Mexican banks and finance companies are closely linked; so the mounting required reserves for commercial banks stimulated a flow of funds to the finance companies—by dint of suasion, high interest rates, and the maintenance of complete liquidity. The growth of the finance companies was so rapid that in 1958 the Banco de México imposed reserve requirements on them similar to those of the commercial banks, thus "extending to them all of the advantages developed in the management of legal reserves," and ending their "disloyal competition" with other financial institutions. This concept is developed in a curious paragraph by Campos Andapia which is, in addition, quoted with apparent approval by Moore:

The influence of the private finance companies in raising the rate of interest is attributable to the use to which they have put the resources gathered. They have failed basically to comply with the condition specified in the theory of financial intermediaries to the effect that, in order to exercise a beneficial effect on the economy, they ought to channel their funds toward financing investments. They have perpetrated two principal misallocations of their resources, which have permitted them to obtain an inflated income and, in turn, pay higher rates of return than their competitors. The misallocations involved . . . are as follows: (1) the great quantity of financing devoted to distributive and speculative activities, and (2) the major financing dedicated to investment in their own companies, in which they obtain the going rate of profit, which, by yielding them an inflated income, allows them to overestimate the market rate of interest and pay a higher rate for the savings entrusted to them.

This paragraph epitomizes the attitude of the authorities toward the financial activities of the private sector. Finally, when even the imposition of high reserve requirements failed to halt the growth of the finance companies, from less than one billion pesos in 1950 to more than nine billion in 1960, the government imposed a growth limit on them. The limit varies with the size of the company, but in the main comes to 1 per cent a month in term deposits. Bond emissions are controlled separately.

These two books are useful and provide a quantity of information in convenient form. They are not noticeably analytical; some of the facts set forth are contradictory; major gaps exist in the data; and some of the interpretations offered, especially by Campos Andapia, are ingenuous. In the case of Moore's book, perhaps the right phrase would be "premature publication." With another two-years' work, the book might have been first-rate. As for

Campos Andapia, in all probability no amount of extra time would have made any difference. His effort is obvious and commendable, but his grasp of money and banking is so tenuous as to vitiate the result. The definitive work on Mexican financial institutions has not yet been written.

I. H. OTTO

*New York University*

*Report of the Committee on Inquiry on Decimal Currency.* Presented to Parliament by the Chancellor of the Exchequer by command of Her Majesty, September 1963. (Cmnd. 2145). London: H. M. Stationery Office, 1963. Pp. 253.

As early as 1853, a Select Committee of the House of Commons recommended decimalization of the British currency. Although this recommendation was subsequently reversed in the late nineteenth-century halcyon days, the proposal has the dubious if unique distinction of being the only reform longer delayed than civil rights in the United States.

The Halsbury report, which again recommends decimalization, will presumably soon be discussed by the public and Parliament, where the Chancellor of the Exchequer in 1961 took the position that the Halsbury Committee "is not being asked to consider the question of 'whether,' but the question of 'how' to decimalize" (p. 2).

Almost all the Commonwealth nations—India, Pakistan, Australia, New Zealand, and others—have already decimalized, or decided to do so. South Africa has as well, and Canada has a dollar-cent decimal currency.

To replace the coinage and the machinery of counting and computing will be costly—the Report estimates £109-£128 million. If despite the recommendation the cost is deemed prohibitive, decimalization could be delayed, and Mr. Selwyn Lloyd, Chancellor of the Exchequer at the time the Halsbury group was appointed, provided this loophole in his remarks to Parliament.

The Committee lists eight criteria which they deem desirable in a decimal system: it should be consistent, simple, flexible, lasting, should not affect the international standing of sterling, should not present people undue difficulties in the transition, should not result in avoidable price increases, and should not result in unduly high machine costs.

To decimalize the current British currency, with its assortment of multiples of 12's and 20's,<sup>1</sup> the Committee argues it can move, basically, in one of three ways, up or down from the three basic British currency units, the pence, the shilling, and the pound. In large measure, any choice is arbitrary. "There is no perfect answer."

Thus if one opts to move up from the pence, by creating a unit equal to 100d., that unit would be worth 8s. 4d. The Committee rejected this type of decimalization.

<sup>1</sup>For readers who have forgotten the basis of British currency, there are 12 pence (d.) per shilling (s.) and 20 shillings per pound (£). Since the pound is now valued at \$2.80, a shilling is valued at 14¢, and a pence at 1.1666¢. The key problem in decimalization is to replace the base of 12 (pence to shilling) with a base unit of 10, similar to the shifting from 12 inches to a metric unit in the United States. There are, of course, broader ramifications of this conversion, some of which this review explores.

From the shilling, the Committee considered a basic unit equal to 10s., with each 'cent' being worth 1.2d. The minority of two of the Committee recommends the adoption of this unit.

However, the majority (of four) recommends the third alternative: using the present pound, each of whose one-hundredth part subunits would be worth 2.4d. Now since 2.4d. is a very large minimum unit at current prices, the majority realizes it will have to add, for the convenience of the whole system, a smaller minimum unit, the  $\frac{1}{2}\phi$ , worth 1.2d.

The Committee is aware that adding this fraction—or technically adding this mil digit to a decimal system, since the  $\frac{1}{2}\phi$  would be worth £.005—adds a "vulgar fraction," whose "introduction undoubtedly complicates a decimal system and therefore probably makes mental calculation and change giving slower and more liable to error." Further, "machines needed to record vulgar fractions, . . . would have to be non-standard. If required to record the fraction decimally, i.e. as .005, machines would have to be of higher capacity and thus more expensive. Manufacturers of business machines and of electronic data processing equipment are strongly opposed to the use of vulgar fractions."<sup>2</sup>

Put differently, the excessively high minimum coin (2.4d.) means that the pound is too large a unit to start with. The Committee had been concerned with long-term inflation of prices in its thinking; it cited something like a 14-fold rise since the fifteenth century—and the criterion that "the system should last" clearly eschews a minimum unit which will fall into disuse as prices rise. The Italian lire, e.g., was once divided into 100 cents, but now even the smaller lire units are not usable. Nevertheless, the £- $\phi$ - $\frac{1}{2}$  system would seem to err on the high side, for the present at least.

In view of these pre-announced difficulties, the reader may be at something of a loss to understand the selection of this seemingly most cumbersome and least-true decimal system of the three. It must be inferred, but seems quite clear, that the overriding consideration behind the majority's selection of this £-cent- $\frac{1}{2}$  system, despite its drawbacks, is the desire to retain the pound as the basic unit of currency. This desire not only reflects the importance, in the Committee's mind, of the pound as a unit of international reserve, with all that implies, but also raises the issue of "associability," with which the Committee is concerned.

Stated simply, "associability" is assumed to be good because a new currency "should" retain some landmarks familiar in the old currency. In each of the three methods of decimalization outlined above, there is some landmark—the pence, the 10-bob note, and the pound. But quite clearly the landmarks virtually end there, for except for a random coin here and there, most of the units would need to be quite new. That associability is taken seriously is doubly surprising in view of the findings of the Applied Psychology Research Unit at Cambridge, which worked with the Committee on this problem. "One surprising result of these experiments was, indeed, the better performance which British subjects achieved in mental arithmetic using the U.S.

<sup>2</sup> P. 12. Also see Chapter 8, paragraphs 287 to 293.

dollar and cent system than the £ s d system they had been using all their lives."<sup>3</sup>

The Committee is also concerned with two cost factors. The cost of new coin and computer machinery has been noted. It comprises the bulk of the estimated cost of decimalization. Second, the matter of price adjustments receives great attention. Clearly, fine adjustments of price—the current  $\frac{1}{2}$ d.—would be wiped out, particularly if the minimum unit of the new coinage were too large. In the two plans recommended, the 10s. of the minority and the £-¢- $\frac{1}{2}$  of the majority, the minimum unit is 1.2d.; in the majority report this is only attained by adding the  $\frac{1}{2}$ ¢ unit—worth 1.2d.—and thus destroying the decimal system.

So much for the Committee's Report. That it is thorough and scholarly will surprise no one who is familiar with the Committee members. That it neither makes easy nor interesting reading probably flows from the subject matter. That its horizons are limited may well flow from the frame of reference within which the Committee was asked to work.

Freed from any restrictive frame of reference, save decimalization itself, it seems to me a better plan could be put forward.

Briefly stated, this is that decimalization be based on a new British unit currently worth 7s. 2d. A moment's reflection will reveal this to be equal to a U.S. dollar, while the unit could be called anything, e.g., an "Elizabeth," the following proposal may for the present be called the "British dollar" plan. The proposed British dollar would be divided into 100 cents whose multiples would doubtless in time by colloquial usage be called something other than dimes and quarters.

There are four ideas behind this proposal. The first is technical. This system meets better than any other the criteria set forth in the Report and avoids most of the pitfalls of the plans currently recommended. The units are small and viable. They provide a consistent and unbroken decimal system, unlike the majority proposal £-¢- $\frac{1}{2}$ . The units are not insularly English but increasingly universal in the English-speaking world. As the Cambridge study noted, Britons can handle the dollar-cent system better than their own. Machines for this coinage are readily available from North America, and in time North America, Australia, and other areas could well import the products of the British coin and related machine industry, which would have export advantages.

Perhaps the most important technical consideration bears on the problem of price increases in the transition, and the related problem of wage rates, etc., to which the Report gives a full hearing. The trouble with both the majority £-¢- $\frac{1}{2}$  plan and the minority 10s-¢ plan, apart from the "vulgar fraction" break in decimalization in the former, and insularity of the latter, is that the minimum unit, the cent, equal to 1.2d. in both plans, is too big. Every price or wage now measured in  $\frac{1}{2}$ d. would be rounded up, it is feared, nearly threefold. (Obviously eventually competition and not currency must be relied upon to hone price adjustments.) In the transition and beyond, however, the ¢ unit in the "British dollar plan" here proposed is .86d., much closer

<sup>3</sup> P. 13. Also see Appendix 8, paragraphs 5 to 15.

to the current  $\frac{1}{2}$ d. than any other minimum unit proposed, and a unit, given the arguments in the report, that seems just about right as a starting coin.

In general, I tend to give little credence to the "associability" argument. If justification is needed, it is clear to me that no system will provide real associability; all will provide random bits. For example in the proposed £-¢- $\frac{1}{2}$  system, the shilling can remain, equivalent in stature to the American nickel. In the British-dollar plan, the 2s. 6d. coin (half a crown) could remain, equivalent to 35¢. But neither of these "associated" coins will buy what it once did; like false cognates in a foreign language, these old familiar "landmarks" may well be more misleading than helpful. Coin machinery would still need to be replaced to take into account new price relationships. As the Cambridge study cited implies, a new and rational system would actually be easier to adapt to. This study, and South African experience, suggest that adaptation should be considered a matter of weeks rather than months. "Associability" may well be a misleading criterion, best abandoned.

Second, and perhaps most important from a British viewpoint, the present British pound could be retained as the historic lynchpin of British currency, and of the extensive system of international accounts which center on sterling. The "British dollar" would be linked in value to the pound sterling at its current value, so there is no possibility of the outside world losing confidence because of an apparent change. The arithmetic of the process is simple: £5 would be exactly equal to 14 British dollars. Though the pound would remain unchanged and unchallenged in the background, all daily working transactions could be carried out in the new "British dollar." The pound and the British dollar would, in relation to other currencies, move up and down the exchange (within IMF limits) exactly as the pound now does, a function of confidence and demand. The managers of the two great reserve currencies—the U.S. dollar and the pound—would consider each other's strengths and needs in exactly the same way they now do. They would work together in bilateral arrangements (swaps, etc.) to support each other's reserves against swings of trade or confidence; and similarly in wider groupings—in the Group of Ten and in IMF—mutual understanding and cooperation, which has made such striking progress in the last few years, would continue along present lines.

Third, the adoption of what amounts to a common internal currency would provide for the U.S. and British economies a common measuring rod on movements of costs and prices. Until now the two economies have been insulated from each other at the level of daily working contacts. A common currency makes understanding of each other's cost structure and behind this of production methods possible in ways much easier for lessons to be learned and applied without the intervention of heavy statistical interpretations of experts. Such interpretations may make good reading in the *American Economic Review* or the Oxford Bulletins, but are unintelligible to the factory manager and shop steward who must understand and improve productivity relationships.

Finally there is a political motive to this plan. At this moment in history

the British people could make a change which would be a momentous step forward in Anglo-American cooperation. By adopting in effect a common currency, the great forward step would be accomplished without negotiation, without treaties, and without any loss of financial independence by either the United Kingdom or the United States. Rather than adopt a unique and insular decimal system, the British could, with no more effort (indeed less) bring the reserve currency—sterling—and the British internal currency, the “British dollar” here proposed, into a new and close relationship with the other major reserve currency—the U.S. dollar.

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### Public Finance; Fiscal Policy

*Fiscal and Debt Management Policies.* A Series of Research Studies Prepared for the Commission on Money and Credit. By WILLIAM FELLNER, RICHARD A. MUSGRAVE, JAMES TOBIN, JAMES R. SCHLESINGER, PAUL H. COOTNER, IRVING AUERBACH, RALPH K. HUITT, AND JOHN LINDEMAN. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. 539. \$6.50.

This is one of nine volumes presenting eight of the many research studies prepared as background papers for the Commission on Money and Credit. If they are a fair sample, the Commission had some excellent material to work with and fulfilled well what was perhaps its most important function, the stimulation of new thinking and research. It must be regretted that the Commission took over three years to publish these papers, which has diminished the social usefulness of the work. It is astonishing that the papers have stood the passage of time so well.

The book begins with two important papers on tax policy. Professor Fellner presents a carefully qualified argument that our tax system should be re-directed away from income taxation towards sales taxation. He advances three lines of reasoning: first, while extreme beliefs on the disincentive effects of income taxation were debunked by scientific investigation, the evidence does not show the effects to be totally absent, and we may have swung too far the other way in our skepticism. Second, the particular notions of equity which justify progressive income taxation are not the only reasonable notions of equity. Sales taxes with exemptions for necessities could promote another kind of equity, presumably a penalization of high living. Finally, he cites international comparisons on the percentage of the tax burden which various countries collect in the form of direct taxes and finds the United States out of line.

Fellner sees two possible ways in which our tax system could move towards greater reliance on sales taxes. If the greatly rising costs of education continue to be financed at the state and local level, the adjustment would come almost automatically. The federal government would, under reasonable assumptions, have room for tax reduction. State and local governments would be forced to increase their tax rates, which would be largely sales (and property) tax increases. Alternatively, the federal government could begin to shoulder more

of the cost of education—an issue he would resolve on educational grounds—in which event he would seriously consider the institution of a federal sales tax with “appropriate exemptions.”

Fellner's uneasiness about disincentive effects is now shared more widely. I find the argument about achieving equity through sales taxes less persuasive. A sales tax designed to be a major revenue source must include a wide range of commodities, too wide a range to discriminate meaningfully among income classes. Even if food and children's clothing are omitted, the income elasticity of demand for the remaining consumption items cannot exceed unity by much. In this age of mass consumption it is difficult to select categories of goods and services for taxation which are not bought by families in most of the income ranges. Certainly not enough items could be identified to constitute a major tax base. Nor do I believe that a major reduction in income taxes in the United States can be rationalized through international comparisons. While we do levy a somewhat greater fraction of our total taxes on income than the major European countries usually chosen for comparison, this difference is largely due to our higher income levels and the greater scope of the corporate sector, not to higher rate structures.

Fellner writes as if he were a voice in the wilderness. But events have caught up with him surprisingly rapidly. In the three years since he wrote, the tax system has developed exactly as he said: income taxes have fallen under growing suspicion; the federal government has enacted large income tax reductions, and state sales and property taxes have been increased to meet the costs of education. It remains to be seen whether the switch from direct to indirect taxation will, in fact, raise the saving and investment rates.

Richard Musgrave questions whether changes in the composition of the tax system can raise the saving rate. While he ranges authoritatively over the full set of relevant issues, including the proper treatment of capital gains, the integration of the personal and corporate income tax, the effects of the high marginal rates, etc., the heart of his paper is a numerical analysis of the impact of the tax structure on aggregate savings. Using survey data and judgment estimates, he derives the marginal impact of changes in the major taxes on saving. He assumes that 80 per cent of the change in corporation income and estate taxes falls on savings, 31 per cent of individual income taxes, 18 per cent of sales taxes, and 25 per cent of property taxes, making for an average impact of 36 per cent for the entire tax system. Given these figures, Musgrave finds that the impact of the tax system on savings is large, but that plausible changes in the composition of the tax system would not raise savings, except for a switch away from corporation income taxation. Since he finds that tax to be shifted in his more recent work with Krzyzaniak, even that possibility is now ruled out. Thus, the full-employment saving rate can only be raised through a higher budget surplus.

Musgrave's analysis seems to me to overstate the impact of taxes on savings. The figure for the personal income tax, which is the most important, is largely based on very high marginal savings rates on incomes over \$15,000, where two-thirds of the savings impact accrues. This rate is assumed to reach 50 per cent at an income level of \$17,000 a year. There is virtually no in-

formation on the savings behavior of these groups, making the estimate very uncertain. I would judge the long-run marginal saving propensity in these brackets to be substantially less than 50 per cent. Musgrave's estimate can perhaps be explained by his statement later in the paper that these incomes are largely from capital. But this is not really borne out by his figures; even in the \$50,000-\$100,000 bracket more than one-half of all income is payment for personal services.

The remaining papers deal with monetary questions. James Tobin applies a portfolio model to debt management. He defines debt to include all debt of government, including noninterest-bearing currency. He then divides the effects of the debt into two kinds, fiscal effects, which are identified with the multipliers applied to the surpluses and deficits that cause the debt to change, and monetary effects, which result from "the private ownership of claims against central government," and which persist as long as the debt exists.

Since the public debt represents private wealth (as long as individuals do not consider their share of the public debt as an offset), the monetary effect of an increase in the debt is considered expansionary. Tobin uses his portfolio model to work out the implications of this statement in some detail. For example, if the increase in the debt is in the form of short-term securities, individuals will not wish to hold the entire increment of private wealth in this short-term form but will put part of the increment into private equity capital. This switching will reduce the supply price of private equity capital, and encourage investment. Similar analyses are offered for demand debt and long-term debt. Tobin also uses his portfolio model to analyze the impact of changes in the maturity structure of the debt and reaches the conventional conclusion that a lengthening of the debt has a contractionary effect.

While this portfolio-effects analysis is useful and, in the hands of Tobin, yields some significant insights into the repercussions of the existence of the stock of public debt, there is a hazard that it directs attention away from other monetary effects which may be more important. When Tobin analyzes an "increase" in the debt, he makes a static comparison of two situations identical in every respect except the size of the stock of debt. This leaves no room for the process of debt-increase itself, which is usually taken to have a contractionary monetary effect. Tobin must assume that the process of issuing new debt is accompanied by monetary policy actions of ease which exactly offset the initial negative impact on the supply price of capital for private investment.

Tobin also deals with the choice of objectives of debt management and monetary policy. He proposes that the interest cost of the debt to the taxpayers be minimized, given the proper contribution of money and credit policy to stabilization. He does not elevate minimum interest costs to be an objective of high priority, but rather argues that whatever the possible contribution of monetary measures to stabilization may be, it can be achieved in various alternative ways: within the family of money and credit instruments he would favor the use of a set of tools that would keep interest costs to a minimum. For example, the money supply should not be expanded secularly by a reduction of commercial bank requirements, as has been done in recent years,

but by open market purchases. Further, he would ask for sympathetic consideration of reserve requirements for nonbank financial institutions which would force them to hold a certain amount of government debt.

Finally, Tobin endorses the proposal for purchasing power bonds escalated by the consumer price index. He argues that the private investor has a large menu of possible investment media available to him, but that despite this proliferation, he still has to choose between a set of securities having a very low specific risk but not hedged against inflation, and another set of securities which is at least crudely hedged against inflation but has the high specific risks of equity investment. Only the government is in a position to insure against changes in the consumer price index, and Tobin argues that it has an obligation to offer this form of insurance to the public. It is unfortunate that the equity aspect of the purchasing power bond has been so little considered and the proposal has never really received a serious hearing in this country because of the unproved assertions about its effects on the preservation of fiscal discipline and the spreading of mass inflation psychology.

Two papers deal with potential new tools for monetary policy. James Schlesinger analyzes proposals to insulate the market for federal debt. I think it is safe to say that his paper buries the idea for the present. Most of the proposals, such as the supplementary security-reserve scheme which would have required banks to hold government securities as a secondary reserve, were made in the immediate postwar years, when the public debt loomed much larger in the economy and was an apparently insurmountable obstacle to an effective, flexible monetary policy. Schlesinger finds that it would have made excellent sense to finance the war through a scheme that would have frozen the new bank reserves as they were created, and that much later trouble would have been avoided. Banks could have been required to purchase and keep securities as British banks were required to do. But with the war-time opportunity missed, and with the banks holding large quantities of marketable government bonds, it would have taken very drastic proposals to achieve insulation, with the social costs considerable and the gains uncertain.

Since the main goal of insulation was to restore Federal Reserve control over the volume of bank loans, Schlesinger appraises insulation as a form of direct control to serve that end. On that criterion it is feasible, but there are more effective control instruments, such as the velocity plan for reserve requirements, differentiated reserve requirements against different forms of assets, or direct limitations on bank loans.

Irving Auerbach examines the instrument of Treasury cash balances as a means of controlling commercial bank reserves. He concludes that in principle this could be a very powerful device. It can be more selective than open market operations and could affect banks quickly, even those remote from the money market. It might make it possible for the authorities to meet their free-reserve targets more precisely than they do now. The initial impact of monetary policy would no longer occur in the government securities market. And even now the instrument is used to insulate commercial bank reserves from short-run swings in federal receipts and payments.

Nevertheless, Auerbach does not recommend use of the Treasury cash bal-

ances for broad policy. He feels that present instruments suffice to regulate bank reserves. But the main arguments are political and administrative. First, the Treasury might be running monetary policy, though this difficulty could be overcome by delegation of the responsibility to the Federal Reserve System. Second, there is the possibility of abuse. In the late nineteenth century, when Treasury balances were used to run at least a counterseasonal monetary policy, the system left room for favoritism by making funds available to certain private banks without interest cost (the famous "pet banks"). Since use of this instrument would require Treasury balances to be kept in excess of the amounts necessary for the normal financial operations of government, it would increase the interest cost paid by taxpayers to the benefit of the banks holding the excess deposits.

Paul Cootner's paper is of considerable theoretical importance but does not cast much light on the question the Commission posed him: what has been the role of speculation in the government securities market and what should be done about it? He builds an elaborate and suggestive model of bank liquidity behavior that will be of interest to monetary theorists; the model is tested by correlations of differentials between long- and short-term interest rates against the levels of these rates. It turns out that the differentials widen when long-term rates are low, which is consistent with his theory (and many others). Cootner then takes his theory to be verified and concludes that since banks act according to his theory, they serve to stabilize the market. He finds that "the market has been quite good in a stochastic sense," and attributes its wide swings to the pursuit of economic stabilization policies by the authorities.

To anyone who has read the Federal Reserve-Treasury Study account of the speculative collapse of the bond market in the summer of 1958, this is an incredible conclusion. No amount of theorizing is going to be able to discover from a correlation of interest rate differentials against interest rate levels whether or not speculators widened the swings of the government bond market. Cootner makes little use of the materials of the Treasury-Federal Reserve Study, and the mix of theorizing to research is askew here.

Two more papers complete the volume. Ralph K. Huitt, a political scientist, presents a skillful and perceptive analysis of the role of the Congress in the formation of economic policy. Finally, John Lindeman surveys political and administrative arrangements affecting foreign credit operations, emphasizing particularly the role of the National Advisory Council on International Monetary and Financial Problems.

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// *The Shifting of the Corporation Income Tax—An Empirical Study of Its Short-Run Effect upon the Rate of Return.* By MARIAN KRZYŻANIAK AND RICHARD A. MUSGRAVE. Baltimore: Johns Hopkins Press, 1963. Pp. viii, 100. \$4.00.

The U.S. corporation income tax occupies a central role in the tax structure of the public sector and takes a major cut of the profits of the corporate

enterprise.<sup>1</sup> The corporate income tax is the federal government's second-largest revenue source. It is expected to yield \$25.8 billion in the coming fiscal year. Despite its obvious importance, the economic effects of the tax remain largely unmeasured. Lewis [8] and Goode [4] have studied its aggregate dynamic characteristics and theoretical impact; Harberger [6] and Lintner and Butters [9] have attempted to measure the impact of the tax on market organization and allocation; Eisner [3] and Hall [5] have studied the interaction between the tax and certain corporate costs.

The history of the corporate income tax is marked by controversies. Economists have been unable to link the tax directly to the benefits from government services received. Nor have they supplied a rationale for taxing consumers and investors indirectly through a profits tax. Indeed, inferences about the economic effects of the tax depend heavily on knowledge of the incidence of the tax between consumers and investors. If investors bear the ultimate burden, the tax may limit investment and growth. If consumers bear the ultimate burden, our corporate product may be at a competitive disadvantage in markets abroad. Enlightened economic policy-making thus requires good evidence on the incidence of the tax.

This is the setting within which Marian Krzyzaniak and Richard Musgrave present their study. They examine corporate response to changing tax rates for the period 1935-59. They undertake the difficult task of disentangling tax-induced behavior from the myriad of exogenous influences on the corporation. Their discussion of corporate responses to tax rate changes in the context of a closed economic system constitutes a major contribution not only to the study of this problem but also to the study of other tax-induced behavior.<sup>2</sup>

The book combines four elements: (1) theoretical derivations of measures of the degree of tax shifting, (2) alternative models of corporate responses to changing levels of tax rates, (3) estimates of those models, and (4) the derived estimates of corporate tax shifting. In Chapter 2 the authors display a variety of indices that measure tax-induced corporate behavior. To replicate various findings that have been published in the literature (Adelman [1], Lerner and Hendriksen [7]), the authors show gross changes in these indices without any adjustment for other changes in the corporate economic environment. Zero tax shifting is implied by gross changes in the index—profit plus interest as a proportion of corporate value-added—between 1922 and 1959. Gross changes in the level of manufacturing rates of return imply shifting in excess of 100 per cent from 1936-39 to 1955-57.

The heart of the study consists of estimating the effect of taxation on the rate of return *ceteris paribus*. The magnitude of the effect is estimated from a relationship that includes other predetermined influences on corporate rates of return. Estimates are based on annual observations from 1935 to 1959,

<sup>1</sup>In 1959, 11.5 per cent of all active corporations paid taxes at marginal rates of 52 or 54 per cent. These corporations received 93 per cent of corporate income. See Internal Revenue Service, *Statistics of Income: 1959-60 Corporation Income Tax Returns*, p. 112.

<sup>2</sup>The shortcomings of some of the past work are discussed in Zellner [10].

excluding 1943-47. Two behavior hypotheses are tested: Model A asserts that the corporation increases its gross rate of return ( $Y_g$ ) over the no-tax rate of return ( $Y'$ ) by some fraction ( $a$ ) of the *tax* in relation to capital, i.e.,

$$Y_g - Y' = a (T/K).$$

Model B asserts that the increase in gross rate of return over no-tax rate of return is some fraction ( $b$ ) of the *tax rate*,

$$Y_g - Y' = b Z.$$

Actually Model A is identical to the assumption that the firm increases its rate of return by a percentage equal to a fraction  $a$  of the tax rate  $Z$ . Both models are estimated in equations where linear combinations of lagged consumption, the lagged ratio of inventory to sales, the present personal tax rate, and present government expenditures represent the no-tax rate of return. The estimates of both models imply shifting by manufacturing corporation significantly in excess of 100 per cent.<sup>3</sup>

A brief excursion into estimation of the tax parameter of Model A for some selected industries discloses that small and large corporations have similar behavior, but there is a range of behavior among industries generally consistent with 100 per cent shifting. Somewhat higher values of the tax parameter are estimated for a group of firms chosen as "price leaders" than for another sample group of "price followers."

A parallel analysis of tax effects on flow of funds is presented with related studies in Chapter 7. Coefficients are estimated for Model A when the dependent variable includes both profit and depreciation as a proportion of equity. The authors recognize that corporation responses to changing acceleration of depreciation will be attributed to the tax coefficient estimated whether rate of return or flow of funds is used as the dependent variable.<sup>4</sup> Because of a general increase in the degree of acceleration over the period 1935-59, actual corporate response to changing tax rates must lie between the values estimated for the tax coefficient in the two equations (1.23 for the equation based on rate of return, 1.46 for the equation based on flow of funds).

The analyses suggest a variety of further work:

1. Interindustry differences in tax responsiveness ought to be examined in relation to capital-output ratios and market structure. Capital-output ratios

<sup>3</sup>  $R^2$  equals .93 for Model B, .98 for Model A (standard version). Goodness of fit is the authors' primary argument for concentrating estimation on Model A.

An instrumental variable technique is used to estimate Model A. However, in nearly all estimates the effective tax rate  $T$  is used as an instrument in spite of the fact that it is an endogenous variable in the system. In the one case where the more appropriate statutory tax rate variable is used as an instrument for Model A, the coefficients are almost identical to those obtained from estimates based on effective tax rates.

The reader will be spared some bewilderment by noting that numerous relationships are derived in Appendix C on the assumption that  $a_4 = 1 + A_4$ . According to footnote 8, page 26, the relationship must be  $a_4 = 1 - A_4$ .

<sup>4</sup> It is not clear why the authors did not use degree of acceleration as an independent explanatory variable, perhaps as a determinant of the parameters  $a$  and  $b$ .

are relevant because of the unresolved question as to how changes in depreciation for tax purposes affect the estimated parameters ( $a$  and  $b$ ). Market structure may account for differences in the ability of a firm to engage in price changes at the time of tax rate changes. If so, a regular relationship between market structure and tax responsiveness should emerge.

2. The short-run dynamics of corporate response to the tax rate might be studied. In the model estimated the tax parameter reflects the degree to which corporations have responded to a level of tax rates. The fact that lagged values of the tax variable contribute insignificantly to the relationship suggests that adjustment is complete within the first year. What happens within that year? The problem could be studied on the basis of available quarterly series with a model that gives estimates of the speed with which corporate business adjusts to new tax environments.

3. A study of tax adjustment in the utility industry, particularly electric utilities, would be most informative. Price regulation entails legally endorsed shifting of taxes to the consumer. Estimates of the tax parameters of Model A for this industry might help to explain why manufacturing industries appear to have responded to changes in taxation in a way that overcompensates for the initial change.

4. The models ought to be estimated on the assumption that businesses respond to changes in tax rates by adjusting the absolute level of profit rather than a rate of return. Such models can be derived from A and B by multiplying each expression by the level of capital stock. Estimates on that basis would be relevant for a variety of reasons. Business may act on the basis of profit position in its pricing behavior; if so, statistical fit could be improved by estimates derived from profit relationships. Estimates based on rate of return may obscure the short-run adjustment as any uncompensated exogenous shock on profits will alter corporate net worth in the following period if corporations maintain constant dividend rates.<sup>5</sup> Thus changes in rate of return may not be a suitable index of corporation adjustment to changes in tax rates in the short run. Finally, it is the absolute profit which appears in most structural models of the United States. It would be instructive to compare the tax response of corporate profits in the Klein-Goldberger and similar models with the response indicated in the authors' formulation.

Despite the fact that the authors make no claim to studying the incidence and growth effects of the corporate income tax, they cannot be forgiven for omitting a chapter on those subjects. Determining the response of corporate enterprise to taxation is of interest primarily because of the far-reaching consequences that response might have for investment, the dynamic and static incidence of our tax structure,<sup>6</sup> and the growth of the economy. A few remarks on the implications of this study for tax policy that appear on page 66 are inadequate and confusing. The authors suggest that the tax is similar to

<sup>5</sup> The existence of some relationship of this kind may explain why residuals from both models fail to pass the Durbin-Watson test for negative serial autocorrelation.

<sup>6</sup> Dynamic incidence refers to the impact of the tax system on changes in income. See Douglas Dosser [2].

a general sales tax on corporate capital and consumer goods. Variation in the profitability and the proportion of value-added originating in corporate rather than noncorporate enterprises in different industries implies that the specific sales tax rate applicable to a given firm would need to vary substantially to replicate the impact of a shifted corporate income tax. The authors indicate that such variations will cause the tax to distort relative price levels. Sufficient relative price movements may be generated by such forward shifting to cause sizable misallocation of resources [6], even in a period as short as the annual period studied by the authors.

In this work Krzyzaniak and Musgrave demonstrate the need to apply well-conceived econometric models for the analysis of tax-induced behavior. Students of business behavior will find this work stimulating and suggestive of further work.

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*Public Finance and Budgetary Policy*. By ALAN WILLIAMS. New York: Frederick A. Praeger, 1963. Pp. 283. \$8.50:

Mr. Alan Williams' book grew out of a course given for honors students at the University of Exeter. The work is entirely theoretical, and it is not easy reading. The analysis rests on complicated graphs, many of them three-dimensional.

Part A deals with microeconomic effects of government tax and expenditure policy. Williams begins with the analysis of the effects of lump-sum taxes, income taxes, expenditure taxes, and wealth taxes on work effort, savings, and financial investment. There are two chapters on the effects of business taxes. Using the conventional partial-equilibrium theory of the firm, Williams studies the impact of lump-sum taxes, profit taxes, value-added taxes, gross-revenue taxes, and gross-cost taxes on price and output policy. Chapters 9 and 10, respectively, are devoted to the question of excess burden and the incidence of various expenditure policies.

Part B of the book, entitled "Macroeconomics," begins with a study of the general equilibrium aspects of tax and expenditure policy. The remaining one-seventh of the book is devoted to questions of stability and growth.

The first sections of this work are the most useful and comprehensive. Although much of this material is standard fare, there are a number of extensions and additions that are not found in other textbooks. One of the more useful features is Williams' emphasis on the differential impacts of alternative tax policies. For example, after determining the effect of a proportional income tax on work effort, the author immediately goes on to compare this result with the effect of a lump-sum tax of equal yield. The differential analysis is undoubtedly the most useful approach to tax questions and, unfortunately, is rarely used. The chapter on the income tax goes beyond the usual discussion of the proportional income tax and deals with progressive rate structures and the complications that arise when incomes are unevenly distributed through time. Also, in treating the impact of government expenditure on different economic agents, Williams provides new insights on a question that is barely mentioned in most textbooks on public finance.

The most ambitious parts of the book relate to the study of the differential effects of proportional expenditure and income taxes, and taxes on wealth of equal yield. These questions involve three dimensions, and Williams uses two approaches, a one-period analysis where the three variables are income, consumption, and leisure, and a three-period analysis where before-tax incomes are taken as given. Some writers, Kaldor being most notable among them, maintain that savings will be increased under an expenditure tax. Williams shows that this proposition is nonconclusive and that savings may or may not increase if a proportional expenditure is substituted for a proportional income tax of equal yield, depending on relative preferences for savings, consumption, and leisure. Williams also arrives at the interesting result that, in the case where saving is highest under the expenditure tax, there is no guarantee that this tax is superior on grounds of individual welfare. The welfare implications are obtained through the use of the basic axiom of revealed-preference analysis. The author is quick to point out that his results are very tentative, limited as they are by one-period analysis. The comparisons of a wealth tax with expenditure and income taxes are also nonconclusive.

The chapter on general equilibrium aspects of budgetary policy is conducted in terms of a two-factor, two-commodity model.

Discriminatory taxes destroy the equality between the marginal rate of transformation in production and the marginal rate of substitution in con-

sumption. The distribution of income depends on the output-mix of the economy when the production functions are of different factor intensity. Also, when the two groups have different spending propensities the effect of any tax or expenditure policy will depend on a complicated interrelation between source of income and use of income considerations.

The basic objection to Williams' treatment of this area is his frequent use of a highly dubious analytical procedure often used in the literature, in which it is assumed that taxes are collected and then returned to taxpayers in the form of lump-sum subsidies. The usefulness of this device presumably depends on some notion of a tax illusion which would make the taxpayer unaware of the fact that the subsidy he receives is exactly equal to the taxes he pays. Otherwise it is difficult to see why a tax on a particular commodity should change the pattern of demand, as demonstrated by Williams, while an income tax will not. Why should an informed taxpayer change his demand for food if he knows that the size of the accompanying subsidy depends on the amount of food he buys and is exactly equal to the tax he pays? More generally, the chapter is disappointing as it is an indication of the underdeveloped state of the general equilibrium approach to budgetary questions. There seems to be no advance beyond such statements as the one found on page 217:

... we might say that some will be better off under the indirect tax, and others under the direct tax. It should be added that it is quite conceivable that both groups would be worse off under the direct tax. ... It depends to a large extent upon the relative magnitude of the income shifts due to factor price changes compared with the effects on consumption through price changes.

The problems are, of course, complex, yet the lack of progress is in large measure due to the shortcomings of graphical analysis. The alternative approach is algebra, which often yields expressions so complicated that they defy qualitative interpretations. However, Harberger's recent work on the incidence of the corporation income tax shows that an empirical application of such results can yield fairly definite conclusions or at least narrow considerably the range of possibilities.

*Public Finance and Budgetary Policy* is in some respects an incomplete and uneven book. There is no distinct treatment of tax incidence. The sections on stabilization policy and growth are thin relative to other parts of the book, and the difficulties associated with choosing a government budget of the "right" size and composition are by-passed. Furthermore, the complete lack of institutional and empirical material weakens the usefulness of the book as an introductory text. Yet, Williams' contribution would probably have been less significant had he tried to be more comprehensive. For by limiting his range he comes to grips with a number of complications and special cases that are by-passed even in the more advanced treatises. Williams has produced a valuable addition to the very small number of competent books on the theoretical aspects of tax and expenditure policy.

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### International Economics

*The United States Balance of Payments in 1968.* By WALTER S. SALANT AND OTHERS. Washington, D.C.: The Brookings Institution, 1963. Pp. xvii, 298. \$5.00; paper, \$2.95.

Ours has been called the age of the critic. None should find this designation more apt than Walter Salant and his co-authors of the "Brookings report." Their attempt to forecast the U.S. balance of payments in 1968 has drawn a flood of comments, including a Joint Economic Committee hearing and a compendium containing no less than 68 statements,<sup>1</sup> as well as those in the standard reviewing media. Probably the ratio of critical and exegetic pages to pages in the original run up by the Brookings report approaches that of Joyce's *Ulysses*.

Long-run forecasting of the balance of payments is, as the authors note, an especially difficult task. When you forecast GNP six years hence, you are fairly sure that it will increase. But the balance of payments is a sum of pluses and minuses, and the chances of getting a forecast with a reasonable confidence interval (assuming, contrary to fact, that long-run forecasters use standard errors) which lies on one side of zero are small indeed. Yet policy decisions relating to payments disequilibria need to rest on the expected future situation and not on past history alone. Furthermore, the one major previous attempt to forecast the U.S. balance of payments, published in 1957 by Sir Donald MacDougall,<sup>2</sup> concluded correctly that the U.S. balance would deteriorate if other countries succeeded in keeping the movements of their price levels in line with that of the United States; however, he largely threw away this conditional forecast by regarding such price restraint as unlikely. Apparently, balance-of-payments forecasting is both necessary and possible.

The Brookings study focuses upon the so-called "basic" balance, excluding short-term capital movements. The underlying model is as follows. Current-account transactions (except interest and dividends) depend mainly upon the growth of GNP and changes in the price levels of the United States and Western Europe, with the exact relations drawn from equations fitted by Polak and Rhomberg of the IMF to 1948-60 data. The authors ingeniously simplify their task by assuming that no significant changes will occur in the international reserves of other areas. Thus, no net changes take place in the over-all balances of "third" countries, and any changes in their international transactions with the United States are then assumed either to leave the U.S. basic balance unchanged or to wind up affecting the basic balance of the Western European countries. Outside of the current account, the balance

<sup>1</sup> U.S. Congress, Joint Economic Committee, *United States Balance of Payments*, hearings, 88th Cong., 1st sess., Washington, 1963; U.S. Congress, Joint Economic Committee, *The United States Balance of Payments: Statements of Economists, Bankers, and Others on the Brookings Institution Study. The United States Balance of Payments in 1968*, Joint Committee Print, 88th Cong., 1st sess., Washington, 1963.

<sup>2</sup> Donald MacDougall, *The World Dollar Problem: A Study in International Economics*, London 1957.

on long-term capital is estimated from prospective changes in returns to capital and in gross foreign assets here and abroad. U.S. government military and aid transactions are based on projected plans of the government agencies involved. Throughout, considerable care is taken to include the indirect effects, or "feedbacks," of changes in a given class of transactions upon other elements in the balance of payments.

Besides the structure of the model itself, a major question concerns the estimation of the independent variables for these functional relations. Operating under instructions from the Council of Economic Advisers, the Brookings group assumed that U.S. GNP will grow at 4.8 per cent annually between 1960 and 1968, in conformance with government growth objectives; furthermore, the Council specified the assumption that prices (the implicit deflator of GNP) would rise at 1.5 per cent annually. The GNP increase for Western Europe was also drawn from stated policy objectives rather than independent projections, in this case the average of the "plan targets" submitted by the European nations to OECD. The behavior of European prices, an annual increase in the GNP deflator of 2.75 per cent, was deduced in the light of the growth in productivity per worker necessary to achieve the GNP growth objectives and the changes in money wages and the distribution of income which might be expected to accompany the growth of productivity.

This use of government targets for GNP growth, as well as the plans of U.S. government agencies for economic aid and foreign military expenditures, makes the Brookings estimate a highly conditional one. The authors themselves found these official targets "rather optimistic," and developed a projection based on alternative assumptions of the same inflation but slower growth for the United States, and less of both growth and inflation for Western Europe. (The authors also feel that projected U.S. economic aid is too high, but a reduction of this forecast would have had little effect on the predicted balance, once all feedbacks are taken into account.) The alternative assumptions give rather different results from the initial ones, and the difference has considerable importance in interpreting the Brookings conclusions. Under the initial high-growth assumptions, the U.S. basic balance improves to a surplus of \$1.9 billion in 1968; under the alternative assumptions, it remains in deficit, but only by \$0.6 billion. But the basic deficit was only \$0.8 billion in 1961, although \$2.1 billion in 1962, and \$2.4 billion through the first three quarters of 1963. The alternative projection is not much of an improvement, and it matters considerably which one we take.

Most of the difference between the projections based upon the initial and alternative assumptions is due to the differing rates of inflation. This fact causes concern about the basic statistical inputs, since the Brookings group had to use a large amount of guesswork in setting a price elasticity for European imports from the United States. In any case many commentators have declared that the Western European countries would never accept the rate of inflation associated with the Brookings initial assumptions. Yet the inflation rate is deduced directly from the assumed growth rate of real GNP. This view really asserts that the Brookings alternative assumptions are the right ones to take. Given the general tone of the authors' discussion, one might ex-

pect the Brookings group also to come down with the alternative projections as their unconditional forecast. But they do not: "Our best guess is that the basic deficit will be eliminated. If the initial assumptions come close to being realized, there is a definite possibility that a significant basic surplus will develop" (p. 230). Given both the uncertainty about actual domestic and export prices, as well as the uncertainty about the values of the crucial price elasticities, it seems wiser to take the Brookings estimates as suggesting an improvement in the basic balance by 1968 rather than as suggesting elimination of the basic deficit.<sup>3</sup>

This issue of the interpretation of the Brookings findings takes on great significance as we turn to the report's concluding chapter on policy findings, an addendum to the original report prepared for the Council of Economic Advisers. Noting that the United States still holds large usable reserves, if the gold reserve requirement against Federal Reserve notes were suspended, they recommend that no further steps be taken to improve the U.S. balance aside from measures which are desirable for their own sake. This recommendation is stated flatly (p. 253) despite the small minimum improvement under the alternative assumptions and the authors' view that the favorable trend may not show up until 1965 (p. 231). While they state eloquently the case for increasing international liquidity (coming close to the Harrod position that desirable domestic policies should never have to be disturbed to protect the balance of payments), they also give blessing to the progressive hardening of the "adjustable peg" of Bretton Woods into a system of absolutely fixed exchange rates. This choice of an unlikely over a likely corrective, for a problem which shows small signs of solving itself quickly, sounds like a dangerous compounding of probabilities.

Whatever one makes of the policy conclusions from the Brookings study, the basic analytical work deserves considerable respect for its unusual care in keeping track of the interrelations and feedbacks among variables. It does not, however, go all the way. The decision to concentrate on the basic balance excludes short-term capital movements from consideration, although recent statistical work has suggested that they interrelate significantly with the current account through export financing. More fundamentally, as Harry Johnson has pointed out, the model suppresses the monetary and financial structure of the economies in question and the financial policies of their governments. Yet balance-of-payments equilibrium is basically a monetary phe-

<sup>3</sup> Powerful additional support for a less cheerful interpretation of the Brookings conclusions comes from a revision of the Polak-Rhomberg model, extending its data span to 1948-62 and altering some of the data sources. Using the coefficients of the revised model, the improvement in the U.S. current account from 1961 to 1968 comes out \$2.6 billion less than the Brookings estimate under the initial assumptions, \$0.8 billion less under the alternative assumptions. It seems doubtful that a full revision of the Brookings forecasts would show a favorable basic balance under the initial assumptions, and would certainly not do so under the alternative assumptions. Furthermore, the fact that the revised Polak-Rhomberg model affects the current account less under the Brookings alternative assumptions than under the initial ones may provide further support for putting greater weight upon the former. For details, see Rudolf R. Rhomberg and Lorette Boissonneault, "Effects of Income and Price Changes on the U. S. Balance of Payments," *I.M.F. Staff Papers*, March 1964, 11, Table 8.

nomenon. Monetary policies and short-term financial flows may adjust passively to changes in real variables and cost-determined changes in the general price level, but one can hardly be blamed for wishing to see this proved rather than assumed.

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*The London Capital Market and Australia 1870-1914.* By A. R. HALL. Canberra: The Australian National University, 1963. Pp. xiii, 221. 30s.

This work is a doctoral dissertation written in 1951 at the London School of Economics and published in 1963 without substantial modification except for the addition of a brief theoretical postscript. The study is primarily concerned neither with "attempts to prove a particular theory of the mechanism of capital transfer" nor with "the effects of changes in the rate of flow on the course of events in the Australian economy," but rather with "the institutions responsible for raising capital in Britain and the factors on the supply side influencing the flow to Australia . . . as one aspect of the flow of British funds overseas." It is therefore likely to be of greater immediate interest to the economic historian than to the economic theorist, except to the extent that it serves the latter as a convenient source of facts and data to theorize about. The principal conclusion drawn by the author is that the interaction of events in the lending area, London, with events in the borrowing area, Australia, rather than the events in either area taken by themselves, must be taken into account to explain the international flow of developmental loans. There seems to be no reason to challenge this conclusion.

Chapters 1-3 survey the types of borrowers, the characteristics of the securities, and the classes of investors in the London capital market for the period of the study. New issues from domestic companies were relatively insignificant. Overseas lending was shifting from Europe to the less-developed areas in Africa, Australia, and the Near East. Increased use was being made of preference shares and debentures, denominations were being made smaller, and less emphasis was being placed upon unpaid shares subject to call, all of which tended to appeal to a wider range of investors. Growth in facilities for marketing large issues, e.g., underwriting syndicates, and the growing popularity of highly marketable securities favored overseas borrowers.

Australian borrowing in London is the subject of Chapters 4-6. Government issues, particularly for railroad construction, made up a large part of the total. Banks, land mortgage companies, and mining companies were also among the borrowers. The section describes temporal fluctuations and geographical shifts in investment from London and explains them in terms both of supply and demand considerations. Of particular interest is the relationship between Australian politics and British investment there after 1890. Anticyclical fiscal policies, such as public works (in which Australia led Western Europe and North America by some forty years), helped to sustain prosperity and hence the demand for capital. But the same policies aroused the suspicions of the British investor of the day, appearing to him as fiscally ir-

responsible, and therefore contributed substantially to the loss of favor which Australian securities, especially governments, suffered in London after 1900.

The book is clearly written, concise, well documented, and well organized, though slightly repetitive. To the reader primarily interested in broad analytical interpretation and in the building of theoretical models parts of the work can be somewhat tedious, though in view of the author's clearly stated purposes, this cannot fairly be taken as a serious fault. The clear exception to this judgment is the eight-page 1962 "Postscript" (Chapter 8) which deserves special comment in spite of its brevity. It is a general statement (partly attributed to other writers) of the process of foreign capital investment in a developing country, including effects upon the terms of trade, the balance of payments, and the sequence of changes in the direction of investment within the borrowing country, especially between "traded" and "non-traded" goods. The analysis is applied interestingly, though sketchily and with qualifications, to the Australian economy for the period under study. Investment is first attracted by the comparative advantage of the land-using industries, e.g., wool and mining. The subsequent stimulus to other lines of economic activity, e.g., building construction, open new opportunities for investment, which, if extensive and persistent, may boost prices and wages, put exporters at a competitive disadvantage in foreign markets, and thus give rise to balance-of-payments difficulties, particularly after the inflow of foreign capital comes to an end. This effect may be retarded if the capital flow is accompanied by the immigration of labor, which keeps wages down. The analysis is not radically new, but taken in conjunction with the historical data at the command of the author, it suggests some intriguing possibilities for a more thorough-going integration of historical fact and economic theory.

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### **Business Finance; Investment and Security Markets; Insurance**

*The Effect of Capital Structure on the Cost of Capital: A Test and Evaluation of the Modigliani and Miller Propositions.* By ALEXANDER BARGES. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. xii, 121. \$4.50.

Professor Barges' monograph, one of the five dissertations selected for publication in the 1962 Doctoral Dissertation Competition sponsored by the Ford Foundation, is both rewarding and disappointing. It is rewarding in that the author's investigation into the problems of measuring the relationship between the cost of capital and capital structure is more thorough than that of his predecessors. It is disappointing in that much of the theoretical discussion is devoted to a rather fruitless search for flaws in the logical structure of the Modigliani-Miller (M-M) argument. A more judicious and useful book might have been forthcoming if the author had been content to begin his theoretical exposition with the observation that the real world is much more complex than the world of the M-M propositions.

The empirical section of this work is the more significant contribution. Barges points out that where stock yield is regressed on the ratio of debt to

equity, the errors introduced by sample heterogeneity with respect to riskiness and size are nonrandom if equity is measured in terms of market value. The positive relationship between stock yields and debt-equity ratios that was found by M-M must, therefore, be spurious to some extent. To avoid these problems Barges proposes that regressions be run in terms of book value rather than market value. Such regressions will test the M-M Proposition II insofar as the only question asked is the existence or nonexistence of a positive relationship between stock yield and debt-equity ratios—not the size of the relationship. Using data from each of three industry samples (Class I railroads, cement companies, and department stores), Barges finds that the evidence for a positive relationship between stock yield and leverage tends to disappear when debt and equity are measured in book terms. For each sample the regression coefficients are not significantly positive. Furthermore, in each case such coefficients are less than the regression coefficients obtained from regressions on data given in terms of market value, although the statistical significance of the differences is not discussed. It is barely possible that Barges' findings can be explained in terms of a collinearity between leverage (in book terms) and risk, the firms with the lower debt-equity ratios also being the riskier enterprises in general. Yet whatever the true relationship, Barges has clearly shifted the burden of proof of Proposition II back to the M-M camp.

Barges' tests of the M-M Proposition I, the statement of the relationship between the average cost of funds and the ratio of debt to total value of the firm, are less conclusive. His regressions on the sample and various subsamples of Class I railroads suggest that the above variables are negatively related. While these findings are fairly straightforward, the conclusions to be drawn from them are not. It does not follow that an unambiguous finding of a negative relationship between the average cost of funds and the ratio of debt to total value of the firm makes the M-M position untenable. The null hypothesis that Barges should have tested in this case is not the simplest version of the M-M Proposition I but Proposition I as amended to conform to the realities of present-day tax policy. Since the amended Proposition I states that the average cost of capital is negatively related to the ratio of debt to total value of the firm, the relevance of Barges' findings to an investigation of the empirical validity of the M-M theorems is not clear.

Having shown that traditional hypotheses are more in accord with the observed data than are the M-M hypotheses, Barges proceeds to examine the theoretical basis for the M-M propositions. His main conclusion is that the M-M propositions depend upon a particular set of assumptions about risk preference. This conclusion takes two forms. The first is a restatement of the observation by Durand that the arbitrage process which M-M portray may involve an extension of the scale of the investor's holdings and thus a difference in the risk equivalence of the pre- and postarbitrage portfolios. This criticism applies to the situation in which the value of a levered firm is greater than the value of (an otherwise identical) unlevered firm. This is valid as an attack on the particular arbitrage scheme proposed by Modigliani and Miller, but it does not get to the heart of the matter since an alternative

arbitrage scheme can be suggested. If the owner of (overvalued) levered stock has bonds in his portfolio such that the ratio of his bond income to the income of his levered stock is at least as great as the ratio of bond interest payments to stockholder income for the levered corporation, the owner of the levered stock will find it to his advantage to sell a combination of levered stock and bonds and purchase unlevered stock. The necessity for margin borrowing which M-M foresaw derived from the tacit assumption that there was no portion of the portfolio of the owner of the levered stock that was not levered. Such an assumption is clearly too restrictive.

Barges' second attack on the theoretical bases of the M-M propositions amounts to a charge of overdeterminacy. Modigliani and Miller postulate a set of discount rates that the market deems compensatory for varying degrees of risk—risk being expressed in terms of mean-variance ratios. Yet, Proposition II also specifies a relationship between returns of various risk classes (stocks of differing degrees of leverage) that is apparently independent of this initial set of discount rates. What is to insure the consistency between the returns deemed appropriate according to Proposition II and according to the initial set of rates for the various risk classes?

The appeal of this argument derives from the fact that Modigliani and Miller chose to represent the rate of discount appropriate to a pure equity stream as a single rate chosen from the initial set of rates that specifies the market's reaction to risk. This is somewhat misleading. Let the postulated set of discount rates,  $r_0, \dots, r_n$ , be defined such that  $r_0$  is the rate of return appropriate to an income stream expected with full certainty and the rates  $r_1, \dots, r_n$  are appropriate to income streams that are uncertain, the outcomes being defined in terms of probability distributions that are characterized by differing mean-variance ratios. Further, let the probability distributions that are associated with the rates of return  $r_1, \dots, r_n$  be defined such that it is uncertain that a positive income stream will be forthcoming. Then any given income stream "X" can be understood as the sum of " $A + Y$ ," " $A$ " being a certain value and " $Y$ " being a stochastic variable defined by a probability distribution for which the least attractive income stream that is possible is zero. The rate of return appropriate to a pure equity stream is thus a function of  $r_0$ , the  $r_j$  appropriate to that part of the expected income stream that is uncertain, and the proportion of the expected income stream "X" that is uncertain. If both the rates of return to a pure equity stream and the rates of return to levered equity are thus understood to be weighted averages of rates drawn from the set  $r_0, \dots, r_n$ , the appropriate weights being established by the proportion of the income stream that can be expected with certainty, the spectre of overdeterminacy vanishes.

The most fruitful of the criticisms that Barges has leveled at the Modigliani-Miller constructs is the charge that the M-M Proposition II is a hybrid. That is, M-M have defined interest payments in current terms while payments to equity are defined in terms of expected value. This is an important observation and deserves following up, for it points up the fact that the M-M model does not deal with such complexities as the expectation of change in the structure of the rewards to risk-bearing, shifts in price expectations, and

changes in the distribution of price expectations among investors of differing attitudes towards risk. The M-M postulates give every appearance of being logically consistent, but they apply to a world that is notably simpler than the real world. Furthermore, they remain, even on their own terms, a set of normative propositions. Whether they are useful as positive propositions remains in doubt.

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### **Industrial Organization; Government and Business; Industry Studies**

*Concentration in the Manufacturing Industries of the United States—A Mid-century Report.* By RALPH L. NELSON. Economic Census Studies, No. 2. New Haven and London: Yale University Press, 1963. Pp. xiv, 288. \$7.50.

This study was sponsored by the Committee on Analysis of Economic Census Data appointed by the Social Science Research Council. It is the second in a series of monographs based on the economic censuses of 1954.

More than half the book consists of tables of basic data prepared by the author. These include various measures of both company and plant concentration for a large number of manufacturing industries for the years 1947, 1954, 1955, and 1956. The concentration measures are based on value of shipments as the measure of both market size and firm or plant size, but concentration in terms of value-added, total employment, and production worker man-hours is also shown. In addition to these statistics based on "four digit" industries, concentration measures for "five digit" product classes are given, on both a company and a plant basis.

The first hundred pages of the book present descriptive and analytical findings, supplemented by a very thorough discussion of the methods used in processing the data and the problems that were met.

Ralph L. Nelson suggests that the well-known finding that large industries tend to have lower concentration than small industries reflects *in part* the tendency of statistical classification systems to make large industries more heterogeneous in the products and techniques they include than small industries. The greater heterogeneity of large industries is not actually demonstrated by Nelson but is apparently inferred from the fact that large four-digit *product classes* tend to have more five-digit subclasses than the small. Even this relation, however, does not necessarily demonstrate the greater heterogeneity of large product classes but could conceivably be due to the practical desirability of avoiding very small five-digit classes.

A comparison of concentration in 1947 and 1954 shows that the weighted average of concentration ratios changed very little indeed over the period, but that there were large changes in concentration ratios for individual industries. These results are very similar to earlier findings for the period 1935 to 1947. The changes in concentration for individual industries are negatively correlated with industry growth, but the correlation is weak.

In comparing plant and firm concentration Nelson finds that the leading firms tend to have both larger plants and more plants than the rest, but that while the tendency for leading firms to have more plants is more pronounced in highly concentrated industries, the tendency for leading firms to have larger plants is not correlated with concentration.

There is a minor error in Nelson's discussion of the importance of multi-plant operation in different industry groups (Table 4.3). He measures the number of plants "with sister plants in the same . . . industry" by subtracting the number of firms from the number of plants. This difference, however, only sets a lower limit to the number of plants "with sister plants in the same industry," and the actual figure may be quite different. If there are five firms and ten plants, the number of plants belonging to multiplant firms must be more than five. But it will be ten if each firm operates two plants, and only six if one firm operates six plants.

The book includes a fairly inconclusive study of the differential characteristics of the four leading firms in each industry and a discussion of the changing composition and characteristics of the 50 largest manufacturing firms.

Some of Nelson's statistical experiments seem unnecessarily cumbersome, and the style of writing does not make for easy or pleasant reading. The book as a whole, however, represents a valuable addition to the material on concentration available to research workers.

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### Land Economics; Agricultural Economics; Economic Geography; Housing

*The Economics of Highway Planning.* By DAVID M. WINCH. Toronto: University of Toronto Press, 1963. Pp. ix, 158. \$3.95.

"This book attempts to extract from economic theory the best guidance available on [the highway planning and pricing] problems and to present it in a form which can be understood by the non-economist" (p. x). The first two-thirds of the study is devoted mainly to setting up an analytical framework for measuring highway benefits and for determining optimum investment programs. The remainder deals mainly with problems of allocating the highway-cost burden among groups of users, property owners, and the public at large.

As is the practice of most public agencies in analyzing their investment projects, only publicly borne costs are incorporated in the denominator of the benefit-cost ratios that highway engineers compute for proposed highway improvements. The numerators, then, are the savings it is predicted the improvements will afford their users. Winch objects to this approach. Since traffic volume is a function of trip costs, the traditional procedure makes it difficult to compare alternative plans involving different user costs. Also, it gives no clues as to the optimum traffic volume on a highway. He therefore proposes an approach which, in effect, recognizes that a highway user operates on both the supply and the demand sides of the market for trips. Un-

der his system, the task a highway authority should set for itself is to build that highway network which maximizes the area between demand and long-run marginal cost schedules, where "costs" include not only those borne by the highway authority but also the vehicle operating, time, and other costs sustained by highway users and the public at large.

In an appendix, Winch attempts to justify this advocacy of what is essentially a "competitive" highway investment criterion in light of the arguments of Lipsey, Lancaster, *et al.* against competitive pricing in some sectors when monopolistic pricing cannot be eliminated in others. The main points in his argument for a marginal benefit cost ratio of unity (in the long run) seem to be (1) that the resources involved in a highway investment program would be diverted mainly from sectors that are approximately competitively organized; and (2) that, contrary to the practices of the private sector, the highway authority would (or at least should) include such technological externalities as traffic noises and smells in its cost calculations.

In dealing with how best to distribute the burden of highway costs, Winch finds considerable fault with the melange of allocation principles involved in the highway literature. Where cost responsibility can be assigned to individual users or other members of the community, he advocates doing so. The remaining costs—by far the largest group, he suggests—should be allocated so as (a) to optimize the use of highway facilities; (b) to preserve "the normal balance of competitive forces" among transportation modes; and (c) to distribute the burden equitably among those who benefit.

For reasons that are never clearly specified (perhaps because he regards them as self-evident), Winch maintains that the system of trip-related charges—mainly gasoline and tire excises supplemented where feasible and desirable by direct tolls—necessary to achieve the closest realistic approximation to the first of these goals would not yield sufficient revenues to cover the costs of an optimum highway network. On grounds that "it is probable that more traffic would be discouraged in the form of marginal trips by all users than of marginal users altogether" (p. 137), he advocates relying on license fees rather than additional trip-related charges to make up the presumed deficit.

My principal substantive objection to Winch's study is related to this insistence that congestion tolls would cover only a small part of the costs of an optimum highway system. Congestion tolls are the analogue of the quasi-rents received by fixed capital in a competitive industry. In long-run competitive equilibrium, by definition, quasi-rents equal capital costs. Essentially the same consideration applies to highways. Congestion tolls would equal capital costs for an optimum system as Winch defines it *if* the production function for trips on a given highway involves constant returns to scale and *if* inputs were purchased on competitive markets. True, the limited available evidence does suggest that the real-world production function involves increasing returns. However, the cost of rights of way and other highway construction inputs seem typically to increase with highway size. While the former influence may well more than offset the latter, it is still by no means obvious that a substantial difference would exist between optimum trip-re-

lated charges and the costs of an optimum highway network. But even if Winch's view of the relation between these two magnitudes is valid, his "equity" and "competitive balance" standards would still militate against heavy reliance on license fees—discriminating, as they do, against infrequent users of highway services.

A large part of the first two-thirds of the study is devoted to sorting the effects of highway investments into "cost," "benefit," and "irrelevant" categories and to discussing ways of attaching dollar values to those costs (the value of travel time, in particular) that are not directly reflected in market transactions. The validity of most of his conclusions on these matters seems indisputable. Unfortunately, however, they are stated in a manner that is unlikely to convince the engineers that Winch numbers among his audience. For example, the highway literature is replete with claims that the increases in land value in the vicinity of improved highways should be added to the numerator of benefit-cost ratios. Dismissing these putative benefits with a bald statement to the effect that including them would entail double counting of benefits transferred from their initial highway-user recipients is unlikely to have much effect.

In brief, a considerable number of obstacles—not the least of which is the reluctance of civil engineers to accept the sage counsel of economists—stand in the way of applying the Winch analytical framework. Nevertheless, he has performed a valuable service by demonstrating that the partial equilibrium framework we use to analyze competitive markets can be employed without fundamental alteration to the highway investment problem.

HERBERT MOHRING

*University of Minnesota*

*Recursive Programming and Production Response.* By RICHARD H. DAY. Contributions to Economic Analysis, XXX. Amsterdam: North-Holland Publishing Co., 1963. Pp. xiv, 226. \$6.00.

The latest monograph in "Contributions to Economic Analysis," a series devoted to the practical application of economic analysis, is an exemplar of the "judicious blend of theory and fact" that its author, Richard Day, takes as his standard. The study reports on an extension of dynamic programming to a sequential optimization process. The application is to farm production data from the Mississippi Delta for nine crops. An area supply functional is estimated by explicitly taking into account the optimizing behavior of the firm decision-makers and their institutional as well as economic environment. Some topics of purely theoretical interest are introduced en route to the solution of the practical problem. But throughout the book, the goal of predicting the complex response of the region's farm output to market, technology, and program is kept to the forefront. Extensive data and technical problems are relegated to appendices where they are fully treated. Misprints are numerous but minor.

Day sets this study in perspective with other studies of supply estimation by pointing out that early procedures which statistically related the supply

of single agricultural commodities to prices and techniques were essentially short-run models. A more dynamic approach to farm supply was taken by Marc Nerlove, who adapted and applied the work of Koyck and Cagan on distributed lags to the estimation of long-run supply elasticities. James Henderson, on the other hand, explicitly recognized that production response underlies market supply and estimated the changing farm land use that would gradually adjust toward maximizing expected net revenues. A brief exposition of these two approaches is given in Chapter 1, and Chapter 2 extends the Henderson model into a class of dynamic programs by means of simple examples clearly explained. Chapter 3 develops the general properties of these recursive programs in a manner rather difficult for the nonmathematician. However, the material serves to demonstrate the abstract rationale for the specific problem attacked and to classify the model as dynamic in the Frisch-Samuelson sense, i.e., its evolution over time is determined recursively by the periodic changing of target functions and constraints.

The next four chapters present the method's application to data for the joint output of nine crops in the Mississippi Delta. Briefly summarized, recursive programming is exhibited as the period-by-period optimization of a net revenue target whose values are the amounts of commodity produced in the period (or acreage times yield) multiplied by the net per unit returns of each commodity lagged one period. The net returns can be related to product demand and input supply equations for a closed dynamic system. The optimization process is constrained by total land supply, acreage controls and, recursively, by coefficients of maximum and minimum adjustment to optimal land use and investment. Technology is represented for each period by a matrix of input requirements and output yields for each unit process level. Processes are classified for each crop combination by five technology stages (major types of input combinations), by 36 input categories, by three soil types, and by a continuously variable yield response function approximated by four discrete levels of nitrogen fertilizer application. The matrix contained 37 distinct processes from 1940-45 and 103 from 1946-57, with 1945 the approximate date of innovation for rice production, cotton-picking machines and self-propelled combines. Solution proceeds repetitively by solving each period's linear program on the basis of the initial conditions generated by the previous program or by the starting data. The model "predicts" acreage well but yield poorly, due mainly to what Day characterizes as the skewed effects of weather conditions. The study terminates with 70 pages of appendix, including data sources, estimation techniques, and a complete tabulation of model inputs and results. References are listed but no index is provided.

The author admits that taking no account of the random effect of weather on yield badly upsets the results. Yet, if the model is intended to predict rather than explain, nothing can be done unless there is evidence of nonrandom weather runs. Perhaps the model could have been converted to a stochastic one after it had generated acreage response to put confidence intervals on yields as they are affected by weather variation.

The coefficients of output and investment adjustment are key parameters of the model. Most of the multiple phasing is conditioned by these con-

straints which are exogenous to the model. Yet there remains some circularity in the method of their estimation. For each quantity assumed subject to an adjustment constraint, a subperiod is selected for which the upper or lower constraint is hypothesized to be operative. Statistical estimates are formed from this period and used in the recursive program. The program's results are compared with actual data and, if the "fit" is not good, new subperiods are chosen for selected quantities to form a second set of adjustment coefficient estimates. The iteration continues in the hope that the program will converge to what Day calls a "meaningful structure," namely, one that accurately predicts succeeding data from the starting conditions. Thus, theoretically, it is no longer a predictive model of producer reaction to change. It becomes a method for estimating those coefficients of producer flexibility and innovation diffusion which best explain the actual response of producers under the other assumptions of the model. It is only fair to add that, due partly to the expense of numerous iterations, the above method was only repeated once to correct *ex post* for incomplete specification of the constraints.

In spite of these criticisms, this reviewer feels that the book is a worthwhile addition to the library of any research economist. Criticism and improved research are easy when rationale and results as well as data are laid honestly before the reader. The theory used has been generalized to an extent that makes it potentially applicable to many problem areas. The specific problem has been attacked with a care for detail and fact that is seldom observed in the writings of those proficient in complicated new techniques.

E. J. R. BOOTH

*Oklahoma State University*

*North Atlantic Policy—The Agricultural Gap.* By JOHN O. COPPOCK. New York: Twentieth-Century Fund, 1963. Pp. 270. \$4.00; paper, \$2.25.

John Coppock, now with the Food Research Institute at Stanford University, has done a much-needed analytical job. He has examined the agricultural policies of the European Common Market countries with reference to several considerations: the over-all economic, political, and military objectives of the Common Market; the wisdom of agricultural resource allocation within the Atlantic community; and the prospect for American farm exports to the Common Market countries.

With respect to all of these considerations, Coppock finds European agricultural policy to be unwise. "Protectionist," "autarkic," and "cartelized" are the words he uses. The case is best stated in the author's own words. Concerning over-all objectives he says, "European unity and greater Atlantic integration would be one of the great events of history. To jeopardize it, even to slow its arrival, by disagreement over agricultural policies is an act of political irresponsibility" (p. ix). On resource allocation he says:

It must be said that the management of Western agriculture . . . has been a failure. On the face of it, there are few economic phenomena to match in absurdity the piling up of huge surpluses of agricultural

products in the United States over the past decade; or the continued production of inferior wheat in Switzerland at about \$4 per bushel, when good Canadian wheat could be imported for about half that amount; or the ludicrous situation of Britain's dumping on the international market in 1957 some 100 million eggs (while importing even more), produced under a subsidy which probably accounted for all of the producers' net income derived from the sale of the eggs . . . (pp. 23-24).

On prospects for U.S. farm exports to Europe:

There seems to be little doubt that the agricultural industries of the European countries, under present policies or those likely to be adopted, are capable of expanding supply at a rate considerably in excess of that expected for demand. It would appear that output can easily be increased at a minimum rate of 2 per cent or more annually—almost double the anticipated rate of rise in demand. If this should take place, European net demand for imported non-tropical foods and feeds would be halved by some time in the late 1960's and would virtually disappear over the following decade (p. 173).

Thus Coppock sees a strong possibility that U.S. agriculture will lose its best export market, a cash market which has been accounting for more than a billion dollars worth of farm products annually. The data on which this analysis is based come from the Ministries of Agriculture in the various countries, from the Food and Agriculture Organization, from the Economic Commission for Europe, the Agricultural Division of the Economic Commission for Europe, and the Organization for European Economic Cooperation. The data have been handled with considerable care. Coppock is experienced in economic analysis, having co-authored (with J. Frederick Dewhurst and P. Lamartine Yates) the recent major Twentieth-Century Fund study, *Europe's Needs and Resources*.

Coppock attributes the prospective increase in European farm production to price policies, adopted for political reasons, which provide farmers with excessive inducements to increase output. He offers a corrective: reduced levels of price support. For example, he proposes that support prices for wheat be reduced below 1960 levels by the following percentages:

Switzerland	—45	Netherlands	—20
Germany	—35	Sweden	—20
Italy	—35	France	—15
Norway	—35	Denmark	—10
Austria	—30	Canada	—5
Belgium	—25	United Kingdom	—5
United States	—23		

Price reductions of this magnitude would be expected, Coppock says, to deter wheat production in most of the countries of Europe. In the United States the effect would be to reduce wheat production somewhat. There

would be no deterrent effect in Canada. The effects on farm income of such changes, says Coppock, "generally are not catastrophic" (p. 200).

The book is well organized, is quite readable, and is amply stocked with well-composed tables.

What can one say of Coppock's diagnosis? This reviewer agrees that the Common Market may indeed founder on agricultural policy. He agrees that the allocation of agricultural resources in the Atlantic community is irrational. However, though impressed with Coppock's scholarly work, he is unready to concede the loss of the U.S. farm export market in Europe. He doubts that, even with the inducements supplied by European farm policies, the farmers of Europe would, simultaneously, feed more people, upgrade diets, and replace imports from the United States. Admittedly, hope may be an ingredient in this judgment. The directional effect on production and trade of autarkic agricultural policies is clear enough, although there may be some dispute as to the degree of influence.

Whether the economic sanity and political courage needed to effectuate Coppock's remedy are to be forthcoming is a judgment left to history. In any case, Coppock has done a piece of work that deserves reading on both sides of the Atlantic. The Twentieth-Century Fund has again demonstrated that economic analysis is a necessary, though not a sufficient, ingredient of wise policy formation.

DON PAARLBERG

*Purdue University*

*Housing Markets and Public Policy.* By WILLIAM G. GRIGSBY. Philadelphia: University of Pennsylvania Press, 1963. Pp. 346. \$6.50.

Focusing on selected aspects of housing market analysis which have a direct bearing on housing and urban renewal policies, William Grigsby's book makes a significant contribution. In a field in which market-oriented studies often by-pass policy implications while policy-oriented writings tend to ignore or misunderstand the allocation process through the market, such an integrated effort warrants attention.

In six chapters, the author examines the structure of the housing market, the much embattled "filtering" process, the movement of house prices and rents, some of the variables determining the level and composition of new construction, the maintenance and improvement of the stock of housing, and residential renewal. In nearly every one, Grigsby offers fresh insights or modifications of existing theories and questions the assumptions or objectives of public policy. The analysis ranges from what amounts to a careful crossing of *t*'s to the most comprehensive critique of the urban renewal program yet to appear in the literature. New empirical materials are drawn for the most part from comparison of the 1956 National Housing Inventory and the 1950 Census of Housing, including special tabulations for the Philadelphia area.

If the book leaves the reader with the impression of a series of fragmented, never-fully-integrated essays lacking in incisiveness or common methodology,

Grigsby disarms him by his modestly stated goal. The study is "tentative and exploratory and reaches no firm conclusions. It seeks instead to encourage analysts to view the market differently, pursue new lines of inquiry, and organize existing data in a different fashion" (p. 28). On the whole the author succeeds rather well, though often laboriously, in meeting these objectives.

Grigsby effectively uses matrix analysis to illuminate the interaction of housing submarkets and their demand and supply components, and flow analysis to trace occupancy changes in the various sectors of the market, as well as verbal or simple numerical models. The dynamics of the supply of existing dwelling units, an often neglected subject, is thoroughly analyzed with reference to Census data. As for the performance of the allocative functions of the housing market, the author takes a position, on the whole well reasoned, between the housing reform literature and the *laissez-faire* school (although his performance standards are never made explicit).

Many readers will have reservations on detail. For example, Grigsby seems to place excessive reliance on 1950-56 data for the development of generalizations. Among other things, rents in 1950, the base point of the analysis, were still at controlled submarket rates in most places, even though, as pointed out in the book, control was gradually abandoned during the next few years. Similarly, the finding that "a large portion of the stock . . . improves with age" may reflect conditions unique to the 1950-56 period. This reviewer was more enlightened by the empirical test in Chapter 4 of one concept of filtering (rent or price movement relative to family income change) than by the somewhat diffuse theoretical discussion of the subject in Chapter 3. He was also disappointed by the occasionally premature termination of analysis pertinent to Grigsby's purpose. Considering the reallocation of income in favor of housing, for example, he raises the familiar question of the benefits of alternative income allocations, notes that the question has not received the attention it deserves, and proceeds by giving it no attention either (pp. 304-5). But these blemishes do not detract from the value of the book as a stimulating research document.

By far the most solid chapter deals with residential renewal. Among other things, the author questions the need for the subsidized land "write-down" in the present renewal program, especially in cases of nonresidential redevelopment; highlights the conflicts between national and municipal renewal goals; and points up the lack of rational policy for distributing federal aid, with the result that the program tends "in the direction of pure pork barrel" (p. 327). The dislocation of existing slum dwellers is viewed as a highly inequitable distribution of the burden of the program. "Redevelopment, which was intended to focus upon families least able to cope with their urban environment, has instead distributed its largesse upon all but that group, and indeed forces upon those who need help the most a form of regressive taxation unequalled in American history" (p. 325). In brief, Grigsby calls for thorough reconsideration of the program.

LEO GREBLER

*University of California, Los Angeles*

### Labor Economics

*Real Wages in Soviet Russia Since 1928.* By JANET G. CHAPMAN. Cambridge: Harvard University Press, 1963. Pp. xiv, 395. \$12.00.

This beautifully careful monograph covers "real wages" (i.e., the incomes from official employment and social services, less taxation and net forced loans) of nonagricultural "workers and employees" (i.e., every earner not a peasant, a state farm worker, a soldier, a forced laborer, or, possibly, an artist). It is most definitely *scuola di Bergson*: comprehensive, fair, exact in its definitions, up-to-date in its methods. So scrupulous is the author that she has most unfortunately refused to go beyond 1954, except for a few bare statements; so that she does not illumine one of the most important of all periods.

Chapter 10 is a good discussion of the place of real wages in the national income. Truth to tell, real wage per worker is just one of those inconveniently "full boxes" in economics. It is a long way from consumption per head of population and is frequently quoted in place of the latter through mental inertia alone. We have always used it, and anyway it emerges easily from the statistics commonly collected. Consumption per head is the interesting "box," empty for about a century after the beginning of serious economic statistics.

Real wages are of course indispensable in labor economics: we could not analyze collective bargaining, incentives, or leisure preference without them. But their relevance for macroeconomics has always seemed to me exaggerated, owing to Keynes' original obsession with the question. And Janet Chapman's book is not about either of these subjects but about welfare. It would then in a way be correct to say that she has used the wrong concept throughout. In her case, however, the excuse is undeniable: otherwise she would be in the morass of peasant incomes, and could only keep afloat on a book twice as big.

Now in market economies, even underdeveloped ones, real wages will probably move in fair sympathy with consumption per head. For the social wage and the taxation of the poor are small, and the free labor market is likely to preserve a constant inequality between wages and rural incomes. Labor force participation, trade union strength, and indeed the whole economy are subject to no sudden change. Not so in the USSR; not only must Mrs. Chapman modify her concept for taxation and the social wage, she must also show its relation to consumption per head. She has principally to deal with (1) the proportion of workers to dependants in a family, (2) peasant migration and peasant incomes, and (3) the division of the social wage between peasants and workers.

Consumption per head has held up very much better than real wages in the USSR, partly because so many more women work than used to in 1928. In this connection there is crying need for a domestic or consumption economist to turn her mind to the USSR. Despite her sex the author has not concerned herself with the consequences for true ultimate welfare or, for

that matter, true final output of the nonperformance of housework. We do not eat tin cans, we eat food cooked on the plate. Housework is production. What we buy in the stores is not final output, and I do not hold our indices of consumption per Soviet head to be truly comparable to Western ones, especially in 1928-37, when the real decline in housework took place.

As to the peasants and townward migration, although both peasant real incomes and real wages fell in 1928-37, consumption per head rose considerably (p. 166). Mrs. Chapman deals very well with this, though she might have generalized more explicitly the paradox that two quantities can both decrease while their average rises. Using particular symbols to illustrate the general case, this occurs when, in a static population,

$$\frac{Uu + Rr}{U + R} < \frac{(U + M)(u - k) + (R - M)(r - j)}{U + M + R - M}.$$

I.e., if we weight the urban and rural incomes  $u$  and  $r$ , obtaining in 1928, by their respective populations, the average can be less than that of smaller incomes  $u - k$ ,  $r - j$ , obtaining in 1937, if there is migration  $M$  to the towns and the migrants gain enough. This can be seen to be possible since it follows that:

$$(R - M)j + Uk < M(u - k - r);$$

i.e., the losses of those who remain in the countryside and the towns are less than the increase in the total income of the migrants; which is entirely possible if the original disparity  $u - r$  was great enough (it may have been equal to  $r$  in 1928.<sup>1</sup>)

As to the social services, not all of them are a social wage since a part—a shockingly small part—is paid out to peasants (pp. 128, 135). In the excusable absence of Soviet statistics on this point, Mrs. Chapman's division is rather arbitrary. No likely error here would affect her general orders of magnitude, but at few points is it more clear how "impure" a concept the real wage is from a welfare point of view.

The book contains rich factual information on social services, taxation, housing, rationing, the different kinds of retail market, etc. There are also some interesting technical points, not enough appreciated by practitioners in this field. Thus an index of money incomes divided by a Paasche price index yields a Laspeyres real income index, and vice versa (p. 30). The author also turns her attention, all too briefly, to the problem of hedonic price indices. A new good, she says, entering consumption between the base and the given year, could in theory be incorporated in an index by giving it such a base-year price as would have *just* rendered its consumption equal to zero (p. 33). But why "just"? Why is a price much greater than that incorrect? After all there is supposed to be only one price compatible with the consumption of a positive quantity, but infinitely many prices are compatible with zero consumption, and it is not obvious why the lowest is the most significant.

<sup>1</sup> If the population changes,  $U$ ,  $M$ , and  $R$  on the right-hand side of the first equation can all be raised or lowered by the same fraction. We can then proceed as before.

But still, there was seldom a happier marriage of theory and practice than in this book.

PETER WILES

*Brandeis University*

*"Disguised Unemployment" in Underdeveloped Areas with Special Reference to South Korean Agriculture.* By YONG SAM CHO. Berkeley and Los Angeles: University of California Press, 1963. Pp. xvi, 163. \$3.75.

In this book the author's objectives are three: to study critically the prevailing concepts of disguised unemployment in underdeveloped economies in search of "a more valid concept and theory"; to devise and apply to South Korea a method for "measuring true surplus labor"; and to study the policy implications of the empirical results. In the pursuit of these goals, he provides much interesting criticism of the current literature and a number of enlightening insights into the problems that arise when a specific area is subjected to study. Nevertheless, perhaps as a result of the inherent complexity of the subject, the book does not always live up to the aspired precision, nor are the three major sections integrated altogether satisfactorily.

In his theoretical sections, he accepts the standard definition of the concept of disguised unemployment, that is, the existence of applied labor with a marginal productivity of zero. He is, on the whole, unimpressed with the traditional literature on the subject, from the early work of John Lossing Buck to more recent writings of Nurkse and Lewis. As noted by Viner, Buck's data were inadequate to illustrate a marginal productivity of zero in the Chinese economy. The Navarretes' concept of disguised unemployment arising from cyclical fluctuations in demand (p. 13) "does not seem to be very helpful as a means of distinguishing unemployment in underdeveloped countries from that in economically advanced countries." Nurkse's work is held to be inconsistent with the facts, and his book is open to criticism (p. 17) on the ground that social institutions and congested population "may be the causes of visible idle labor, rather than the causes of so-called disguised unemployment."

These detailed criticisms of the literature are, however, primarily a background for Yong Sam Cho's major attack on the traditional treatment of disguised unemployment, which rests on a fundamental theoretical difficulty inherent in the concept. Specifically, he would like to know (pp. 4, 29) if the people of the underdeveloped world are "really so irrational as habitually to apply labor unnecessarily," that is, to work when the marginal product is zero. Cho's answer is clearly no. Therefore, when he turns to his study of Korean data, he concentrates not on "disguised unemployment," but rather on an alternative source of surplus labor, that is, those who constitute the part-time overtly idle. Thus, the quotation marks in the title of Cho's book are important. This dismissal of the traditional concept of "disguised unemployment" may, however, be looked on with some misgiving. Cho's simple reiteration of the irrationality of work resulting in a zero marginal product is not a conclusive case against authors who have argued that social institutions may develop to perpetuate this type of irrationality. This section on

the theoretical literature raises many questions that need to be answered, but Cho's attempts to do so are not always as tight and precise as they might be.

Before leaving the theoretical arena, the author discusses with approval the model based on the wage-working vigor relationship which has been developed by Leibenstein and others. Because of the malnutrition of those in underdeveloped countries, labor hours are not a homogeneous input; a worker's productivity may be expected to rise as his level of consumption rises. On the whole, this section is reasonably clear. However, because of the importance of the proposition in his general treatment of "disguised unemployment," Cho might usefully have explained in greater detail how this model illustrates (p. 42) that "the marginal productivity of employed workers is positive." Specifically, the reader may wonder why he arrives at a result in line with his earlier skepticism regarding "disguised unemployment," when Leibenstein, with the same basic model, came to the contrary conclusion that "landlords, as a group, are in an improved position if institutional arrangements permit them to employ the entire labor force . . . and yet not utilize the entire labor force" (*Economic Backwardness and Economic Growth*, p. 75). In my opinion, Leibenstein's use of the model is more open to criticism than Cho's; yet Cho has an obligation, which he leaves unfilled, of explaining why there is a difference on a matter of central importance between his conclusion and those of Leibenstein, whose model forms the basis of Cho's discussion.

Given Cho's emphasis on the theoretical implications of the wage-working vigor relationship, it is disappointing that this subject is not given detailed consideration in the empirical chapters, and this is particularly so because the author's medical training provides a suitable background for such a study. In spite of this shortcoming, however, the empirical chapters are among the best in the book and provide a significant contribution to our understanding of labor problems in the agricultural sector of a specific underdeveloped country. Cho estimates the surplus labor as the difference between labor available and labor employed. His conclusion is that unemployment is a seasonal phenomenon: during certain particularly busy times of the year, there is actually a shortage of labor in the sense that there is more work to be done than can be comfortably accomplished by the manpower available. These empirical chapters give evidence of being carefully developed; nevertheless, the results perhaps suffer from overadvertisement. The inherent difficulties of the subject must lead to skepticism regarding Cho's claims to have found the "true surplus labor" or a "reliable standard for the measurement of withdrawable surplus labor." Because workers who are idle at some periods during the year are needed at others, the terms "withdrawable" and "surplus" are themselves not very precise.

This difficulty is in large measure recognized when the author turns to the policy suggestions of his study, which hinge around the use that can be made of the seasonally available labor in the agricultural areas themselves. While these policy conclusions may usefully serve as a basis for further discussion, they raise certain problems. For example, it is not clear that his pro-

posed high rent tax aimed at pushing down agricultural land prices and causing the virtual extinction of absentee landownership (pp. 133-34) would be significantly less disruptive than outright confiscation.

PAUL WONNACOTT

*University of Maryland*

*Some Aspects of Wage Theory and Policy.* By SIDNEY WEINTRAUB. Philadelphia: Chilton Books, 1963. Pp. xi, 254. \$7.00.

Professor Weintraub deals with those aspects of wage theory and policy which are involved in a Keynesian-type aggregative model of income, employment, and prices. The price level receives most attention, and the principal argument is that *changes* in the price level are *caused* by changes in money wage rates. In brief, the book makes the case for a cost-push theory of inflation where unions push wages and wages push prices. The familiar dilemma is posed: How to reconcile the goals of full employment, price stability, and collective bargaining of wages? Weintraub concludes that the goals are in conflict and that negotiated wages must be replaced by a program of wage controls by a government agency.

The orientation of the book is, then, toward the current policy issues of inflation, unemployment, and "administered" wages and prices. Although Weintraub is carried to his policy position by his theoretical model, the case for wage controls could rest on other theories of wage-push inflation. This is worth noting because the theory in this book is rather sketchily presented. There are footnote references to the author's other published works for "more extended treatment" of the theoretical points, but here I would suggest that the criticisms of Weintraub's theory which Abba P. Lerner has made in a review of a previous book should also be examined by the reader.<sup>1</sup> There is still the tendency in this book to present macro-equations as definitions and then to confer them with behavioral significance which is supported, if at all, by meager empirical evidence or simply by the author's assumptions. For example, in Chapter 1 the main point made is the importance of wages and salaries in the consumption function and aggregate demand. But this "importance" simply stems from labor's large share of national income and from Weintraub's *assumptions* that (1) rentiers have near-constant incomes and consumption expenditures in the short run, and (2) wage earners and profit receivers spend 95 per cent and 50 per cent of their respective incomes on consumption.

In one of the equations of the macro-model presented in Chapter 2 the price level is defined as the quotient of average labor costs per unit of output divided by labor's share of output. Since labor's share is held to be constant, it is labor costs that alter the price level. Of course, one could just as accurately substitute the word "capital" for "labor" in the phrases above, but the author is setting the stage for the role of unions so labor costs are the focus of attention. He has already stressed the importance of wages in

<sup>1</sup> Abba P. Lerner, "On Generalizing the General Theory, A Review Article," this *Review*, March 1960, 121-43.

aggregate demand, although no feedback effect from labor costs to aggregate demand is analyzed. The higher prices which follow rises in labor costs are financed, says Weintraub, by increases in velocity and/or increases in the money supply which the government is forced to make to avert unemployment.

The proposition that labor's share, when defined as the ratio of employee compensation to business gross product, is constant over short periods is defended in the next two chapters. The discussion deals with the arithmetical relations of wages moving with prices and employment with output, lending short-run stability to the ratio:  $(\text{wages} \times \text{employment}) / (\text{prices} \times \text{output})$ . There is nowhere in the book any analysis of labor's share in terms of production functions and elasticities of substitution, demand shifts among labor-intensive and capital-intensive goods and services, changing factor supplies, or technological progress that may be labor-saving or labor-using.<sup>2</sup> Nor does Weintraub assess the criticism that the constancy of labor's share may be a statistical artifact resulting from offsetting changes of the shares in the industrial components of the aggregative figure.<sup>3</sup>

In Chapter 5 the author portrays the decade of the 1950's as one with "chronic unemployment" (pp. 41, 150) and inflation. But unemployment was quite low by historic standards from 1951 to 1956, and in the last few years of the decade, as Weintraub admits, "the price level has behaved fairly well" (p. 229). Nevertheless, even the slow upward creep of the price level along with the recent (from 1958 onwards) rather high levels of unemployment do call for attention and explanation. There are several well-known hypotheses on the problem that Weintraub never mentions: a growing "structural unemployment" that is not absorbed by increases in aggregate demand, the Gurley-Shaw explanation of rises in velocity (and the price level) stemming from the recent growth of money substitutes, and the charge made by Richard Ruggles and others that the price index is biased upwards.

In Chapter 6, which comprises nearly a third of the book, Weintraub advocates a program of wage controls that would, if necessary, require mandatory wage terms of all contracts involving 2000 or more employees. The tripartite governmental board administering the controls would be guided by average productivity increases, although consideration would be given to areas and industries with either labor surpluses or deficits. The latter proviso raises the problem of determining when a shortage or surplus of workers of some specific type exists (a much more difficult task than counting job vacancies and unemployed workers) and of how much of a wage change is necessary to achieve equilibrium. The controls would be over money wage terms of the contracts, and the problem of how to control supplementary or fringe benefits is inadequately discussed in a footnote (pp. 224-25). The

<sup>2</sup> Weintraub's reference to Irving Kravis' excellent article on this subject (this *Review*, Dec. 1959, 917-49) does not, in my opinion, excuse the author's failure to analyze the forces determining labor's share.

<sup>3</sup> See Robert Solow, "A Skeptical Note on the Constancy of Relative Shares," this *Review*, Sept. 1958, 618-31.

pros and cons of wage controls have been debated at length before, and I will not add my (largely negative) thoughts.

One final reservation to the *raison d'être* of the program concerns the validity of a *union-caused* wage-push inflation. Weintraub never mentions the many empirical studies of the effects of unions on both relative wages and on the absolute level of money wages. It is safe to say that the summary judgment of these studies would not support Weintraub's view of a crisis in our wage-setting procedures. A reader of this book should keep in mind that only about 25 per cent of the labor force is unionized, and that some of the industries experiencing wage increases that Weintraub points to as inflationary are largely nonunion. These points are not made by the author.

In conclusion, I do not believe Weintraub makes a convincing case on theoretical grounds for wage-push inflation nor for the efficacy of dealing with the current issues of full employment and price stability (the balance-of-payments problem is not mentioned) by means of a program of wage controls.

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*Labor, Management, and Social Policy: Essays in the John R. Commons Tradition.* Edited by GERALD G. SOMERS. Madison: University of Wisconsin Press, 1963. Pp. xiv, 303. \$6.00.

This is a collection of ten papers commemorating the centennial of the birth of John R. Commons. The contributors are on the faculties of economics and law at the University of Wisconsin. Each has chosen a different subject. Two of the essays deal with Commons' approach to public policy and his "labor theory." The others mention Commons, in some cases only incidentally, in treating such diverse subjects as unions, collective bargaining, social insurance, migratory labor, prevailing wage law, and manpower analysis and policy.

As perhaps is to be expected in such a volume, the emphasis is more on Commons' views and on past developments than on new ideas or new approaches. Robert Ozanne attempts to rebut critics of the Commons-Perlman interpretations of union origins and "models" of union development, without, however, much success because no method for systematic testing of their rather generally stated hypotheses is presented, and some of the scattered assertions offered in rebuttal are questionable.

The article by Gerald Somers contains a good discussion of the problems of overcoming unemployment in depressed areas and of the role of retraining in the program. Based on research, particularly in West Virginia, it arrives at an interesting set of conclusions in a subject area largely neglected by economic analysis.

Collective-bargaining theory has suffered from lack of contributions beyond insightful, though untestable, assertions and rarified abstraction. However, Reed Tripp's treatment of the subject is too descriptive and diffuse to provide more than background material for a systematic body of theory.

The other essays offer a considerable amount of factual material but little in the way of economic analysis. The article on social insurance by Arthur Altmeyer does present a thoughtful review of developments and issues in that field by the former Commissioner for Social Security.

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### **Population; Welfare Programs; Consumer Economics**

*Leisure Spending-Behavior.* By GEORGE FISK. Philadelphia: University of Pennsylvania Press, 1963. Pp. xix, 202. \$6.00.

Considering the economic importance of the subject, there has been little effort expended on the analysis of the determinants of the demand for leisure. Although the author of this study is aware of interest in leisure spending by economists interested in the dynamics of aggregate consumer spending, he writes primarily from the point of view of a professor of marketing. He projects aggregate demand for leisure goods and services to 1965 and 1970, defines market segments in terms of economic and demographic characteristics of families, and attempts to explain the segmentation by markets which he observes.

A useful feature is the assembly of data from three principal sources and the systematic comparisons of the concepts used in the 1950 Survey of Consumer Expenditures of the Bureau of Labor Statistics, the *Life Study of Consumer Expenditures*, of 1955-56, and the National Income Supplement. Much of the analysis is devoted to "total measured leisure," which is the sum of outlays for recreation, reading, alcoholic beverages, and foreign travel. While in 1960 this sum amounted to 9.0 per cent of disposable personal income, in 1946-47 it had been 11.0 per cent. It is hard to find evidence of a boom in leisure spending in these series. Even the increase in foreign travel from 0.4 per cent in 1950 to 0.6 per cent in 1960 is less impressive when one compares the level of 0.7 to 0.8 in 1929-30. Fisk guesses that total spending for "fun" may exceed 15 per cent of national income; there will remain uncertainty as to how this total is behaving over time until better statistical series are developed. It is clear that existing classifications of expenditures are not really satisfactory for studies of leisure or recreation expenditures.

By the time this review is in print interest in projections of leisure spending to 1965 based on data through 1958 will have little more than methodological interest. The method used essentially was to compute time-series income-elasticity coefficients from annual data for 1948-56 and for 1954-58 and use these coefficients to prepare two sets of estimates for 1965. Many statisticians are likely to have doubts about the usefulness of coefficients of income elasticity based on very short time series. No estimates of the standard errors of the regression coefficients are presented, but the two sets of income elasticities are very different. The discussion of the projections is complicated by the procedure of using per capita time-series income-elasticity coefficients as summary statistics for the combined change in tastes and income. Fisk then speculates about probable changes in the "income elastic-

ity" when what he clearly means are changes in tastes, to the confusion of at least one reader. Surely changes in taste can influence the constant term in a regression equation as well as the coefficient for income.

The cross-section analysis that makes up the bulk of the remainder of the book is likely to be more useful. Extensive cross-tabulations are presented with as many as three independent variables shown at a time. The findings are varied. It appears, for example, that Negroes spend less on total leisure than whites, allowing for differences in income and family size. Negroes seem to spend more on alcohol than whites, but this finding, like others concerning spending on alcohol, is rendered suspect because people on the average report only about 42 per cent of spending on alcoholic beverages. It may be merely that Negroes are more honest in this respect. People's spending on recreation appears to belong to that class of expenditures which is more closely related to their physiological age than to their stage in the family life cycle. This finding is consistent with other work which has shown that participation in outdoor recreation such as swimming, hiking, driving, and picnics is a function of age. (See the *Study Reports* of the Outdoor Recreation Resources Review Commission.) Per capita expenditures on "measured leisure" comprise only 4.3 per cent of income after tax for the income class over \$25,000 compared to 8.8 per cent below \$1,000, according to the 1950 Study of Consumer Expenditures. Can it be that "fun" is more attractive to low-income people? Or did the pleasures of the prosperous somehow escape unmeasured?

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*The Economics of Welfare Policies.* By MARGARET S. GORDON. New York and London: Columbia University Press, 1963. Pp. xi, 159. \$3.00.

This volume is one of a series of three on the economics of health, education, and welfare, commissioned by the Ford Foundation. Both the Foundation and the author view the book as an introduction to some of the economic issues in the welfare area.

Dr. Margaret S. Gordon's preface indicates the purpose she hopes the book will serve: ". . . to stimulate interest in the economic issues associated with welfare policies, and to call attention to some of the important unsettled questions, rather than to provide an exhaustive discussion of all the problems." Time alone will tell whether interest is stimulated. If not, it would, perhaps, tell us more about our profession than about this contribution.

Dr. Gordon defines welfare programs as "social programs designed to transfer income in cash, in kind, or in the form of services, to those whose capacity for self-support has been impaired, interrupted, or (in the case of children) has not yet reached maturity." Health and education are excluded, however, and the focus is largely on income-maintenance programs. One might disagree on some of the items excluded and included under this definition (e.g., some portion of the veterans' programs), but the definition is workable. In any case, its greatest relevance is to the early and sketchiest part of the book. It does not affect the two chapters that comprise the bulk of the volume.

One can, perhaps, get a feel for the book's coverage and emphasis by listing the chapters (and their length): Introduction (5 pages); Welfare Programs in the United States (8 pages); Some International Comparisons (10 pages); Welfare Programs and Income Redistribution (7 pages); The Old-age, Survivors, and Disability Program (46 pages); Unemployment Compensation (40 pages); Conclusions (2 pages). Additionally, there is an Appendix describing provisions of the OASDI and Unemployment Insurance System (16 pages) and a list of references (totaling 226).

The text is largely concerned with a review of issues and previous research. The presentation is lucid and straightforward. Knowledgeable readers will find the discussion well documented and the list of references most helpful. Those who have not given much thought to the welfare area will find the book provocative and may be pleasantly surprised at the number of research questions that are suggested and the degree to which other areas of economic analysis impinge on issues in the welfare area. The book can, therefore, serve the graduate student in search of a dissertation topic as well as the professor who needs convincing that a topic in this area will require the use of analytic skills, knowledge of the literature in the mainstream of economics, quantitative techniques, and so forth. Obviously, too, it is sufficiently short to warrant the hope that others, as well, will read it and find new avenues of research opened up.

Perhaps, then, the book is best thought of as a research agenda (though an agenda which this reviewer feels fails to emphasize the cost-benefit analysis sufficiently). But how much more delightful to read this than to read a research agenda as such! For while the "agenda"—broadly defined—may be the volume's chief contribution, the discussion of issues and unsettled questions is interesting and stimulating in its own right. Perhaps Dr. Gordon would feel that if research were not stimulated the book would not have been worth writing, but it is certainly worth reading (even by those whose research time is "committed" but who want to talk more intelligently about public issues). If the reader finds the welfare area stimulating and wants to read further, he would do well to turn to Eveline Burns' book, *Social Security and Public Policy*. Both authors eschew the purely descriptive approach, emphasize policy issues, and raise analytical questions. The books are organized differently and have different points of emphasis. The one, therefore, does not serve as a substitute for the other.

One trusts that Dr. Gordon's hopes will be fulfilled and more economists will be concerned with the welfare area. Issues abound and decisions will be made—with or without the contribution of the economists. We need more researchers in the field—persons who, like Dr. Gordon, see the relevance of work in other areas of economics to the welfare area.

One also hopes that Dr. Gordon will write the longer volume on the economics of welfare programs that she speaks of in her preface. There is a need for many such books, and Dr. Gordon is one of a handful of persons who could write one of them.

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### Related Disciplines

*The Politics of Scarcity—Public Pressure and Political Response in India.*

By MYRON WEINER. Chicago: University of Chicago Press, 1962. Pp. xix, 251. \$5.00.

It is interesting to see how economists and political scientists working in the field of economic development have begun to turn to the questions of economic policy formation, with respect to economic problems in the under-developed countries. Within the past year Albert Hirschman's *Journeys Toward Progress* and Ray Vernon's *The Dilemma of Mexico's Development* have dealt with the adoption of certain specific development policies in various Latin American countries from an economist's point of view. This book deals with a similar range of problems (as well as others) in India from the viewpoint of a political scientist. (It is in turn strongly influenced by the Almond-Coleman book, *The Politics of the Developing Areas*.)

Myron Weiner's approach is quite different from that of the economists who are specific-policy-oriented; what he does is examine the role of pressure groups in India, and their relations to the government. After his general introduction, he identifies the specific pressure groups—communal, labor, business, peasant, and student—and discusses their aims, their membership, their hopes from the government, and their methods of exerting influence upon the government. He then discusses the response of the government to these pressure groups, presenting both a theory of the relationship between government and pressure groups in India and suggestions for improving the relationship. Ostensibly this is not an economic study; but in a country such as India, where one of the main responsibilities of the government is that of economic development, many of the policies of the government deal with economic problems, and very many of the specific functions of the government are to implement economic policies. Thus the main general problem that the book deals with is the role of interest groups in the making of economic policy in India. As such, it is unique in the literature of Indian economic development specifically and still rare in the general field of development.

I recommend for the economist especially those pages dealing with the attitude of the Indian population toward the role and tasks of government—an attitude which is not, I suspect, very different from that in other under-developed countries; the complex relations of business groups to political parties in both the formation of general policies and of specific administrative policies, and conversely, the popular and government attitudes toward business; the meaning and value of so-called "corruption" in a controlled economy; the power structure in rural India and the relation of peasant groups to farm policies and legislation in such fields as land reform, cooperatives, agricultural taxation, and other rural policies; the decision-making process in India with respect to the location of industries in the Plan—a discussion based on various examples. The treatment of each of these points was both highly informative and stimulating of other ideas and relationships.

Weiner concludes with a general thesis of the sharp dichotomy between government and politics in India—with the former essentially performing a largely administrative role, with its policies free from politics and based on rational analysis of what is desirable or optimal; politics, however, is irrational and “bad,” upsetting “the best” plans of government, and reflecting selfish interests. (A somewhat longer treatment of the reasons for this attitude would have been desirable.) Since the meeting ground between the “interests” and the government is a very narrow one, this leads, on the one hand, to an assumption of numerous functions by the government (since it considers it can do these best) and, on the other hand, to the acceptance of violence as a normal means of exerting pressure by the interest groups upon the government (since they have little other contact). One method that Weiner suggests to deal with this sharp split, a method of special interest to the economist, is to reduce the role of government by reducing the amount of direct control of the economy, and permitting greater scope for the market as a decision-making mechanism.

My main point of issue with the book is in fact with its general hypothesis of such a very sharp dichotomy between the government and the “interests,” with its assumption that the policies of the former are based on principles of economic rationality. While this is to some degree a hope on the part of the government, in fact the Plans *are* political documents and are prepared as such; and the broad economic policies of the Plans with respect to such topics as sectoral allocation of resources, sources of taxation, price policies, location of industry, etc., are strongly influenced by political calculations both within the Planning Commission itself and the Commission's parent body—the National Development Council. The Plans are *not* a product of an “above the battle” economically rational analysis (although this enters into the Plans) within the government, but they reflect argument and compromise within the bureaucracy preparing the Plans (both within the Planning Commission and the economic ministries); between the state governments and the Central Government; and within the Congress Party—and throughout this process political calculations weigh heavily. An interesting task for an economist would be to try to show how the various group ideologies and interests do in fact influence the macroeconomic character of the Plans and the implementation of the Plans—and thus the economic development in fact achieved in India during the past decade and likely to be achieved in the future. A related problem that the book deals with only peripherally is to what extent rapid economic growth is compatible with a government whose role is essentially that of an “honest broker” among various interest groups.

My general view of this book is that it is a most valuable and well-written contribution to the study of the politics of growth in all underdeveloped countries, as well as India, and I highly recommend it as such.

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### Related Disciplines

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# NOTES

## POLICY STATEMENT ON ROLE OF THE A.E.A. IN ECONOMIC EDUCATION

Concern for improved economic education has grown during the past decade in every significant quarter. Leaders and rank and file among the general public, business, labor, the schools (at all levels), and the economics profession have become alerted and active. Increasingly, the interest and concern have been manifested in positive programs.

The American Economic Association has played a part in this movement. For a decade it has had a standing Committee on Economic Education. It has joined with others in sponsoring, or nominating members of, the National Task Force on Economic Education, the national television program "The American Economy," a Materials Evaluation Committee, a Committee on Measurement of Economic Understanding, and a Publications Advisory Committee to the Joint Council on Economic Education. Through its Committee on Economic Education, it established a special committee to prepare and publish a report on economics in secondary school textbooks on economics, social studies, and American history. The Association is formally affiliated with the Joint Council on Economic Education, nominates three members of the Council's board of trustees, and is prepared to cooperate with other responsible groups working to raise the scientific quality of economic education.

The economics profession has a responsibility second to that of no other group for the improvement of economic education among our present and future citizens; and the American Economic Association, as the body representing the profession, is now moving to step up its activities in this area and to assist its members to discharge that responsibility. The Association firmly disavows any purpose to determine or promulgate "authorized" economic doctrines or policies. It will, however, through its committees and interested members, undertake to make professional guidance and assistance more readily available to those who seek it.

Other professional associations in the social sciences, notably the American Historical Association through its Service Center for the Teachers of History, have shown the way. The American Economic Association has an experienced, responsible, and competent associate—the Joint Council on Economic Education—and other supporting organizations are at hand. Teachers, schools, and school systems at national, state, and local levels are asking for professional assistance, and members of the Association who are prepared to provide assistance need the leadership, coordination, and support that the Association alone can give.

In line with the foregoing, the Executive Committee of the Association has voted to reconstitute the Committee on Economic Education, with a mandate to move actively to develop a positive program to help improve economic understanding, and has approved an arrangement under which three members of the new Committee will serve as a special advisory committee to the Joint Council on Economic Education. This arrangement does not in any way make the Association responsible for actions of the Joint Council, but it will provide close, effective liaison, and will provide the Council systematically with outside counsel from professional economists.

The new Committee on Economic Education consists of G. L. Bach, Carnegie Institute of Technology; M. R. Colberg, Florida State University; Rendigs Fels, Vanderbilt University; R. A. Gordon, University of California; B. W. Lewis, Oberlin College; and E. T. Weiler, Purdue University. It has been requested to develop and to implement with all due speed a program of action and assistance, as indicated above, and to report periodically to the Association.

EXECUTIVE COMMITTEE  
March 6, 1964

## 1965 SLATE OF OFFICERS

A nominating committee consisting of Gottfried Haberler, chairman, Avery B. Cohan, Richard M. Cyert, G. A. Elliott, Marc Nerlove, and Nathaniel Wollman has submitted the following slate of nominees for 1965 officers of the American Economic Association.

*President-Elect:*

Fritz Machlup

*Vice-Presidents:*

Nicholas Georgescu-Roegen

Alexander Gerschenkron

Charles J. Hitch

W. Arthur Lewis

*Executive Committee:*

Moses Abramovitz

Neil H. Jacoby

Henry A. Latané

Robert H. Strotz

*Representative, Social Science Research Council:*

Karl A. Fox

The annual meeting of the Association will be held in Chicago, December 28-30, 1964, with headquarters at the Conrad Hilton Hotel.

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SURPLUS CENSUS REPORTS AVAILABLE FOR 1947, 1954, AND 1955

The Bureau of the Census is planning to dispose of surplus copies of reports based on 1947 and 1954 Censuses of Manufactures, the 1954 Census of Mineral Industries, and the 1955 Annual Survey of Manufactures. A limited number of the following publications are available:

*1947 Census of Manufactures*

- Vol. I General Summary
  - II Statistics by Industry
  - III Statistics by States
  - Indexes of Production
  - Product Supplement

*1954 Census of Manufactures*

- Vol. I General Summary
  - II Part 1 Industry Statistics, General Summary and Major Groups 20 to 28
  - Part 2 Industry Statistics, General Summary and Major Groups 29 to 39
  - III Area Statistics

*1954 Census of Mineral Industries*

- Vol. I Summary and Industry Statistics
  - II Area Statistics

*Annual Survey of Manufactures*

Volume for 1955 only

Interested persons should write promptly to the Publications Distribution Section, Bureau of the Census, Washington, D.C., 20233, indicating the volumes they would like to receive. Requests will be filled as long as the supply lasts. Although no charge will

be made for these publications, it will be necessary for the recipient to pay shipping charges in some cases.

### NSF GRANTS FOR GRADUATE SCIENCE FACILITIES

For the past three years the National Science Foundation has provided matching grant money for the renovation or construction of graduate facilities. In October, 1963, it issued a new booklet NSF 63-48, *Grants for Graduate Science Facilities*, which describes its facilities programs. The programs have been somewhat broadened. These grants provide financial support for those portions of facilities (excluding classrooms) that are to be used for basic scientific research or for graduate training in the sciences. Such facilities may be used by faculty, staff, and by graduate or postdoctoral students. The institutions must provide, from nonfederal sources, matching funds equal to the amount provided by the Foundation. Funds, up to 15 per cent of the total requested for construction and fixed furnishings, may also be requested for movable and general-purpose laboratory apparatus.

While the behavioral sciences have participated in the Foundation's facilities programs since their inception, a separate Behavioral Sciences Facilities Program was created in the summer of 1963. During the past three years approximately 50 grants have been made for behavioral science facilities. These have ranged from \$2,350 to \$1,400,000. Often two or more departments or research units submit a common proposal.

Inquiries concerning the facilities programs should be addressed to the Division of Institutional Programs, National Science Foundation, Washington, D.C., 20550. Since there are no "closing dates," proposals may be submitted at any time.

### Announcements

The National Institute of Social and Behavioral Science, in collaboration with the Section on Social and Economic Sciences of the American Association for the Advancement of Science, will hold sessions for contributed papers at the annual meeting of the A.A.A.S. in Montreal, Canada, December 26-31, 1964.

Association members interested in presenting a paper at these sessions should forward titles and 300-word abstracts not later than September 1 to Donald P. Ray, Director, National Institute of Social and Behavioral Science, 863 Benjamin Franklin Station, Washington, D.C., 20044. Papers should be based on research current or recently completed by the author. Subjects may concern any aspect of national or international economic problems. Topics of a multidisciplinary nature are also of interest.

Selected papers of the sessions will be published by the National Institute. The American Economic Association is an affiliate member of the Section on Social and Economic Sciences of the A.A.A.S.

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The University of Michigan announces the establishment of the W. S. Woytinsky Lectureship Award. One thousand dollars will be awarded biennially "for the best book, article or speech in the broad field of economics to educate public opinion or influence economic or social policy." Questions concerning this award may be addressed to Woytinsky Award Committee, Department of Economics, University of Michigan, Ann Arbor, Michigan.

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The Sidney Siegel Memorial Fund Committee wishes to announce free copies of the following books:

Siegel and Fouraker: *Bargaining and Group Decision Making*

Fouraker and Siegel: *Bargaining Behavior*

Siegel, with Siegel and Andrews: *Choice, Strategy and Utility*

Messick and Brayfield, editors: *Decision and Choice: Contributions of Sidney Siegel*

are available to students, in the United States or abroad, having a scholarly interest or being engaged in research in the area of individual choice behavior and bargaining be-

havior. To obtain copies of these monographs, the student should ask his major professor to write to Preston Cutler, Center for Advanced Study in the Behavioral Sciences, at 202 Junipero Serra Boulevard, Stanford, California, stating the name and address of the student and specifying which book is desired. All such requests will be honored until the Sidney Siegel Memorial Fund has been depleted. Contributions to the fund have been made by Fellows of the Center for Advanced Study in the Behavioral Sciences of the classes of 1957-58 and 1961-62.

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The New England Council announces its 1964 Publications Prize Program for the selection and publication of outstanding research of economic development relevant to New England. Dissertations and manuscripts dealing with the process of regional economic change in general and New England in particular are eligible. For more detailed information write to Mr. Rudolph W. Hardy, Director of Economic Research, The New England Council, 1032 Statler Office Building, Boston, Massachusetts, 02116.

#### *Deaths*

Paul Baran, professor of economics, Stanford University, March 26, 1964.

Richard C. Kao, research staff, The RAND Corporation, January 1964.

Ben F. Lemert, professor emeritus, Duke University, October 28, 1963.

Frederick C. Mills, professor emeritus of economics, Columbia University, February 9, 1964.

Leo Silverman, instructor, department of economics, New York University, February 1964.

#### *Retirements*

H. A. Logan, professor emeritus, University of Toronto.

#### *Visiting Foreign Scholars*

Keith Hancock, University of Adelaide, Australia: visiting professor of economics, Princeton University, academic year 1964-65.

Hans J. A. Kreyberg, Technical University, Trondheim, Norway: visiting professor, department of economics, University of Minnesota, academic year 1963-64.

#### *Promotions*

Arnold P. Ballantyne: associate professor of economics, U.S. Air Force Academy.

Vladimir N. Bandera: associate professor, economics department, Boston College.

Robert E. Barckley: professor of economics, San Diego State College.

Nicholas A. Beadles: associate professor of economics, University of Georgia.

Paul T. Bechtol: associate professor of economics, Colorado College.

Herbert Chesler: assistant professor of economics, University of Pittsburgh.

J. H. Dales: professor of economics, department of political economy, University of Toronto.

L. Aubrey Drewry, Jr.: associate professor of economics, University of Georgia.

H. C. Eastman: professor of economics, department of political economy, University of Toronto.

D. F. Forster: assistant professor of economics, department of political economy, University of Toronto.

J. H. Green: professor of economics, department of political economy, University of Toronto.

John P. Henderson: professor, department of economics, Michigan State University.

Marvin Jackson: assistant professor of economics, Arizona State University.

Subbiah Kannappan: associate professor, department of economics, Michigan State University.

Anne O. Krueger: associate professor of economics, University of Minnesota.

J. I. McDonald: assistant professor of economics, department of political economy, University of Toronto.

Francis M. McLaughlin: assistant professor, economics department, Boston College.

H. David Maloney: professor of economics, DePauw University.

John E. Metcalf: associate economist, First National City Bank, New York, December 17, 1963.

Howard W. Nicholas: professor, department of economics, Clark University.

Dennis O'Connor: assistant professor of economics, Arizona State University.

Lloyd D. Orr: assistant professor of economics, Indiana University, February 1964.

Abraham J. Siegel: professor of industrial relations, Massachusetts Institute of Technology.

George Slasor: assistant professor of economics, department of political economy, University of Toronto.

Marjorie S. Turner: professor of economics, San Diego State College.

Ray O. Werner: professor of economics, Colorado College.

J. W. L. Winder: associate professor of economics, department of political economy, University of Toronto.

### *Administrative Appointments*

Gordon Bivens: acting chairman, economics department, University of Wisconsin Extension Division.

Irving Brecher: director, Centre for Developing-Area Studies, McGill University.

Cono Casella: professor of economics and director, Arthur T. Roth Graduate School of Business Administration, C. W. Post College of Long Island University.

Kenneth J. Curran: dean, Colorado College.

Robert L. Dickens: assistant dean of arts and sciences, Duke University, July 1963.

Denis A. Flagg: chairman, department of economics, San Diego State College.

William C. Frederick: professor of business administration and dean, Graduate School of Business, University of Pittsburgh, March 1, 1964.

Clara H. Friedman: director of research, National Board of the Coat and Suit Industry.

Frank B. Gopen: president, Investors Guidance Corporation.

Paul M. Jones: chairman, department of economics and business administration, Colorado College.

R. E. Ludt: senior vice-president, Celanese Corporation of America.

Charles L. Quittmeyer: administrative director, department of economics, College of William and Mary.

Roy J. Sampson: associate professor of transportation and acting head, department of marketing, insurance and transportation, School of Business Administration, University of Oregon, spring term 1964 and academic year 1964-65.

Donald A. Schwartz, University of Wisconsin-Milwaukee: administrative assistant to the Mayor of Milwaukee.

Elroy J. Steele: head, economics department, University of Omaha, September 1, 1964.

Frank I. Stern: assistant director and acting director of market research, New York Central System.

John Turnbull: chairman, department of economics, University of Minnesota.

Roger C. Van Tassel: executive director, New England Economic Research Foundation.

Norman J. Wood: chairman, department of economics, University of Georgia.

James S. Worley: director, Graduate Program in Economic Development, Vanderbilt University, 1964-65.

John Parke Young, U.S. State Department: Central American Common Market, Guatemala.

### *Appointments*

James W. Adams: instructor, department of economics, University of Georgia.

Robert Baldwin: professor of economics, University of Wisconsin.

Robert Berney: assistant professor of economics, Arizona State University.

Raford Boddy: assistant professor, department of economics, State University of New York at Buffalo.

Nicholas G. Bohatiuk: associate professor of economics, Le Moyne College, Syracuse.

John H. Boyd: instructor in economics, University of Minnesota.

Raul M. Branco: assistant professor of economics, University of Texas.

Vernon M. Briggs: assistant professor, department of economics, University of Texas.

John A. Brittain, Vanderbilt University: senior research staff, Brookings Institution, June 1964.

Byron W. Brown: instructor, department of economics, Michigan State University.

John Burgess: assistant professor, department of economics, Goucher College.

Omer L. Carey: assistant professor of business administration, College of Economics and Business, Washington State University.

Robert M. Carlisle: technical staff, Systems Analysis and Evaluation, Aerospace Corporation.

Reynold C. Carlson, Vanderbilt University: Ford Foundation, Rio de Janeiro, Brazil.

N. K. Choudhry, State University of New York at Buffalo: assistant professor of economics, department of political economy, University of Toronto.

Charles H. Cline: instructor in economics, University of Minnesota.

Lee Cobb: associate professor, department of business statistics, University of Alabama.

Benjamin J. Cohen: assistant professor of economics, Princeton University.

Jacob Cohen, Bowling Green State University: professor of economics, University of Pittsburgh, fall 1964.

John Conlisk, Stanford University: assistant professor of economics, Rice University, September 1964.

Morris A. Copeland: Robert Julius Thorne professor of economics, Cornell University, spring term 1965.

John G. Cragg, Princeton University: assistant professor, department of economics, University of Chicago, academic year 1964-65.

L. S. Curtis: lecturer, economics department, Washington University.

Norman Daniel: lecturer in finance, University of Alabama.

Albert L. Danielsen, Duke University: assistant professor of economics, University of Georgia.

Richard B. DuBoff: assistant professor, Bryn Mawr College, September 1964.

Noel J. J. Farley: assistant professor, department of economics, Goucher College.

David Felix: professor, economics department, Washington University, September 1964.

Peter Frevert: assistant professor, department of economics, State University of New York at Buffalo.

Justin Fuller: instructor, department of economics, University of Georgia.

Sigfried Garbuny: associate professor, department of economics, Georgetown University.

Robert R. Gigliotti, Bureau of Labor Statistics: Naval Warfare Analysis Group, Center for Naval Analyses.

William Gillies: assistant professor, Bureau of Business Research, University of Nebraska.

Michael Godfrey: assistant professor of economics, Princeton University.

A. J. Goldenthal: transportation economist, Ford Motor Company, Dearborn, Michigan.

Kenneth Goldin: assistant professor, department of economics, State University of New York at Buffalo.

Herschel Ivan Grossman: assistant professor of economics, Brown University, September 1964.

Marshall M. Hall, University of Wisconsin: assistant professor, economics department, Washington University, September 1964.

E. Wannamaker Hardin, Jr.: instructor, department of economics, Duke University, second semester, 1963-64.

Mitchell Harwitz: associate professor, department of economics, State University of New York at Buffalo.

Edward Heiden: assistant professor of economics, University of Wisconsin.

Samuel Hollander, Princeton University: assistant professor of economics, department of political economy, University of Toronto.

Janet M. Hooks: assistant professor of economics, Eastern Illinois University.

Donald L. Huddle, Vanderbilt University: assistant professor of economics, Rice University, September 1964.

Saul H. Hymans: assistant professor, University of Michigan.

Clinton E. Jencks, University of California: assistant professor of economics, San Diego State College.

Marvin G. Julius: extension associate, department of economics and sociology, Iowa State University.

David Kamerschen: assistant professor, economics department, Washington University, September 1964.

Everett Kassalow: professor of economics, University of Wisconsin.

William M. Keane, University of Colorado: assistant professor, department of economics and business administration, Duke University, September 1964.

Allen Kelley: assistant professor of economics, University of Wisconsin.

George K. Keyt: instructor in economics, University of Minnesota.

Boyd Franklin King: instructor, department of economics, Georgetown University.

Melvin L. Kliman: instructor in economics, University of Minnesota.

Jan Kmenta: assistant professor of economics, University of Wisconsin.

Marian Krzyzaniak, Wayne State University: associate professor of economics, Rice University, September 1964.

Gerald M. Lage: instructor in economics, University of Minnesota.

Jerome R. LaPittus: assistant professor of economics, Cornell University, September 1964.

Arlyn Larson: assistant professor of economics, Arizona State University.

John Lindauer: assistant professor of economics, Occidental College.

Gail B. MacDonald: instructor in economics, University of Minnesota.

Douglas MacEachin: instructor in economics, Miami University, academic year 1964-65.

John L. McEntaffer: instructor, department of economics, Bowdoin College.

Stephen A. Marglin: assistant professor of economics, Massachusetts Institute of Technology.

James Melvin: instructor in economics, University of Minnesota.

Thomas V. Munzenreider: instructor in economics, University of Minnesota.

Edward Murphy: instructor, department of economics, Georgetown University.

Mohammed I. Nadiri: assistant professor, economics department, Northwestern University.

Thomas H. Naylor, Tulane University: assistant professor, department of economics, Duke University, second semester, 1963-64.

Mian Abdul Hameed Naz: senior economic analyst, Planning Commission, City of Baltimore, February 1964.

Douglas Needham: instructor in economics, Princeton University.

Cecile Jaffe Newburg, U.S. Employment Service: labor economist, Division of Research, U.S. Dept. of Labor.

Hugh O. Nourse: associate professor, department of economics, University of Illinois.

William H. Oakland: assistant professor of political economy, Johns Hopkins University.

William A. Paton: visiting professor of accounting, School of Business Administration, University of Oregon.

Harold L. Pazer: assistant professor of business administration, College of Economics and Business, Washington State University.

William Pollak: instructor in economics, Princeton University.

Frederick C. Raines: assistant professor, economics department, Washington University, September 1964.

Daniel P. Remington: lecturer in marketing, School of Business Administration, University of Oregon.

John C. Ritchie: AID in Medellin, Colombia, February 1964 to February 1966.

Donald M. Roberts, University of Illinois: economist, Ernst and Ernst.

Hugh Rose, University of Rochester: associate professor of economics, department of political economy, University of Toronto.

Sherwin Rosen: assistant professor of economics, University of Rochester.

Richard Rosenberg: instructor in economics, University of Minnesota.

Reid Ross: lecturer, economics department, Washington University.

William R. Russell, University of Wisconsin: assistant professor, department of economics, Michigan State University.

Richard Sander: instructor in economics, University of Minnesota.

Donald J. Schilling: assistant professor of economics, University of Missouri, fall 1964.

Barney Schwalberg: assistant professor of economics, University of Wisconsin.

Carl C. Schwan, Jr.: instructor in business administration, Vanderbilt University, 1964-65.

Karl Shell: assistant professor of economics, Massachusetts Institute of Technology.

Lawrence G. Smith, University of Chicago: assistant professor of economics, San Diego State College.

Kevin C. Sontheimer: instructor in economics, University of Minnesota.

Gary Sorenson: instructor in economics, Claremont Men's College.

Harold W. Stevenson: professor of finance, School of Business Administration, University of Minnesota, summer 1964.

Richard E. Suttor: research associate, department of economics and sociology, Iowa State University.

Overton H. Taylor, Harvard University: professor of economics, Vanderbilt University, 1964-65.

Hans B. Thorelli, University of Chicago: professor of business administration, Indiana University.

Richard H. Timberlake, Jr.: professor of finance, College of Business Administration, University of Georgia, January 1964.

Joseph Tryon, Georgetown University: National Planning Association.

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Yaroslav Vanek: associate professor of economics, Cornell University, September 1964.

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Jerome Wells: assistant professor, department of economics and Center for Regional Economic Studies, University of Pittsburgh, fall 1964.

Charles Wilber: assistant professor, department of economics, The American University.

James Wilde: instructor in economics, Princeton University.

Bruce W. Wilkinson: assistant professor of economics, department of economics and political science, University of Saskatchewan.

Thomas David Williams: lecturer in economics, Northwestern University.

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Charles Winston: lecturer, economics department, Washington University.

Arthur W. Wright: instructor in economics, Oberlin College, fall 1964.

### *Leaves for Special Appointments*

S. N. Afriat, Rice University: Cowles Foundation for Research in Economics, Yale University, first semester 1964-65.

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Martin J. Beckmann, Brown University: Institutsdirektor, Institut für Ökonometrie und Unternehmensforschung der Universität Bonn, Germany, summer and first semester 1964-65.

Martin Bronfenbrenner, Carnegie Institute of Technology: visiting professor, Cornell University, summer 1964.

John A. Buttrick, University of Minnesota: Fulbright Grant, University of Tokyo, academic year 1963-64.

A. W. Currie, University of Toronto: advisor to the Ministry of Transport of Nigeria, Lagos, Nigeria, academic year 1963-64.

George Delehanty, Northwestern University: visiting professor, Massachusetts Institute of Technology, 1964-65.

Frank T. de Vyver, Duke University: Fulbright Lecturer, University of Western Australia, second semester 1963-64.

Alfred L. Edwards, Michigan State University: assistant deputy Secretary of Agriculture, December 1, 1963 to November 30, 1965.

Eugene Elander, Miami University: Dayton campus, Miami University-Ohio State University, academic year 1964-65.

Edward C. Ettin, Duke University: visiting professor, Board of Governors of the Federal Reserve System, Washington, D.C., academic year 1964-65.

Edward M. Foster, University of Minnesota: Chief of Party, University of Minnesota-Los Andes-AID project in Bogota, Colombia, 1963-64.

Peter Gregory, University of Minnesota: Cornell University-University of Chile program in industrial relations and labor economics, AID, 1963-64.

D. G. Hartle, University of Toronto: Director, Royal Commission on Canada's Taxation.

John C. Hause, University of Minnesota: Institute for Defense Analysis, 1963-64.

George H. Hildebrand, Cornell University: Massachusetts Institute of Technology, academic year 1964-65.

Dell Bush Johannesen, University of North Carolina: visiting associate professor of economics, University of Texas, second semester 1963-64.

David Kaun, University of Pittsburgh: Brookings Institution, 1964-65.

Anthony Y. Koo, Michigan State University: visiting professor of economics, University of Michigan, academic year 1964-65.

Francis Kutish, Iowa State University: marketing specialist with AID in El Salvador, November 1963 for two years.

Bert C. McCammon, Jr., Indiana University: visiting professor, University of Washington, second semester 1963-64.

Alexander Melamid, New York University: University of Lagos, Nigeria, spring semester 1964.

Raymond F. Mikesell, University of Oregon: visiting professor, Graduate Institute of International Studies, Geneva, and advisor, U.S. Delegation to the United Nations Trade and Development Conference in Geneva, spring term 1964.

Richard A. Miller, Wesleyan University: National Science Foundation fellow, Massachusetts Institute of Technology, 1964-65.

Sam A. Morgenstern, U.S. Employment Service: Government of Israel.

Leon Moses, Northwestern University: Ford Foundation Faculty Research Fellowship, 1964-65.

Nathan Reich, Hunter College: Fulbright lecturer, University of Frankfurt, Germany, February 1964.

Ray C. Roberts, Old Dominion College: visiting associate professor, department of economics and business administration, Duke University, academic year 1964-65.

A. E. Safarian, University of Saskatchewan: visiting professor of economics, department of political economy, University of Toronto, academic year 1963-64.

Geoffrey S. Shepherd, Iowa State University: Chief of Party, AID in Viet Nam, March 1964 for one year.

Larry A. Sjaastad, University of Minnesota: Chief of Mission, University of Chicago-Cuyo-AID project in Mendoza, Argentina, 1963-64.

Kenneth Strand, Oberlin College: OECD in Paris, 1964-65.

Hans B. Thorelli, Indiana University: professor of marketing, Institute pour l'Etude des Méthodes de Direction de l'Entreprise, Lausanne, academic year 1964-65.

William P. Yohe, Duke University: Fellow, Center for Advanced Study in the Behavioral Sciences, Stanford University, academic year 1964-65.

Edward Zabel, University of Rochester: Cowles Foundation, Yale University, September 1964 to September 1965.

### *Resignations*

Mark A. Alexander: University of Pittsburgh, January 1964.

A. James Boness: University of Pittsburgh, April 1964.

Edward C. Fei: University of Wisconsin.

L. Reed Tripp: University of Wisconsin.

Martha Van Hoesen Barnett: Goucher College.

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Manuscripts and editorial correspondence relating to the regular quarterly issues of this REVIEW should be addressed to John G. Gurley, Managing Editor of THE AMERICAN ECONOMIC REVIEW, Stanford University, Stanford, California 94305. *Style Instructions* for guidance in preparing manuscripts in acceptable form will be provided upon request to the editor.

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# The American Economic Review

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## AGGREGATE PRODUCTION FUNCTIONS AND MEDIUM-RANGE GROWTH PROJECTIONS

*By* RICHARD R. NELSON\*

The conceptual basis for believing in the existence of a simple and stable relationship between a measure of aggregate inputs and a measure of aggregate output is uncertain at best. Yet an aggregate production function is a very convenient tool for theoretically exploring some of the determinants of economic growth, and it has served as a framework for some interesting empirical studies. Moreover, in an attempt to assess the growth prospects for an economy, to identify the variables that are likely to determine the growth rate, and to examine the policies affecting growth, the explicit or implicit use of an aggregate production function is almost indispensable.

In recent years economists have developed a variety of aggregate production functions. Several of these models are quite similar in basic conception but focus on different variables or make different assumptions about the interrelationships of the variables. Some represent amendments of earlier models. The different models yield somewhat dissimilar explanations of past growth and contingency forecasts with respect to future growth. The purpose of this paper is to try to place several of these models within a general framework so that their assumptions and implications can be compared and evaluated. First, the variables and relationships stressed by several different but related aggregate production models and the explanations they provide of the 1929-60 growth record of the United States will be examined. In the course of doing this a general production function will be developed which treats the different models as special cases. Then the analysis will turn to certain complementarity relationships between the variables that earlier formulations have tended to slight. As an application of some of the ideas, the concluding section will briefly examine a few of the quite different contingency forecasts provided by the different

\* The author is with The RAND Corporation. Much of the work which underlies this study was undertaken while he was on the staff of the Council of Economic Advisers. The author is indebted to M. Brown, W. Capron, C. Cooper, E. Denison, B. Massell, M. J. Peck, J. Schlesinger, and R. Solow for many useful suggestions.

models and other aspects of the problem of forecasting economic growth.<sup>1</sup>

Before the different models are examined, it will be useful to set out the basic growth record they have to explain. Between 1929 and 1960 real deflated GNP grew at an average annual rate of approximately 2.9 per cent. However, the rate of growth of output varied considerably over the period. From 1929 to 1947 (roughly the period of the depression to the start of postwar normalcy), real GNP grew at an average annual rate of 2.5 per cent—the average of a significantly slower growth rate during the depression, a tremendous surge of output during the war, and a postwar slowdown. From 1947 to 1960 the growth rate averaged 3.5 per cent a year—somewhat faster through the mid-fifties and somewhat slower from the mid-fifties to 1960.

Between 1929 and 1960 there was also considerable variation in the amount of economic slack as measured by unemployed labor and underutilized capital. Since production functions of the sort examined in this paper are designed to deal with secular factors and are not well suited to deal with the effects upon productivity of changing degrees of slack, it is dangerous to examine year-to-year changes in GNP. Therefore, we will be concerned with the average growth rates over various periods where the end points are roughly comparable in terms of the unemployment rates. To adjust further for the differences in degree of slack at the various terminal dates used, the Okun cyclical adjustments will be used and all data normalized to a 4 per cent unemployment rate [16]. Potential GNP for a year is defined as what GNP would have been had the unemployment rate been at 4.0 per cent of the labor force. Potential labor input is defined as man-hours that would have been worked had the unemployment rate been 4.0 per cent. The Okun adjustment to derive potential labor input from actual labor input tries to take account of cyclical effects in average hours worked per week and labor force participation rates, as well as percentage employed. The adjustment to derive potential GNP from actual GNP involves the labor-input adjustment and a productivity adjustment related to the unemployment rate. If the Okun equations are used to adjust for unemployment being somewhat higher in 1960 than in 1929, the average rate of growth of real potential GNP over the period was approximately 3.1 per cent; 2.5 per cent from 1929 to 1947, and 4.0 per cent from 1947 to 1960 with growth slowing down in the latter part of this subperiod.

Although the different models tend to stress different variables and relations, all of them are based on the relationship between growth of output and growth of labor input and capital input. Table 1 shows the

<sup>1</sup> This paper will focus on short- and medium-run growth. The long-run, steady-state properties of the models will not be examined. This is not a paper on the "Golden Age." But for an interesting comparison of the long-range implications of several models, see E. Phelps, [17].

average rate of growth of potential GNP, gross capital stock, and potential labor input measured in man-hours for selected subperiods.

Perhaps the most interesting aspects shown by Table 1 are, first, only a small absolute variation in the rate of growth of labor input; second, considerable variation in the rate of growth of the capital stock with variations in the rate of growth of GNP positively associated with variations in the rate of growth of the capital stock; third, potential GNP growing at a faster pace than either capital input or labor input in each subperiod;<sup>2</sup> and, fourth, a more rapid growth of potential output after World War II than before. Whereas the division of the 1929-60 record into the subperiods of Table 1 is relatively arbitrary, almost any division shows these four phenomena. These are the facts that an aggregate analysis of economic growth must explain.

TABLE 1—THE HISTORICAL RECORD<sup>a</sup>  
(percentage annual growth rates)

Year	Potential GNP	Potential man-hours	Gross capital stock
1929-60	3.1	.7	2.0
1929-47	2.5	.5	1.0
1947-60	4.0	.8	3.6
1947-54	4.4	.7	4.0
1954-60	3.5	.8	3.1

<sup>a</sup> The growth of potential output estimates is derived by using the Okun adjustments on the GNP series. The data for growth of potential man-hours are derived from the Knowles [13] series for actual man-hours, adjusted to get potential man-hours by the Okun equations. The capital stock series is from E. Denison [5]. Since producer's plant and equipment has grown at almost the same rate (in each subperiod) as total capital, I have not had to decide which concept is more relevant.

### I. *A Comparative Analysis of Aggregate Production Functions* *The Cobb-Douglas Model and Its Extensions*

The analysis of this section will be concerned with the Cobb-Douglas model and its extensions. The analysis could have been built around the more general constant elasticity of substitution model [2], but within a wide range which probably brackets the elasticity of substitution the conclusions are little different from those drawn from the simple Cobb-Douglas model. This fact, quite surprising to me at least, is proved in The RAND Memorandum from which this article is derived [15, ap-

<sup>2</sup> This statement does not hold true for earlier periods. During the 1909-29 period, potential GNP did not grow as rapidly as did the capital stock. Since much of Douglas' data [7] was for this period, this is one explanation of why he was able to get relatively good fits of Cobb-Douglas models without assuming any growth of total factor productivity. Kendrick [12] has called attention to this phenomenon. It is clear that growth of total factor productivity has been much greater since 1919 or 1929 than before.

pendix]. The analysis of this section also will be limited by the assumption that the effect of technical change is to shift the production function without altering its basic shape—that technical change is neutral. The reason for this limitation is that it is extremely difficult to measure the “nonneutrality” of technical change.<sup>3</sup> However, the effects of nonneutral technical change are examined in the preceding reference.

*The Basic Cobb-Douglas Model.* The basic Cobb-Douglas production function with constant returns to scale can be written:<sup>4</sup>

$$(1) \quad O_t = A_t L_t^b K_t^{1-b}$$

where  $O_t$  is potential GNP in year  $t$ ,  $L_t$  and  $K_t$  are potential labor input and capital input, respectively, in year  $t$ ,  $A_t$  is an index of total factor productivity, and  $b$  and  $1-b$  are the elasticities of output with respect to labor and capital, respectively. In pure competitive equilibrium and assuming a number of other stringent conditions, the shares of capital and labor will be indicators of these elasticities.

Taking logarithms, differentiating, and assuming  $b$  to be a constant (the basic Cobb-Douglas, neutral technological change assumption), the rate of growth of output is:

$$(2) \quad \Delta O/O = \Delta A/A + b(\Delta L/L) + (1-b)(\Delta K/K)$$

where  $\Delta O/O$  is the relative rate of growth of GNP;  $\Delta K/K$  and  $\Delta L/L$  are the relative rates of growth of capital and labor inputs; and  $\Delta A/A$  is the relative rate of growth of total factor productivity, or to put the matter more accurately, the part of the growth of output that cannot be explained by the growth of capital and labor using the simple Cobb-Douglas growth equation.<sup>5</sup> In a very real sense it is a measure of our ignorance.

Let us assume that the shares of labor and capital do provide an approximation to the output elasticity and roughly equal .75 and .25 over the period.<sup>6</sup> Further, assume that growth of man-hours and of gross capital stock are suitable measures of the growth of the services of these inputs.<sup>7</sup> Then  $\Delta A/A$  can be calculated as a residual,  $\Delta O/O - b(\Delta L/L) - (1-b)(\Delta K/K)$ , and for  $b = .75$  is presented in Table 2, column 1.

<sup>3</sup> But for one attempt, see [4].

<sup>4</sup> See Solow [25] for an example of the use of this kind of model, incorporating technological change.

<sup>5</sup> The relating of changes in GNP in one year to changes in the capital stock during that same year ignores problems of lags. However, since the analysis of this paper is concerned with average behavior of  $\Delta O/O$  and  $\Delta K/K$  over periods of several years, this problem is less serious.

<sup>6</sup> The sensitivity of the growth rate to different assumptions about output elasticities will be examined later. The  $b = .75$  assumption perhaps should be considered an upper bound.

<sup>7</sup> Obviously there are some serious problems in using capital stock as a measure of capital input. Vernon Smith's suggestion [22] that stock is the relevant concept is comforting but not

Notice what a large share of the average annual growth rate the unexplained residual is made to explain (approximately two-thirds in all of the subperiods).<sup>8</sup> And notice that, still assuming  $b$  to be approximately .75, the postwar acceleration of growth must be, in large part, explained by an increase (by more than one-third) in the rate of growth of total factor productivity. Similarly, the retardation since 1954 must be explained by a slackening-off (but not to the prewar rate) of growth of total factor productivity.<sup>9</sup> These conclusions, however, are strongly dependent upon the assumption that  $b = .75$ .

Table 1 showed, first, that throughout the period potential GNP grew faster than either input, but capital grew faster than did labor;

TABLE 2—GROWTH OF TOTAL FACTOR PRODUCTIVITY\*  
(percentage annual growth rates)

Year	$\Delta A/A; b = .75$	$\Delta A/A; b = .5$	$\Delta O/O$
1929-60	2.1	1.7	3.1
1929-47	1.9	1.7	2.5
1947-60	2.5	1.8	4.0
1947-54	2.9	2.1	4.4
1954-60	2.1	1.6	3.5

\* From Table 1 using equation (2).

second, that labor grew at a relatively constant rate; and, third, that the rate of growth of the capital stock varied considerably over the period, with simultaneous relatively rapid growth of both GNP and capital. Given these data, the greater the weight placed by the production function on the growth of capital—that is, the greater is  $(1 - b)$ —the less weight must be placed on the average rate of growth of total factor productivity in explaining average growth of GNP. (If the assumption of constant returns to scale is relaxed and the coefficients can add up to greater than one, the importance of  $\Delta A/A$  can be further reduced.)

completely convincing for this kind of analysis. Zvi Griliches, in an unpublished manuscript, has estimated that capital services have grown faster than gross stock by about half a percentage point a year.

<sup>8</sup> If Griliches is correct that capital services grew significantly faster than gross capital stock, then the unexplained residual is reduced somewhat, but it still is very large.

<sup>9</sup> Although almost any division of subperiods will show a postwar acceleration, it should be noted here that the selection of subperiods has influenced the interpretation of this phenomenon. Much of the increase in  $\Delta A/A$  over its pre-1947 average was concentrated in two short periods of time: 1948-50 and 1957-59 [5]. In other postwar years  $\Delta A/A$  was not significantly greater than its 1929-47 average. I do not know how this should be interpreted; in part, the explanation may lie in inadequate statistics. In any case, whether or not the increases in total factor productivity accounted for during these two periods are assumed to be, in fact, occurrences more evenly spread out in time, it is clear that  $\Delta A/A$  has, on the average, been higher since 1947 than before 1947 and probably has slowed down somewhat in recent years.

The low weight assigned to capital elasticity in the preceding calculations ( $1-b=.25$ ) resulted from the assumption (extraneous to the model) that factor shares are good estimates of factor elasticities. If this assumption is not adopted but rather  $b$  and  $\Delta A/A$  are obtained by regression, the estimated  $(1-b)$  is much larger—in the neighborhood of .5 or greater. And the average  $\Delta A/A$  for the period is smaller, as is shown in Table 2. More important, changes in  $\Delta A/A$  play a much smaller role in explaining changes in the growth rate over the subperiods, since the changes in  $\Delta K/K$  now “explain” a larger share of the concurrent changes in  $\Delta O/O$ .

However, this result carries no great economic insight. The variation in  $\Delta A/A$  calculated as a residual over the period represents the “errors” in a regression equation fitted for the entire period, and, if a regression is formally fitted,  $b$  will be calculated so as to minimize the variation of  $\Delta A/A$ . To put it another way, since  $\Delta L/L$  is (empirically) relatively constant, and since  $\Delta A/A$  is (by assumption of the regression) also a constant, the regression calculations will choose a  $(1-b)$  best suited to “explain” changes in  $\Delta O/O$  from changes in  $\Delta K/K$ . But if we have extraneous (to the model) information about  $b$ , the regression model must be constrained to take this information into account. If we continue to think strictly within the framework of the simple Cobb-Douglas model, the choice between the low  $(1-b)$ , and consequent large variation in  $\Delta A/A$ , explanation of past growth, and the high  $(1-b)$ , and consequent small variation of  $\Delta A/A$ , explanation depends on one’s judgment on, first, the extent to which factor shares provide good estimates of factor elasticities (which in turn depends on which of the wide number of theories of income shares one holds), and, second, whether it is likely that the rate of technical change has in fact increased in the postwar period.<sup>10</sup>

In any case, it is extremely important that  $\Delta A/A$  be explained. A growth theory that explains half of growth and much of the variation in growth by an unexplained residual (which is, after all, what “growth of total factor productivity” really is) is not much of a theory [6]. There are several different models that can be considered as providing amendments to the Cobb-Douglas model, which try to probe into the nature of  $\Delta A/A$ . One focuses on improvements in the quality of the capital stock. Another focuses on improvements in the quality of the labor force. By providing different descriptions of the determinants of  $\Delta A/A$ , these models further enrich the set of possible interpretations of the 1929–60 growth record.

<sup>10</sup> If it is not assumed that  $b$  was a constant over the period, but instead possibly varied from subperiod to subperiod, then the range of alternative explanations increases greatly. For such an analysis, see Brown and Popkin [4]. While this possibility is excluded by the assumptions of this section, the more general model developed in [15, appendix] does admit this possibility.

*Capital Quality and the Embodiment Effect.* One of the factors that has contributed to the growth of total factor productivity has been advances in technology which have improved the productivity of new capital goods. In his 1959 article, Robert Solow [23] suggested that the basic Cobb-Douglas model did not take account of the strong complementarity between technical change and investment. In his article, and in subsequent articles based on it [26], a distinction is made between "organizational" improvements which do not require new capital and "design" improvements which do. In contrast with the implicit assumption of the basic Cobb-Douglas model, Solow assumed that design improvements need to be embodied in new capital equipment. The effect of this need for "embodiment" is, as we shall see, to raise the sensitivity of the growth rate to changes in the rate of growth of capital.<sup>11</sup>

The Solow model can be written:

$$(3) \quad O_t = A'_t L_t^b J_t^{1-b}.$$

Although  $K$  in the basic Cobb-Douglas model is, in a sense, the number of machines (a proxy for the flow-of-machines services),  $J$  in a Solow model is a quality-weighted number of machines with new machines given greater weight than old machines, reflecting the newer technology embodied in them.  $A'_t$  is an index of economic efficiency, differentiated from  $A_t$  of equation (1) by the fact that  $A_t$  includes things that are incorporated in  $J_t$  as well as in  $A'_t$  of equation (3).<sup>12</sup>

The rate of growth is:

$$(4) \quad \Delta O/O = \Delta A'/A' + b(\Delta L/L) + (1-b)(\Delta J/J).$$

Assuming that advancing technology permits the quality of new machines to improve at  $\lambda_k$  per cent a year,  $J$  can be written:

$$(5) \quad J_t = \sum_0^t K_{v,t}(1 + \lambda_k)^v.$$

$K_{v,t}$  is the amount of capital built in year  $v$  (of vintage  $v$ ) which is still in use at time  $t$ .<sup>13</sup>

<sup>11</sup> It should be noted here again that the analysis of this paper relates for the short and medium run, not the long run, steady state, or "Golden Age."

<sup>12</sup> In a later formulation of his model, Solow separates plant from equipment in his  $J$  measure.

<sup>13</sup> It is important to note that  $K_{v,t}$  is gross capital of vintage  $v$  remaining at time  $t$ , and its measurement does not take into account any declines in value due to obsolescence.  $K_{v,t}$  is smaller than  $K_{v,v}$  if the physical productivity of machines declines through the years or if some machines are actually discarded. But the effect of obsolescence is not in the  $K_{v,t}$  measure. Let  $C_{v,t}$  be the measure of the value of capital of vintage  $v$  at year  $t$  which includes the effect of obsolescence. Then, if the productivity of capital improves at a rate  $\lambda_k$  a year,

$$C_{v,t} = K_{v,t}(1 + \lambda_k)^{-(t-v)},$$

or

$$K_{v,t} = C_{v,t}(1 + \lambda_k)^{t-v}.$$

To compare the growth implications of equations (2) and (4), it is useful to use the following approximation to equation (5):<sup>14</sup>

$$(5a) \quad J_t = B(1 + \lambda_k)^t K_t [1 + \lambda_k(\bar{a}_o - \bar{a}_t)].$$

Here  $\bar{a}_t$  and  $\bar{a}_o$  are the average ages of capital at times  $t$  and  $o$ , respectively.<sup>15</sup> This simplification involves a single moment of the age distribution of capital and replaces an equation involving the full distribution of vintages. Empirical tests suggest that equation (5a) is a very good approximation indeed.<sup>16</sup>

For small values of  $\lambda_k$  and  $\bar{a}_t$  not very different from  $\bar{a}_o$ ,  $\Delta J/J$  then can be approximated by:<sup>17</sup>

$$(6) \quad \Delta J/J = \Delta K/K + \lambda_k - \lambda_k \Delta \bar{a}.$$

$\Delta \bar{a}$  is the change in the average age of capital. Equation (6) makes a good deal of economic sense and could have been arrived at directly. The first two terms of equation (6) give the rate of growth of the quality-adjusted capital stock when the age distribution of the capital stock is not changing through time. The third term provides an adjustment when the age distribution is changing. A given age distribution determines a given difference between average quality and the quality of new capital. If each old machine were one year older, the difference between average quality and new quality would be larger by  $\lambda_k$ . More generally change in the gap between average quality and the quality of new equipment is approximately equal to  $-\lambda_k \Delta \bar{a}$ .

Using equation (6) in equation (4):

And, using equation (5),

$$J_t = \sum_0^t C_{vt}(1 + \lambda_k)^{t-v}(1 + \lambda_k)^v = (1 + \lambda_k)^t C_t,$$

and

$$\Delta J/J \approx \lambda_k + \Delta C_t/C_t.$$

Thus the Solow embodiment model reduces to the simple Cobb-Douglas if capital net of obsolescence is used in the equation.

$$^{14} (1) \quad J_t = (1 + \lambda)^{-\bar{a}_o}(1 + \lambda)^t \sum K_{vt}(1 + \lambda)^{\bar{a}_v - t}$$

$$(2) \quad J_t \approx B(\bar{a}_o)(1 + \lambda)^t K_t \sum K_{vt}/K_t(1 + \lambda v + \lambda \bar{a}_o - \lambda t)$$

where  $B(\bar{a}_o) = (1 + \lambda)^{-\bar{a}_o}$ .

$$(3) \quad J_t = B(1 + \lambda)^t K_t \left[ 1 + \lambda \bar{a}_o - \sum \frac{K_{vt}}{K_t} \lambda(t - v) \right]$$

$$(4) \quad J_t \approx B(1 + \lambda)^t K_t [1 + \lambda(\bar{a}_o - \bar{a}_t)]$$

<sup>15</sup> Knowles [13] also works with an "average age" model.

<sup>16</sup> Various series of  $J$  for different values of  $\lambda$  prepared by Richard Attiyeh were compared with series of  $J$  using the approximation formula. In all cases the series were almost identical.

<sup>17</sup> Thus  $1 + \lambda_k(\bar{a}_o - \bar{a}_t) \approx 1$ . The assumption that  $\bar{a}_t$  and  $\bar{a}_o$  do not differ significantly is appropriate for examining short-run changes.

$$(4a) \quad \Delta O/O = [\Delta A'/A' + (1-b)\lambda_k - (1-b)\lambda_k \Delta \bar{a}] \\ + b\Delta L/L + (1-b)\Delta K/K.$$

Save for the term  $-(1-b)\lambda_k \Delta \bar{a}$ , the growth equation for the embodied technical change model is similar to that for the simple Cobb-Douglas model.<sup>18</sup> If the average age of capital does not change,  $\Delta A'/A' + (1-b)\lambda_k$  is the rate of growth of total factor productivity, that is, the  $\Delta A/A$  of the original Cobb-Douglas model.  $(1-b)\lambda_k$  is the part that needs to be embodied in new capital, and  $\Delta A'/A'$  is the part that does not. The term involving changes in the average age, however, makes a great difference. Changes in  $\bar{a}$  reflect changes in the difference between the average technology in use and the best available technology. Given  $\Delta A'/A'$  and  $\lambda_k$ , the rate of growth of total factor productivity will be higher if the average age of capital is falling than if the average age

TABLE 3—CHANGES IN THE AVERAGE AGE OF CAPITAL\*

Year	$\bar{a}$ (average age)	Subperiod	Average $\Delta \bar{a}$ (average change in average age)
1929	16.5	1929-60	+ .006
1947	20.0	1929-47	+ .20
1954	18.0	1947-60	- .23
1960	17.0	1947-54	- .28
		1954-60	- .17

\* The data are from Knowles [13].

is constant or increasing. We shall continue to call the bracketed set of terms "growth of total factor productivity."

Table 3 presents the average annual change in the average age of capital for the relevant subperiods of the 1929-60 period.

Zvi Griliches has suggested that one of the objectives of growth theory should be to reduce the unexplained residual; one of the ways that he has tried to do this is to consider explicitly improvements in the quality of capital.<sup>19</sup> In a way this simply passes back the problem to another stage—we still have to explain why the quality of capital increased. But it does permit us to understand better what is going on.

Let us assume for the moment (the assumption will be modified later) that all of total-factor-productivity growth is the result of design tech-

<sup>18</sup> Recalling an earlier footnote, notice that, if the capital stock net of obsolescence is used rather than gross capital stock,  $J/J = \Delta C/C + \lambda_k$ . There is no term involving  $\Delta \bar{a}$  in the  $\Delta J/J$  equation and, if  $C$  rather than  $K$  is used in the  $\Delta O/O$  equation, there again is no term involving  $\Delta \bar{a}$ , since the average age of capital is caught in the net capital stock measure. In other words the "embodiment" effect is taken care of by the "net" capital measure.

<sup>19</sup> Z. Griliches [9] and also several unpublished manuscripts.

nical change that needs to be embodied, and hence all of the residual of the simple Cobb-Douglas model really is the result of the failure of the model to take into account improved capital quality. Then, for any value of  $b$ , in each of the subperiods  $(1-b)\lambda_k - (1-b)\lambda_k\Delta\bar{a}$  must, by definition, be equal to  $\Delta A/A$  as calculated from the simple Cobb-Douglas model. Thus, assuming all technical change to be embodied,  $(1-b)\lambda_k$  of the Solow model can be calculated from the relationship  $(1-b)\lambda_k = (\Delta A/A)/(1-\Delta\bar{a})$  where  $\Delta A/A$  is derived from equation (2) and was presented in Table 2. Estimates of  $(1-b)\lambda_k$  are presented in Table 4.

TABLE 4—THE COMPONENTS OF EMBODIED TECHNICAL CHANGE\*  
(percentage annual growth rate)

Periods	$(1-b) = .25$			$(1-b) = .50$		
	$\Delta A/A$	$(1-b)\lambda_k$	$-(1-b)\lambda_k\Delta\bar{a}$	$\Delta A/A$	$(1-b)\lambda_k$	$-(1-b)\lambda_k\Delta\bar{a}$
1929-60	2.1	2.1	0	1.7	1.7	0
1929-47	1.9	2.3	-.4	1.7	2.1	-.4
1947-60	2.5	2.0	.5	1.8	1.5	.3
1947-54	2.9	2.3	.6	2.1	1.7	.4
1954-60	2.1	1.8	.3	1.6	1.4	.2

\* Data from Tables 2 and 3.

The implications of this extreme assumption are interesting. Notice that if all growth of total factor productivity were the result of technical change which needs to be embodied in new capital (improved quality of capital) and if  $(1-b)$  is assumed to be equal to .25 (capital's share of GNP), a considerable part of the variation in the growth rates during the subperiods is explained by different rates of growth of capital and labor and different trends in the average age of capital; thus less weight need be placed on variation in  $\lambda_k$ . These factors are sufficient to explain the postwar acceleration of growth, if not the slowdown in recent years. There is no need to assume that "technical change" was faster in the immediate postwar era than in the prewar era. In a way embodiment explains too much because, with the full-embodiment model, it is necessary to assume that since 1954 the rate of technological progress has fallen sharply below its 1929-54 rate. But in general, assuming  $(1-b) = .25$ , and assuming a constant rate of technological progress, differences in the rate of growth of total factor productivity during the subperiods are relatively well explained by a widening gap between best practice and average practice before 1947 (reflected in a growing average age of capital) and a narrowing gap in the postwar period (reflecting in a falling average age of capital).

In the Solow model, has a mysteriously changing average age of capital replaced a mysteriously changing rate of growth of technology as the primary factor explaining changes in the growth rate? Not at all. Changes in the trend of the average age of capital are explained very nicely by changes in the rate of growth of the capital stock.

Assuming exponential depreciation (exclusive of obsolescence) at a rate  $\delta$  a year, changes in the average age of capital can be approximated by the following expression:<sup>20</sup>

$$(7) \quad \Delta \bar{a} = 1 - (\Delta K/K + \delta)(\bar{a}_{t-1}).$$

Empirical checks suggest that equation (7) is a quite good approximation formula. Equation (7) makes good economic sense.  $\Delta K/K + \delta$  is the rate of gross capital formation. If gross capital formation is zero, at the end of a year all old capital will be one year older, and there will be no new capital, so  $\Delta \bar{a} = 1$ . At the end of a year  $\Delta K/K + \delta$  is the ratio of new capital to total capital. The effect of new investment on the average age of capital is greater, the greater new investment is relative to the total capital stock, and the greater the average age of that capital stock.

<sup>20</sup> Let  $K_{ij}$  be the amount of capital built at time  $i$  that is still remaining at time  $j$ . Then:

$$\bar{a}_t = \frac{K_{t-1,t} + 2K_{t-2,t} + 3K_{t-3,t} + \dots}{K_t}$$

$$\bar{a}_{t-1} = \frac{K_{t-2,t-1} + 2K_{t-3,t-1} + 3K_{t-4,t-1} + \dots}{K_{t-1}}.$$

But since

$$K_t - K_{t,t} = K_{t-1,t} + K_{t-2,t} + \dots$$

$$\bar{a}_t = \frac{K_{t-2,t} + 2K_{t-3,t} + 3K_{t-4,t} + \dots}{K_t} + \frac{K_t - K_{t,t}}{K_t}.$$

Assuming

$$K_{t-j,t} = (1 - \delta)K_{t-j,t-1}; \text{ and } K_t = (1 + \Delta K/K)K_{t-1}$$

$$\bar{a}_t = \frac{(1 - \delta)}{(1 + \Delta K/K)} \bar{a}_{t-1} + \frac{K_{t-1}(1 - \delta)}{K_{t-1}(1 + \Delta K/K)}$$

$$\bar{a}_t - \bar{a}_{t-1} = \frac{1 - \delta}{1 + \Delta K/K} - \frac{(\Delta K/K + \delta)\bar{a}_{t-1}}{1 + \Delta K/K}.$$

Thus for small  $\Delta K/K$  and  $\delta$ :

$$\Delta \bar{a} = 1 - (\Delta K/K + \delta)\bar{a}_{t-1}.$$

It might be noted that a more exact approximation is:

$$\Delta \bar{a} = 1 - (\Delta K/K + \delta)(\bar{a}_{t-1} + 1).$$

Using this equation for any  $\Delta K/K$  and  $\delta$ , it is possible to compute the long-run equilibrium average of capital:

$$\bar{a}_\infty = \frac{1}{\Delta K/K + \delta} - 1.$$

Thus, an increase in the rate of growth of capital would tend to increase the rate of reduction in the average age of capital (or reduce the rate of increase) or, in other words, in the medium run (if not in the very long run) [17] "embodiment" raises the sensitivity of the growth rate to the rate of growth of capital. What the simple Cobb-Douglas model misses is that the effect of capital growth on growth of potential *GNP* is determined not solely by  $(1-b)$ , but also by the rate at which "design improvements" are occurring and the gap between best practice and average practice. To put it another way, new investment not only leads to "more" capital—the magnitude of the effect determined by  $(1-b)$ —it leads to "more productive" capital—the magnitude of the effect determined by  $\lambda_k$  and  $\bar{a}_t$ .

Substituting equation (7) in equation (4a):

$$(4b) \quad \begin{aligned} \Delta O/O &= \Delta A'/A' + (1-b)\lambda_k \bar{a}_{t-1} \delta + b \Delta L/L \\ &+ (1-b)(1 + \lambda_k \bar{a}_{t-1}) \Delta K/K. \end{aligned}$$

Notice that the coefficient in front of  $\Delta K/K$  is no longer simply  $(1-b)$ . If  $(1-b)\lambda_k = .02$  and  $\bar{a}_t = 18$ , then  $(1-b)(1 + \lambda_k \bar{a}_{t-1}) = .59$ , more than double  $(1-b) = .25$ . The effect of embodiment, just as the effect of  $(1-b)$  larger than capital's share of national income, is to increase the sensitivity of the rate of growth of *GNP* to the rate of growth of the capital stock.<sup>21</sup>

To recapitulate, the simple Cobb-Douglas model (with constant weights) yields two possible interpretations of the 1929-60 growth record. If  $\Delta A/A$  is assumed to be constant, the growth record is explained by  $(1-b)$  significantly greater than .25. If, on the other hand,  $(1-b)$  is assumed to be approximately .25, changes in  $\Delta A/A$  account for a major share of the changes in  $\Delta K/K$ . Under this second interpretation, the correlation between  $\Delta A/A$  and  $\Delta K/K$  needs to be explained.

The Solow model provides one possible explanation.  $\Delta A/A$  is the result of technical change which needs to be embodied in new capital; growth of total factor productivity is really the result of improvement in the quality of new capital. Assuming  $(1-b) = .25$ , under this interpretation the variation in  $\Delta A/A$  needed to explain changes in  $\Delta O/O$  is quite well accounted for by variations in  $\Delta K/K$ , and there is little need to assume any dramatic change in the rate of growth of technological knowledge ( $\lambda_k$ ) over the period.

This is a very interesting result. However, unfortunately it is pushing the point a bit to assume that all growth of total factor productivity is

<sup>21</sup> Statistically, it would be quite easy to confuse the effect of embodiment with the effect of a large static output elasticity  $(1-b)$ . And, if the statistician did not assume that the production function were homogeneous of degree one, embodiment could easily be mistaken for a high capital elasticity plus economies of scale (the coefficients of labor and capital adding to greater than one).

the result of such "design changes." We know, for example, that education (which presumably does not need to be embodied in new capital) has played an important role. To the extent that  $\Delta A'/A'$  of equation (4) is not zero, then, assuming  $b$  to equal .75, changes in the average age of capital (caused by a varying rate of growth of the capital stock) are not sufficient of themselves to explain changes in the growth of total factor productivity among the subperiods. And the smaller  $\lambda_k$ , relative to  $\Delta A'/A'$ , the more the explanation of the acceleration of postwar growth must depend on either an increase in  $\lambda_k$  or  $\Delta A'/A'$ , or a  $(1-b)$  substantially in excess of the share of capital.

*Improved Labor Quality.* Solow has focused attention on improvements in the quality of the capital stock. Edward Denison, following Theodore Schultz's lead, has drawn attention to improvements in the quality of labor input [5] [21]. Without too much forcing, the Denison model can be interpreted as introducing an average labor quality variable into the new-style Solow model. Thus

$$(8) \quad O_t = A_t^* (L_t q_t)^b J_t^{1-b},$$

$$(8a) \quad O_t = A_t^* Q_t^b J_t^{1-b}.$$

Just as improvements in the quality of the capital stock are included in Solow's  $J$ , improvements in the quality of the labor force are included in Denison's  $Q = Lq$ . Just as  $A'$  was a narrower concept than  $A$ , so  $A^*$  does not include all that  $A'$  includes.<sup>22</sup> However, while it turns out that Solow's quality-of-capital measure can be nicely related to a single variable (the average age of capital can be related back to the rate of growth of the capital stock itself), as we shall see Denison's quality-of-labor measure is not so easily handled.

Defining  $\lambda_L = \Delta q/q$  so that  $\Delta Q/Q = \Delta L/L + \lambda_L$ , the basic Denison growth equation, modified to incorporate the results drawn from the Solow model, can be written as follows:

$$(9) \quad \begin{aligned} \Delta O/O = & \Delta A^*/A^* + b\lambda_L + (1-b)\lambda_k - (1-b)\lambda_k \Delta \bar{a} \\ & + b\Delta L/L + (1-b)\Delta K/K. \end{aligned}$$

In this formulation  $\Delta A^*/A^* + b\lambda_L + (1-b)\lambda_k - (1-b)\lambda_k \Delta \bar{a}$  is the rate of growth of total factor productivity,  $\Delta A/A$  of equation (2);  $\lambda_L$  is the rate of improvement in the average quality of the work force.  $\Delta A^*/A^*$  is improvements not directly "embodied" either in capital or labor (for example, improvements in the allocation of resources and better management practices).<sup>23</sup> Improvements that do not directly require em-

<sup>22</sup> For a somewhat different approach to the narrowing down of  $A$ , see Griliches [9].

<sup>23</sup> Since we are assuming that the static output elasticities add up to one, we are assuming away the possibilities of economies of scale as a source of growth of productivity. Of course, it is simple to relax this assumption.

bodiment in either capital or labor will be called "organizational."

It should be noted, however, that with a Cobb-Douglas type of function, improvements in the quality of *all* labor (or in average quality) can just as well be treated as disembodied change or as "organizational" change. While the breaking out of  $\lambda_L$  is an analytical convenience and permits us to understand the growth process better, as equation (9) is formulated, it is not correct to say that  $\lambda_L$  is productivity increase embodied in labor in the same sense that  $\lambda_k$  is productivity increase embodied in capital.

The reason that  $\lambda_k \Delta \bar{a}$  enters the expression although there is no comparable term involving  $\lambda_L$  is that  $\lambda_k$  is defined in terms of the quality of *new* capital, and  $\lambda_L$  is defined in terms of the average quality of all labor. Improvements in basic educational standards, which principally affect the new entrants to the work force, could be treated like  $\lambda_k$ , in which case there would be a term equivalent to changes in the average age of the work force in the growth equation. And, in fact, when Denison calculates the effect of education on growth, he proceeds in roughly this way.

Thus in the Denison formulation the term  $b\lambda_L$  has been added to the basic growth equation where  $\lambda_L$  is the rate of improvement in labor quality. Denison relates improvement in labor quality to three variables.  $\lambda_L^E$  is improvement in labor quality due to improvement in educational attainment. He assumes that 60 per cent of the income differential associated with greater education is attributable to education. To suggest the orders of magnitude involved, his calculations are roughly consistent with the rule of thumb; each additional year of education increases labor quality by approximately 6 per cent.<sup>24</sup> Denison calculates that, considering both the increase in the average number of years of schooling per person in the work force and the increase in the length of the school year, improved average educational attainment increased labor quality at roughly one per cent a year over the period.

Denison then considers the changing age-sex composition of the work force. He concludes that the rate of improvement in composition,  $\lambda_L^C$ , proceeded at an average annual rate of .1 per cent.

Finally, Denison argues that, as the average work week declines, labor productivity per man-hour increases but with diminishing returns. He assumes that between 1929 and 1947 more than half of the decline in the average work week was offset by consequent improvements in labor productivity, and between 1947 and 1960 the offset was approximately one-third. Over the entire 1929-60 period, the rate of improvement in

<sup>24</sup> Each additional year of education is associated with, roughly, a 10 per cent increase in average income. Denison assumes that roughly 60 per cent of this is the result of the increased education.

average labor quality due to declining average hours of work,  $\lambda_L^H$ , averaged .3 per cent a year according to Denison.

Table 5 shows Denison's estimates of  $\lambda_L^B$ ,  $\lambda_L^C$ , and  $\lambda_L^H$ . The table suggests that the rate of improvement in labor quality was roughly constant over the period. Assuming that  $b = .75$ ,  $b\lambda_L = 1.0$  per cent a year or improvements in labor quality account for roughly half of the average

TABLE 5—COMPONENTS OF IMPROVED LABOR QUALITY\*  
(percentage annual growth rates)

Period	$\lambda_L$	$\lambda_L^B$	$\lambda_L^C$	$\lambda_L^H$
1929-60	1.4	1.0	.1	.3
1929-47	1.4	.9	.1	.4
1947-60	1.3	1.0	.1	.2
1947-54	1.3	1.0	.1	.2
1954-60	1.3	1.0	.1	.2

\* Data from Denison [5].

TABLE 6—THE COMPONENTS OF  $\Delta A/A$ \*  
(percentage annual growth rates)

Period	$\Delta A/A$	$b\lambda_L$	$\Delta A/A - b\lambda_L = (1-b)\lambda_k(1-\Delta d)$	$(1-b)\lambda_k$
1929-47	1.9	1.0	0.9	1.1
1947-60	2.5	1.0	1.5	1.3
1947-54	2.9	1.0	1.9	1.5
1954-60	2.1	1.0	1.1	0.9

\* Data from Tables 2, 4, and 5.

annual growth of total factor productivity of 2.0 per cent a year experienced over the 1929-60 period.

While the details of Denison's calculations are extremely bothersome, no one would argue either that improved labor quality has not been important or that technological change which requires new capital in order to be effective is the full story of productivity growth.<sup>25</sup> It can be shown (see Table 6) that if Denison's estimate of the contribution of improved labor quality is roughly correct, and if the rest of the explanation of growth of total factor productivity is improved capital quality resulting from technical change that needs to be embodied, then, assuming  $(1-b) \approx .25$ , these two factors split the credit roughly fifty-fifty. Under this explanation, however, it is impossible to assume that both

<sup>25</sup> Note that if  $\Delta A/A \approx .02$  and  $1-b = .25$ , then, if all total-factor-productivity growth is technical change which needs to be embodied,  $\lambda_k \approx .084$  ( $b\lambda_k \approx .021$ ). It is interesting that Solow [26] never experiments with a  $\lambda_k$  this large. Naturally, therefore, his regressions yield a  $(1-b)$  much greater than .25.

$\lambda_L$  and  $\lambda_k$  have been constants. Taking Denison's estimate of a (constant)  $\lambda_L$ , the variation in  $(1-b)\lambda_k$  of Table 6 is significantly greater than the variation of  $(1-b)\lambda_k$  of Table 4, although less than the total variation in  $\Delta A/A$  during the several periods.

Even if Denison's estimates of the rate of improvement in labor quality are high (and his estimate of the contribution of the shortened average work week certainly is suspect), design technical progress which needs to be embodied in new capital (in the Solow sense) probably was less than half of total-factor-productivity growth. And the lower is  $\lambda_k$ , the less sensitive is the rate of growth of GNP to the rate of growth of the capital stock. Denison has suggested (both in his book [5] and in a recently published article [5a]) that even this low  $\lambda_k$  overstates the sensitivity of the growth rate of output to the growth rate of capital, since much of new technology requires only marginal modification or addition to equipment, not totally new plant and equipment.

The conclusion must be that the embodiment effect, as Solow describes it, cannot have been large enough so that changes in the rate of growth of capital fully explain the variations in the growth rate of potential GNP that the United States has experienced during the 1929-60 period.<sup>26</sup> If the shares of capital and labor are assumed to provide tolerably good measures of the static output elasticities, the evidence suggests that  $\Delta A^*/A^* + b\lambda_k + (1-b)\lambda_L$  increased sharply in the early post-war period and then declined somewhat after 1954. If we accept Denison's data, which suggest that  $\lambda_L$  has been relatively constant, the burden must fall on changing  $\lambda_k$  and  $\Delta A^*/A^*$ .

However, it is very interesting that  $\Delta K/K$  and growth of total factor productivity have been so highly correlated. Although the Solow type of embodiment cannot fully explain this correlation, perhaps other factors can.

## II. *The Cobb-Douglas Model in a More General Analysis of Economic Growth*

Solow's interaction effect describes one of the interactions among the variables of the Cobb-Douglas model, but there may be a number of other important relationships between the variables. They may be linked in the sense that changes in one determine the effect of changes

\* Somehow many economists have come to view the requirement that new technology be embodied in new capital as in some sense a happy phenomenon. The reason for this seems to lie in the greater sensitivity of the growth rate to the investment rate that embodiment implies. But surely, the less the requirements for new technology to be embodied in new capital, the less costly is faster growth. Of course it might be replied that if growth itself were an objective regardless of cost, and if it were easier to influence  $\Delta K/K$  than other variables that affect growth, then a strong embodiment provides a strong handle for policy. But surely this is a strange argument.

in another. Changes in one may stimulate changes in another or changes in certain underlying conditions not explicitly included in the model may have an effect on several of the variables of the model. This section will examine several obvious but important examples of these phenomena.

### *Sources of Interaction*

*Education, Technical Change, and Improved Allocation.* Perhaps the greatest theoretical difficulty with Denison's method of examining the contribution of various factors to economic growth is that he does not deal explicitly with the very strong complementarity among the factors. In particular, it is quite clear that the effects upon GNP of the three principal contributors to growth of total factor productivity—technological change, improved educational standards and levels, and improved allocative efficiency—should not be viewed as independent.

Educated people, principally scientists and engineers, are a critical input to the research and development process; thus the rate at which technological understanding is increased is strongly related to the number of educated people applied to that purpose. The relatively high salaries these people receive are a direct reflection of their contribution to advancing technology. Were scientists and engineers the only inputs to the technological change process, then, in the absence of market imperfections, their salaries actually would be a good measure of technological change. Surely it is a mistake to measure the contribution of technological change to economic growth after subtracting the higher incomes that R&D scientists and engineers receive. Yet this is, in effect, what Denison's method does.

Although (as Denison points out) this direct and obvious linkage between educational input and technological change may not be of major quantitative importance, one might seriously propose the hypothesis that the need for and the return to educated people generally, not just research and development personnel, are in large part functions of the desired and actual rate of technological change. Industries and firms that have large research and development staffs also tend to have a relatively high percentage of scientists, engineers, and other trained people in other functions—management, sales, production [10]. This is scarcely surprising. New technological developments need to be evaluated by people in management who can understand them and who can understand the nature of the market for them. Information about new products needs to be communicated from the firm that develops them to the potential market by salesmen who can describe the product and its uses and can answer questions. In the early stages of production before the techniques become routinized, highly trained people are re-

quired to deal with the problems that invariably arise. In the absence of technological change, economic decision-making could be more routine. Just as R&D is essentially problem-solving, requiring highly trained people, the development of a new product or technique creates problems that require expertise in the form of experience and training, as well as imagination, for their solution. The relatively high remuneration of people who can deal imaginatively with these problems, just as the high salaries of R&D scientists and engineers, is in part, perhaps in major part, the reflection of the importance of technological change in economic growth. Should the pace of technological change diminish, the returns to higher education probably would also.

Nor can the importance of shifting allocation of labor and capital be evaluated independently of consideration of the effects of technological change. The fact that technological change proceeds at often dramatically different rates in different industries is one of the principal sources of economic disequilibrium in allocation of labor and capital. When technological change is relatively rapid in an industry faced by an elastic demand curve, it tends to increase the optimum share of the nation's capital and labor resources that should be allocated to that industry. A subsequent shift of labor and capital into that industry would increase national income. When technological change proceeds relatively rapidly in an industry facing an inelastic demand curve, that industry's optimum share of the economy's resources tends to decline. A subsequent shift of labor and capital out of that industry to others would be reflected in rising value of production. Clearly it would be a mistake to estimate the importance of technological change to economic growth by treating these subsequent reallocations as if they would have raised the value of output as much as they did, in the absence of technological change. For it is the pace and inequality among sectors of technological change which, in large part, determine the gains society can obtain by shifting resources from one industry to another.

Finally, to close the circle, one of the important lessons we have learned from experience with depressed areas and industries and with training and retraining programs is that basic literacy is almost a prerequisite for both learning of a new job and learning to do a new job. If a high level of education is essential to create technological change, a basic education is essential to permit people to adjust to it, and for the economy to gain maximum benefit from it. At lower levels as well as at higher levels, the returns to education are strongly affected by the pace of technological change.

*New Plant and Equipment as a Source of Economic Flexibility.* The preceding discussion has attempted to show that the sources of growth of total factor productivity cannot be viewed as independent. Thus it

might be useful to look more broadly at the relationship between physical investment and growth of total factor productivity.

Solow has focused attention on the fact that often new capital is needed to embody new technology. Yet surely the advantage that new equipment has over old is not limited to a more up-to-date technology. As relative factor and product prices change, as demand and technology change, as the size of the market increases, new plant and equipment can be tailored to the changing economic situation. New plant and equipment, as well as education, plays a major role in providing the economy with flexibility.

As Johansen has suggested in his growth model, plant and equipment once built may be quite inflexible [11]. It is designed to work with a certain quantity of labor and to produce a certain quantity of output, as well as being designed around a certain technology.<sup>27</sup>

Denison and others have suggested that movement of labor from the farms to higher-productivity jobs has played a relatively important role in U.S. economic growth during the 1929-60 period. Massell has estimated that shifts of the relative allocation of capital and labor between industries account for approximately one quarter of total-factor-productivity growth in the postwar era [14]. As was noted earlier, these shifts in allocation, and the increase in the value of GNP resulting from those shifts, should not be considered independent of technological change. And it is clear that these shifts were at least partially dependent upon the creation of new capital. To the extent that old capital is inflexible, additional labor cannot be added to it without sharply diminishing returns. The productive movement of labor from one sector or industry to another is limited by the rate at which new plant is being built in the industry to which labor is moving. Thus both directly and indirectly, there is complementarity between the rate of growth of total factor productivity resulting from better allocating resources (labor and capital), and the rate of growth of the capital stock.<sup>28</sup> This is not embodiment in the Solow sense, but it has the same effect.

<sup>27</sup> If it were not for this inflexibility, we would not observe the phenomenon of obsolescence. If old equipment were flexible, if it could operate with widely varying quantities of labor and other variable inputs, then old paid-for capital equipment in good condition could be made competitive with new equipment simply by operating at very high capital-labor ratios (higher capital-labor ratios than for new equipment). But we observe that old equipment tends to operate at lower capital-labor ratios than new equipment; and as new and increasingly productive equipment comes into the market, the inability of older equipment to operate at higher capital-labor ratios, and thus to reduce variable unit costs, sooner or later makes that equipment obsolete even if it is in fine physical condition.

It also should be noted here that inflexibility of a Johansen sort may tend to make factor shares and output elasticities diverge [18].

<sup>28</sup> It is interesting that Massell's results suggest that shifting allocation of capital has been more important than shifting allocation of labor. If the analysis above is correct, it should be difficult to distinguish between the effects of the two; however, Massell's results tend to reinforce the belief that capital growth contributes to more efficient allocation.

The Johansen type of inflexibility also suggests that the extent to which the economy takes advantage of economies of scale is dependent upon new capital, not just more capital. To the extent that plant and equipment are indivisible, they must be built with a certain market size in mind. To take full advantage of increases in the size of the market, new plant and equipment must be better suited than existing plant for that larger-size market. Similarly, new plant and equipment must be built to take advantage of new opportunities for specialization.

The Johansen model therefore suggests that the complementarity between growth of total factor productivity and growth of the capital stock is more general than that suggested by the Solow model. In the Solow model new capital is needed to embody new technology. In the Johansen model new capital is needed to take maximum advantage of changing economic opportunities generally. An increase in the rate of growth of the capital stock should lead to an increase in the rate of growth of total factor productivity, not only by reducing the gap between average and best technology, but by creating a capital stock better suited to present demands, relative factor costs, and opportunities for economies of scale. The distance between the existing state of the economy and the production-possibility frontier which would exist in a world of perfectly flexible capital is strongly related to the rate of new investment.<sup>29</sup>

This point carries added significance when the uncertainties surrounding new technology are recognized. Embodiment, even in the strict Solow sense, is not a one-shot proposition. The early versions of a new product or process are likely to be quite primitive and plagued by unforeseen difficulties. Improvement and perfection is a sequential learning process, and the rate of learning is dependent not only upon the length of experience with a particular version of the technology but on the rate at which suggested improvements actually are tried out. To the extent that these improvements require embodiment, the rate of learning is strongly affected by the rate of new investment. This complementarity between learning and investment has been stressed by Arrow [1]. And to the extent that experience and experimentation in actual use are important aspects of the process of technological change, the Arrow effect implies that rapid rate of growth of capital not only keeps the technology in use close to the frontier but also helps to accelerate the rate at which the frontier advances.

It should be noted, however, that Denison's point with respect to the limited amount of new capital needed to embody new technology is also

<sup>29</sup> Perhaps it is this type of notion that Frankel [8] had in mind when he developed his aggregate production function in which an increase in the stock of capital increases the level of economic efficiency generally.

relevant to the more general complementarity relationships discussed above. Certainly not very much new capital is needed to take care of a moderate shift in labor allocation. Not very much new capital is needed to permit experimentation and experience with new techniques. It is therefore likely that, in the short run at least, the complementarity effects of new investment are subject to rather sharply diminishing returns. Yet there could still be a substantial amount of difference in effect between a very low rate of gross investment (such as we experienced during the 1930's) and a somewhat higher rate, such as the postwar average, if not between the very high rates of the early 1950's and the somewhat lower rates of the late 1950's.

*Positive Feedback of Incentives.* The complementarity between changes in the rate of capital growth and of total-factor-productivity growth also is the result of positive feedback of incentives. When the pace of design technical change increases, the profitability of new equipment relative to old increases, and an increase in investment is stimulated. Indeed, much of modern investment theory is hinged on the notion that investment booms are, in large part, the result of the development of new products and processes. Thus an increase in  $\lambda_k$  should stimulate an increase in  $\Delta K/K$ .

Schmookler's analysis suggests that the incentive mechanism also works the other way [20]. When the rate of investment is high, the potential market for inventions which require embodiment is high, thus an acceleration of the growth of the capital stock should stimulate an increase in  $\lambda_k$ . When Schmookler's hypothesis is linked with the Arrow hypothesis, incentive for technological advance is linked with opportunities for experimentation. This combined effect quite probably is very powerful.

*The Effect of Prolonged Economic Slack.* The causes for the correlation between growth of capital and growth of total factor productivity go deeper. Both almost certainly are depressed by prolonged economic slack of the sort experienced during the 1930's. It also is clear that both are stimulated when the economy comes out of a depression or recession. When jobs are plentiful, there is less pressure for protection from foreign competition, less incentive for featherbedding, less resistance to the adoption of new technology. It also is clear that the rate of growth of the capital stock tends to be higher when demand is pressing on capacity than when capacity is slack. While it is not inconceivable that the magnitude and design of the capital stock might be such that producers would be operating at preferred operating rates while unemployment is high, empirically it seems to be true that over the 1929-60 period, when unemployment has been high, capital has been operated at less than desired rates.

The effect of full employment on managerial incentives for innovation and on labor mobility is less clear.<sup>30</sup> However, although there is not enough evidence at present to predict whether the rate of capital formation and of total-factor-productivity growth would be greater in an economy that sustained a high level of employment or in one that fluctuated with moderate amplitude around a high level, given our present institutional and political structure, it seems probable that either of these possibilities would generate a faster average growth rate than would an economy with chronic high unemployment.<sup>31</sup>

It is reasonable to believe, then, that save for the World War II period and the Korean War period<sup>32</sup> stimulation for both capital growth and growth of total factor productivity should have tended to move (inversely) with the unemployment rate over the 1929-60 period. A glance at Tables 1 and 2 suggests that this, in fact, has been the case. It should be noted that these cyclical effects on growth of the capital stock and of productivity are additive to the effects of slack on growth of man-hours and productivity that Okun has analyzed.

### *Some Implications of Interaction*

The analysis of interaction suggests that it is misleading to assume that the various factors resulting in the growth of total factor productivity are independent, and to estimate the contribution of technological change by examining the residual after having estimated the contribution of education, shifting allocation of resources, and so on. It also is misleading to assume that the contributions of the various factors leading to growth of total factor productivity are independent of the rate of growth of the capital stock.

The growth rate of potential output has varied considerably over the period, principally the result of related and interdependent changes in the rate of growth of the capital stock, the rate of technical progress, and improvements in economic efficiency generally. All these factors tend to move together. They tend to be encouraged by strong aggregate demand, discouraged by economic slack. A change in one tends to increase the importance of the others.

This suggests that a simple regression model that tries to estimate

<sup>30</sup> The data are reasonably clear that movement off the farms and to urban areas is greater in periods of high over-all employment. But the effect of full employment on labor mobility in the nonagricultural sectors is less clear.

<sup>31</sup> It may be significant that Kendrick, in comparing productivity growth in different industries, finds that industries which experienced only mild cyclical fluctuations tended to have a faster rate of growth of productivity than industries which experienced severe cyclical fluctuations.

<sup>32</sup> During these periods investment was limited by policy, and perhaps demand was so strong that incentives for innovation and for productive shifting of labor were dulled.

$\Delta A/A$ , or  $\lambda_k$ ,  $\lambda_L$ ,  $\Delta A^*/A^*$ , and  $b$  for any period by relating  $\Delta O/O$  to  $\Delta K/K$  and  $\Delta L/L$  may miss the point. The variables of the model are not independent, and  $\lambda_k$  and  $\Delta A^*/A^*$  are undoubtedly not constants.

Assume that the conclusions above are roughly correct; that changes in  $\Delta O/O$  are the result of interdependent changes in  $\Delta K/K$ ,  $\lambda_k$ , and  $\Delta A^*/A^*$ , and that  $\Delta L/L$  has been relatively constant compared with the other variables. Because changes in capital growth have been associated with changes in output growth, a simple linear regression would tend to give a high weight to  $(1-b)$  in a model that does not include the embodiment effect. In a model that did not assume constant returns to scale, the effect of embodiment could be misinterpreted as economies of scale (the coefficients adding up to more than one) and a relatively high static capital elasticity.

But we know more about the process of economic growth than the factors and relationships treated formally by the aggregative growth models we use. For example, if we believe that factor shares tend to equal the elasticities of output with respect to the factors, we are not justified in estimating  $b$  and  $(1-b)$  from a simple regression. If we believe that various components of total-factor-productivity growth are affected by growth of capital directly, or by the same variables that influence capital growth, we are not justified in assuming them to be constants, nor are we justified in estimating these variables by regression, ignoring this interaction effect. If we believe that the returns to education are related to the pace of technological change, we are not free to treat  $\lambda_k$  and  $\lambda_L$  as independent.

In sum, the aggregative production function may be a useful part of the framework for studying economic growth. But it is a mistake to try to introduce into the production function variables such as average years of education without an explicit theory that shows *how* that variable should be entered. Further, it is a mistake to ignore what we know about economic processes from other kinds of analysis. Regressions of growth of GNP with respect to growth of capital and labor should be constrained by what little we know of the relationship between output elasticities and factor shares. To accomplish this and also to take account of the other interactions discussed in this section probably will require that the production function be imbedded explicitly or in a richer, more complete model of economic growth [3].

### III. *Medium-Range Growth Projections*

The preceding analysis has some rather interesting implications with respect to medium-range (say one decade) growth projections. While the range of possible forecasting models is very wide, in order to limit the discussion it will be convenient to focus the discussion upon a com-

parison of the growth projections for the 1960-70 period that Robert Solow has derived from his model, and the projections of Edward Denison. However, great liberties will be taken in interpreting both of these authors.

### *The General Framework*

As shown in Section II, the amended Cobb-Douglas model can be written in three equivalent ways:

$$(10a) \quad \Delta O/O = \Delta A/A + b\Delta L/L + (1-b)\Delta K/K$$

$$(10b) \quad \begin{aligned} \Delta O/O = [\Delta A^*/A^* + b\lambda_L + (1-b)\lambda_k - (1-b)\lambda_k \Delta a] \\ + b\Delta L/L + (1-b)\Delta K/K \end{aligned}$$

$$(10c) \quad \begin{aligned} \Delta O/O = [\Delta A^*/A^* + b\lambda_L + (1-b)\lambda_k \bar{a}_{t-1} \delta] + b\Delta L/L \\ + (1-b)(1 + \lambda_k \bar{a}_{t-1}) \Delta K/K \end{aligned}$$

These are the basic equations we shall use for comparing alternative growth projections. For some purposes one version is more convenient, for other purposes another version. Note that all three versions can be interpreted as relating growth of output to growth of labor, growth of capital, and growth of productivity of capital and labor. The difference between equation (10a) and the other two versions is that equation (10a) contains no explicit interaction relationship.

In this paper we are not particularly concerned with the difficult problem of projecting potential labor input since the production functions being examined shed no light on that topic. For this reason the projection of the Bureau of Labor Statistics for labor force growth of about 1.7 or 1.8 per cent a year over the 1960-70 period will be accepted without criticism, and, for want of a better hypothesis, it will be assumed that, abstracting from cyclical considerations, the average work week will decline at .5 or .6 per cent a year. Thus potential man-hours will be assumed to grow at 1.1 to 1.3 per cent a year. This rate of growth is more than half again greater than the growth of potential man-hours during the 1929-60 period. This is mainly the lagged result of the relatively high birth rates of the postwar era.

Note that if the unemployment rate decreases over the period, the growth of actual man-hours would exceed the growth of potential man-hours as a result of cyclical gains in the employment rate, in the length of the average work week, and in the rates of labor force participation. Likewise, growth of actual output would exceed the growth of potential output due both to the cyclical effect on growth of man-hours and to the cyclical effect on productivity. Although the analysis of this section will be of potential output and input, not actual output, it is important to note that if the unemployment rate is 4.0 per cent in 1970 (as com-

pared with nearly 6.0 per cent in 1960), and if the Okun adjustments are correct, the annual rate of growth of actual GNP will average .6 percentage points greater than the rate of growth of potential GNP. Thus if potential grows at 4.0 per cent, and full employment is achieved in 1970, actual GNP will have grown at 4.6 per cent a year over the decade. And this is aside from any effects that decreasing economic slack would have on the rate of growth of potential GNP.

Once a rate of growth of labor input is assumed, the task of growth projection using the extended Cobb-Douglas model breaks naturally into two parts. The first is projection of a likely range for the rate of

TABLE 7—THE POSTWAR AND LONGER-RUN GROWTH RECORD\*  
(percentage annual growth rates)

Period	$\Delta O/O$	$\Delta K/K$	$\Delta A/A; b = .75$	$\Delta A/A; b = .5$
1929-60	3.1	2.0	2.1	1.7
1947-60	4.0	3.6	2.5	1.8

\* Data from Tables 1 and 2.

TABLE 8—COMPARATIVE PROJECTIONS\*  
(percentage annual growth rates)

	$\Delta O/O; b = .75$		$\Delta O/O; b = .5$	
	$\Delta A/A$ 29-60	$\Delta A/A$ 47-60	$\Delta A/A$ 29-60	$\Delta A/A$ 47-60
$\Delta K/K$ 29-60	3.5	3.9	3.3	3.4
$\Delta K/K$ 47-60	3.9	4.3	4.1	4.2

\* Data from Tables 1 and 2.

growth of the capital stock. The second is projection of a likely range of growth of total factor productivity. However,  $\Delta A/A$  and  $\Delta K/K$  can not be assumed independent.

Let us use the 1929-60 experience as a guide to the range of possibilities. The relevant numbers from Tables 1 and 2 are reproduced in Table 7.

Assuming  $\Delta L/L = .012$ , let us examine how the projected potential GNP growth rate depends on whether the 1929-60 experience or the 1947-60 experience holds during the 1960's for  $\Delta K/K$  and  $\Delta A/A$  for  $b = .75$  and  $b = .5$ , respectively.

Table 8 expresses the heart of the growth-projection problem. It matters a good deal whether the 1960-70 experience with respect to both growth of total factor productivity and growth of the capital stock resembles the postwar experience or the longer-run average experience.

Despite the fact that the simple Cobb-Douglas model (with factor shares providing estimates of output elasticities) places a low weight on growth of capital, the difference between the postwar rate of capital growth and the long-run average rate is so great that the difference matters greatly. Even in the low-capital-weight model ( $1-b=.25$ ), the difference in the rate of growth of capital over the two comparison periods makes almost as great a difference in the growth projections for the 1960's as do differences in the rate of growth of total factor productivity. And in the model in which capital is heavily weighted ( $1-b=.50$ ), they make almost all the difference.

As was pointed out in Section II, a model with a high correlation between rate of growth of total factor productivity and the rate of growth of the capital stock, but with a low static output elasticity with respect to capital, tends to yield the same conclusions as a low correlation, high static output elasticity model. It is not surprising, therefore, that the principal diagonals of the two matrices of Table 8 are almost identical. Solow's "embodied" technical change growth model brings out this point clearly.

### *Growth Projections with Complete Embodiment*

As was shown in Section II, since embodiment results in a high correlation between rate of growth of capital and rate of growth of total factor productivity, the "embodied" technical change model is capable of explaining changes in the rate of growth of potential GNP over the 1929-60 period without recourse either to a postwar acceleration of technical change or to a static elasticity of output with respect to capital significantly in excess of the share of capital in national income.

At the polar extreme, then, if all total factor productivity growth were the result of embodied technical change, and  $(1-b)$  were .25, then  $(1-b)\lambda_k$  over the 1929-60 period must have been 2.0 per cent a year. With  $\Delta A^*/A^*=\lambda_1=0$ , equation (10c) can be written:

$$(11) \quad \Delta O/O = [(1-b)\lambda_k \bar{a}_{t-1} \delta] + b\Delta L/L + (1-b)(1+\lambda_k \bar{a}_{t-1})\Delta K/K.$$

With  $\bar{a}_{t-1}=17$  (the average age of capital in 1960),  $\delta=.035$ ,  $\Delta L/L=.012$ , and  $(1-b)\lambda_k=.02$ , the growth rate would be the following function of the rate of growth of the capital stock:<sup>33</sup>

$$(11a) \quad \Delta O/O = .022 + .59\Delta K/K \text{ [Projection with full embodiment].}$$

If  $\Delta K/K=.02$  (the 1929-60 average), then  $\Delta O/O=.034$ . If  $\Delta K/K=.036$  (the 1947-60 average), then  $\Delta O/O=.044$ .

<sup>33</sup> The constant term of equation (11a) is  $(1-b)\lambda_k \bar{a}_{t-1} \delta + b\Delta L/L$ . The coefficient before  $\Delta K/K$  is, of course,  $(1-b)(1+\lambda_k \bar{a}_{t-1})$ .

*Projections with Limited Embodiment*

Equations relating the growth rate of potential GNP to the rate of growth of the capital stock also can be derived for the totally "disembodied" technological change extreme (equation (10a)). Assuming  $b = .75$  and assuming  $\Delta A/A = .02$  or  $.0025$  (the 1929-60 and 1947-60 rates respectively):

$$(12) \quad \Delta O/O = (.029) + .25\Delta K/K \text{ [Projection with no embodiment].}$$

$$(.034)$$

The values of  $\Delta O/O$  for  $\Delta K/K = .02$  and  $.036$ , for  $\Delta A/A = .02$  and  $.025$ , were given in Table 1.<sup>34</sup> Notice that the "embodied technical change projection" is more than twice as sensitive to the rate of growth of the capital stock as the more conventional model. Notice also that if equation (12) is used, the growth projector is forced to decide what rate of growth of total factor productivity to assume, for the experience of the 1929-60 period does not provide a unique answer.

This certainly is so for Denison. According to his model,  $\Delta A/A$  varied considerably over the period. Indeed both  $\lambda_k$  and  $\Delta A^*/A^*$  varied. Denison must make a choice here. The choice he actually makes is guided by several considerations.

Denison's analysis suggests several reasons why the rate of growth of total factor productivity should be less rapid during the 1960's than during the 1947-60 period. Increases in productivity resulting from declines in the work week should be much less important. There should be a decline in the rate of growth of average school years per worker. Productivity advance resulting from economies of scale should be smaller.

But, even assuming that this analysis is correct, and that the effect of the declining work week and of increasing educational standards will be weaker during the 1960's than in earlier decades, Denison still has some freedom left if he recognizes the fact that his "residual" was greater in the postwar era than in the prewar era. It is interesting that Denison ends up projecting a rate of technical change faster than the 1929-60 average, but significantly smaller than the 1947-60 average.<sup>35</sup> And, since he projects a decline in the effect of variables that contribute to growth of total factor productivity, he ends up actually projecting a slightly smaller rate of growth of total factor productivity during the

<sup>34</sup> The two constants of equation (12) result from the two different assumptions about  $\Delta A/A$ . The constants are  $\Delta A/A + b\Delta L/L$ .

<sup>35</sup> It should be noted that my use of 1947 as a dividing line between the postwar and earlier periods is critical here. Although Denison's projected rate of technological change is less than the 1947-60 average, it is roughly equal to the 1954-60 average rate, and to the 1950-60 average rate.

1960-70 period than during the 1929-60 period, not just than during the 1947-60 period.<sup>36</sup>

Denison's "best guess" projection is for a 3.3 per cent annual growth of potential GNP, with a growth of the capital stock of 2.5 per cent a year. Taking great liberties with Denison's formulation, assuming that  $(1-b)\lambda_k = .005$  and that all of  $\lambda_k$  needs to be embodied (which Denison correctly would deny), the basic Denison growth equation can be written:<sup>37</sup>

$$(13) \quad \Delta O/O = .025 + .33\Delta K/K \text{ [Denison, with full embodiment of } \lambda_k].$$

This yields a 3.3 per cent growth rate of  $\Delta K/K = .025$ . If  $\Delta K/K = .036$ , as it has averaged since 1947,  $\Delta O/O = .036$ .

But the growth-projection equation that Denison finally reaches is far less important than the analysis on which he bases his projection. The assumptions of interest are, first, that several of the factors contributing to growth of total factor productivity will be less important during the 1960's than earlier and, second, that embodiment is quite unimportant.

Denison undoubtedly is correct in assigning a smaller weight to  $\lambda_k$  than does the pure embodied technical change model. However, due to the strong links between education and technical change, his attempt to treat the effects of technical change as a residual undoubtedly leads to an understatement of the importance of technical change. He also undoubtedly is correct in arguing that often technological change requires only a small amount of new capital. But if the analysis of Section II is correct, he probably also has underestimated the importance of other, if less direct, connections between growth of capital and growth of total factor productivity. (Of course these are not treated by the formal embodiment model either.) And, although he may be right that the longer-run experience of growth of total factor productivity is a better prediction for the 1960's than the postwar experience, the point is surely open to question. Thus his estimates both of the constant and the capital growth sensitivity terms may well be low.

### *The Major Uncertainties*

While other projection models certainly could provide higher or lower forecasts, the comparison of the Solow and Denison projections suggests the range of uncertainty with respect to the medium-range

<sup>36</sup> This point of course is not dependent upon choice of subperiods.

<sup>37</sup> The constant in equation (13) is derived from Denison's projection of  $\lambda_L$  and  $\Delta A^*/A^*$  on the assumptions that  $b = .75$  (slightly smaller than Denison's assumption) and that  $\Delta L/L = .012$ . The coefficient before  $\Delta K/K$  is derived from the assumption that  $(1-b)\lambda_k = .005$ .  $\lambda_k$  and  $\Delta A^*/A^*$  were adjusted to add up to be consistent with Denison's projection for technological change plus changes in efficiency and economies of scale.

growth prospects of the U. S. economy and helps to identify the principal causes of the uncertainty.

To the extent that the experience of the 1929-60 period can shed light on the prospects for the future, during the 1960 period growth of potential GNP is likely to be somewhere between an annual rate of 3.2 per cent and 4.3 per cent. The high end of the range will be achieved if the rates of growth of the capital stock and of total factor productivity are at the postwar rate, the lower end if they are at the longer-run average rate.

Much of the uncertainty, therefore, relates to the interpretation of the acceleration of growth of capital and of total factor productivity in the postwar period. One interpretation is that the postwar spurt essentially represents a making up of ground lost during the Great Depression, that the slowdown during the past several years shows that the "making up" has been completed, and that the 1929-60 average represents an average of below-normal growth and above-normal growth. According to this interpretation we should not expect growth of the capital stock or of total factor productivity during the 1960-70 period to differ greatly from the 1929-60 average rates, and the recent slowdown suggests that to project the 1947-60 rates would be unrealistically optimistic.

Another interpretation is that the 1929-60 average is much too heavily influenced by the depression decade of the 1930's. While it probably is true that the very rapid rate of growth of capital and of total factor productivity during the 1947-54 period was, in large part, a make-up phenomenon, this cannot be said of the post-1954 period. It should be recalled that the rate of growth of the capital stock, since 1954, has been significantly greater than the 1929-60 average and, after adjusting for changing degrees of slack, the same is true of growth of total factor productivity. Further, it is quite clear that the average rate of growth of the capital stock since 1954 has been less than it would have been had fiscal and monetary policy been more effective in keeping aggregate demand pressing on aggregate potential. Growth of total factor productivity undoubtedly also has been depressed by economic slack.

Thus, according to this interpretation, the 1954-60 record (even more the 1929-60 record) significantly understates the growth of the capital stock and the growth of total factor productivity we may expect in an economy where aggregate demand is not permitted to lag significantly behind growth of economic potential.

Further, according to the more optimistic interpretation, the postwar increase in the rate of R&D spending has not been without effect. The latent rate of technical change—the rate at which productivity could

be increased as a result of new technical knowledge were there sufficient demand to spur innovation and reduce resistance, and sufficient investment to embody the new technology and to permit high labor mobility—probably has been greater in the postwar era than before. Even with the economic sluggishness of the past five years, which certainly has made innovation more difficult, the rate of growth of potential total factor productivity has exceeded its long-run average.

From analysis of the Solow and Denison projections it is clear that this difference in interpretation lies at the root of the differences in the growth projections. Perhaps looking at the experience of the economy over a longer period of time than 1929–60 can shed some light on which interpretation is likely to be more nearly correct. Between 1909 and 1960 the rate of growth of the capital stock averaged approximately 2.5 per cent a year, and between 1909 and 1929 the average annual rate of growth of the capital stock was approximately 3.0 per cent. This suggests that the 1929–60 capital growth experience probably was heavily affected by the depression years. And to the extent that growth of capital and growth of total factor productivity are correlated, this suggests that the 1929–60 experience for  $\Delta A/A$  also was heavily affected by the depression.

The experience of the past few years also suggests that since the war there has been a change and that it would be a mistake to extrapolate the longer run 1929–60 experience into the future. From 1959–62, potential GNP has grown at an annual rate of greater than 3.5 per cent. This is faster than the rate that Denison projects for the future. And yet, from 1959–62 the growth of labor input was significantly less than we can expect during the middle and late 1960's, and, as a result of persistent economic slack, the capital stock grew at an annual rate of only about 2.0 per cent. If we have in fact achieved an annual growth potential of approximately 3.5 per cent under these conditions, we certainly should do better as the rate of growth of the labor force increases, provided we can achieve and maintain adequate aggregate demand.

Although Solow's model undoubtedly overstates the correlation between growth of capital and growth of total factor productivity, Denison's model undoubtedly understates it. It is likely that during the 1960's the rate of growth of the capital stock and of total factor productivity will both be near the high postwar rates or neither will be. If this is correct, and if it also is correct that one of the major reasons for the postwar acceleration has been the absence of deep recession and (save for the past several years) prolonged slack, our growth record during the 1960's may be more dependent on an ability to reduce economic slack than on any other measure. We shall have full employment and rapid growth of potential GNP together or we shall have neither.

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## MANUFACTURING INVESTMENT, EXCESS CAPACITY, AND THE RATE OF GROWTH OF OUTPUT

By ALICE BOURNEUF\*

Since the mid-fifties the United States has operated at less than full utilization of the labor force and plant capacity, even at the peaks of mild output fluctuations. A higher rate of growth of investment would have led to higher total output, assuming conventional multiplier effects. Another relationship may exist between the growth of output and investment in response to it. Would a higher rate of growth of output lead to correspondingly larger investment? If output fluctuated less, would investment be more stable, or higher? This study aims to throw some light on these questions. It analyzes manufacturing investment in the United States since 1950. Linear regression equations indicate close relationships between investment, excess capacity, and output changes, both in total manufacturing and in most of the industries analyzed. These equations can be used to obtain tentative answers to the above questions, assuming the relationships found are not themselves altered by changed conditions of output growth.

To analyze the effects of output changes on investment is to test some form of the acceleration principle. In its simplest mechanical form, the accelerator has failed to explain short-period changes in investment. A more flexible form of the accelerator is used in this study; investment is assumed to be negatively related to the difference between capacity output and actual output, positively related to total capacity, and positively related to the change in output. The acceleration principle cannot provide a complete explanation of investment, but it may explain a substantial part of it. Edwin Kuh emphasized recently that real and financial factors must be considered complementary [4, pp. 260-68] and suggested that accelerator-type forces may be more important in periods of pressure on capacity. This paper tests the accelerator in a period during most of which there was little or no pressure on capacity.

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### *I. Data Analyzed*

The choice of investment and output data was dictated by the desire to analyze individual industries. Investment data are the U. S. Commerce Department and Securities and Exchange Commission expenditures on new plant and equipment, converted to 1954 constant prices by using the Commerce Department's index of the prices paid by private purchasers of plant and equipment [13] [14]. Federal Reserve production indices were found for 13 industries for which investment data are available [9] [10]. The output indices were converted to 1954 value added by manufacture dollars by multiplying the ratio of the output index in any year to the 1954 index by the 1954 value added by manufacture.<sup>1</sup>

Measures of output capacity were used instead of estimates of the value of capital stock. It seemed worth while to experiment with the industry-capacity indexes compiled by McGraw-Hill and the total manufacturing-capacity indexes of McGraw-Hill and Frank de Leeuw. Capital stock estimates based on book-value data would have required much more extensive statistical work and raised difficult problems of valuation and depreciation.<sup>2</sup>

Measures of capacity also raise problems. An ideal measure might be capacity under normal operations, an engineering concept, or capacity at minimum average cost, or below some level of marginal cost, accounting concepts.<sup>3</sup> The capacity indexes used in this study are not ideal in either sense; they probably are closer to the engineering concept. McGraw-Hill indexes are calculated from capacity as individual manufacturers see fit to report it, and these manufacturers report preferred operating rates at 90 to 100 per cent of capacity. The de Leeuw index, which is based on three separate series, attempts to minimize bias and errors. The series are the McGraw-Hill capacity index, one of the new gross capital stock estimates of the Department of Commerce [3],

<sup>1</sup> The Census value-added totals were adjusted as far as possible to the coverage of the production indices, and the production indices to the coverage of the investment data. But the investment data are on a company basis, and the Census and Federal Reserve data on an establishment basis. Investment estimates include large expenditures for nonmanufacturing establishments, especially in primary metals and petroleum. For the source of the investment data see [13] and [14], of the production data see [9] and [10].

<sup>2</sup> Capital stock estimates for selected years for many industries have been compiled by the National Industrial Conference Board, see [1] and [2]. The capacity indexes of McGraw-Hill are in [11], of de Leeuw in [6] in the form of quarterly operating rates. End-of-year capacity indexes were furnished the author by de Leeuw.

<sup>3</sup> See [5] [6] and [7] for a discussion of problems of capacity measurement, and [6] for an explanation of the construction of de Leeuw's index. The de Leeuw index rises 1 per cent more per year than the Commerce series, and  $1\frac{1}{2}$  per cent less per year than the McGraw-Hill index, which in fact rises at the rate of the Commerce series for equipment only at Bulletin F lives.

and an index based on output data and on operating rates reported to McGraw-Hill.

The capacity indexes were converted into capacity output in billions of 1954 value-added dollars. The year 1955 was used as a base, since operating rates reported at the end of both 1954 and 1955 were close to capacity. Calculations of total output capacity based on operating rates and output for 1955 were, therefore, likely to be less subject to error than similar calculations for years of low operating rates. Capacity for the year 1955 was calculated by multiplying the 1955 output, in 1954 value-added dollars, by the reciprocal of the average of the two end-of-year operating rates.

## II. *Investment Equations*

A great many equations were fitted to the data since 1950.<sup>4</sup> The best results were obtained from the following equation, in which  $I$  is investment,  $C$  capacity,  $Y$  output, and  $C_{bt}$  is capacity at the beginning of the year  $t$  as distinct from  $C_t$ , which is average capacity for the year.

$$(1) \quad I_t = a(C_{t-1} - Y_{t-1}) + bC_{bt} + c \Delta Y_t + k.$$

The excess-capacity variable should have a negative effect on investment;  $C_{bt}$  is meant to catch replacement investment; and  $\Delta Y_t$  should have a positive effect on investment.

The equations fitted for total manufacturing and for most of the individual industries tested are reasonably good fits. The fact that many of the equations for individual industries are satisfactory suggests that the equation for total manufacturing has a sound basis in the behavior of individual industries.<sup>5</sup> The equations met one test of their estimating ability fairly well. Equations fitted to 1950-61 data were used to estimate 1962 investment. For total manufacturing and for nine of the 13 industries, the differences between the estimates and

<sup>4</sup>All of the equations tested were variations on the flexible accelerator theme. Equations fitted to total output, total capacity, and changes in output gave poor results, as did equations using the same variables in ratio form to avoid collinearity. Lagged changes in output did not help. When a direct measure of excess capacity looked promising, several experiments were made with lagged output changes, and with some variable to explain the important steady amount of replacement investment.

<sup>5</sup>The use of  $\Delta Y_t$  may be questioned on the ground that it may reflect the investment change rather than vice versa. The author believes this is not serious. Experiments with a change in GNP term instead of the change-in-output term produced very poor results. The simple correlation of changes in manufacturing output and changes in GNP is close, but the standard error of estimate of the regression equation equals about one-fourth of the average change in GNP. Changes in government expenditures and in residential construction have been calling the tune for changes in GNP. In any case the problem cannot be considered serious for the individual industry equations.

TABLE 1—COMPARISON OF ESTIMATED AND ACTUAL INVESTMENT FOR 1962 AND 1963  
(in billions of 1954 dollars)

	Estimated from Equation	Actual Total	Difference: Estimated minus Actual	Standard Error of Estimate of Equation	Ratio of Difference to Standard Error of Estimate
<b>PART A: TOTAL MANUFACTURING</b>					
Equations from 1950-61 data					
McGraw-Hill 1962	12.39	11.83	.56	.727	.77
de Leeuw 1962	12.21	11.83	.38	.639	.59
McGraw-Hill 1963	14.30	12.49	1.81	.727	2.48
de Leeuw 1963	14.02	12.49	1.53	.639	2.39
Equations from 1950-62 data					
McGraw-Hill 1963	14.05	12.49	1.56	.699	2.23
de Leeuw 1963	13.85	12.49	1.36	.609	2.23
<b>PART B: INDIVIDUAL INDUSTRIES</b>					
ESTIMATES FOR 1962					
Equations from 1950-61 data					
1. Electrical Machinery	.587	.561	.026	.029	.89
2. Nonelectrical Machinery	.990	1.03	.040	.074	.54
3. Pulp and Paper	.659	.593	.066	.061	1.08
4. Petroleum	2.660	2.34	.320	.214	1.49
5. Stone, Clay, and Glass	.471	.471	.000	.055	.00
6. Textiles	.426	.496	— .070	.040	-1.75
7. Chemicals	1.43	1.27	.160	.122	1.31
8. Rubber and Rubber Products	.171	.187	.016	.018	.88
9. Transport Equipment (Excluding Motor Vehicles)	.414	.382	.032	.067	.47
10. Iron and Steel	.938	.834	.104	.266	.39
11. Motor Vehicles and Parts	.930	.675	.255	.317	.80
12. Nonferrous Metals	.263	.252	.011	.136	.08
13. Food and Beverages	.668	.805	— .137	.078	-1.75

	Coefficients and Standard Errors					Mean Investment	Standard Error of Estimate	$r^2$	$r^2$	Von Neuman Ratio
	$C_{t-1} - Y_{t-1}$	$C_{it}$	$\Delta Y_t$	$k$						
Investment Equations										
McGraw-Hill 1950-61	-.264 (.044)	+.143 (.024)	+.150 (.039)	-4.87		11.3	.727	.826	.740	1.53
1950-62	-.260 (.041)	+.139 (.022)	+.142 (.035)	-4.32		11.4	.699	.823	.743	1.51
de Leeuw 1950-61	-.289 (.041)	+.095 (.016)	+.152 (.034)	+2.61		11.3	.639	.867	.799	1.39
1950-62	-.287 (.039)	+.092 (.014)	+.148 (.031)	+2.98		11.4	.609	.865	.805	1.38
Capacity Equations										
McGraw-Hill 1950-61		$I_{t-1}$	$C_{it-1}$	$k$		Mean Capacity	Standard Error of Estimate	$r^2$	$r^2$	Von Neuman Ratio
1950-62		.663 (.354)	1.005 (.019)	+.083		150.2	1.89	.998	.996	2.99
de Leeuw 1950-61		.731 (.361)	.993 (.018)	+.840		154.3	1.95	.998	.996	2.64
1950-62		.676 (.226)	1.003 (.018)	-2.295		148.9	1.21	.998	.996	3.08
		.695 (.215)	.999 (.015)	-1.88		149.8	1.16	.988	.998	3.01

Symbols:

 $I$  = investment $C$  = capacity measured in output terms $Y$  = output $C_{it}$  = capacity at beginning of year  $t$

actual investment were less than, or close to, one standard error of estimate of the relevant equation; the differences in the other four industries were less than two standard errors (see Table 1). Provisional estimates for total manufacturing for 1963 were not so successful, even when equations were fitted to 1950-62 data.

The equations based on the de Leeuw index of total manufacturing capacity perform better than those based on the McGraw-Hill index. The de Leeuw equations are slightly better fits than the McGraw-Hill equations (see Table 2). In all of the total manufacturing equations the coefficients are significant at the .05 level and have appropriate signs. The correlation coefficients are about as high as can be expected with such a simple model. The standard errors of estimate of the de Leeuw equations are smaller than those of the McGraw-Hill equations, but the latter are well below 10 per cent of average investment.

### III. *Capacity Equations*

An analysis of the effect of output changes on investment must consider not only the direct effect of output changes on investment but also the indirect effect of output changes on excess capacity, and, through excess capacity, on investment. Furthermore, investment, as affected by output changes, then increases capacity, excess capacity, and replacement needs, and, therefore, in turn affects investment in future years. To analyze the full influence of output changes on investment over time an equation is needed to estimate the effects of investment on total capacity or on changes in capacity. A complete explanation of capacity would require many variables. With only 12 observations only simple equations could be tested, relating changes in capacity to investment this year or last year, or to last year's change in capacity. The equations relating this year's changes in capacity to this year's investment gave tolerably good results; to simplify the later analysis and computations, the equation was put in terms of total capacity at the beginning of successive years, as follows:

$$(2) \quad C_{bt} = a I_{t-1} + b C_{bt-1} + k.$$

The capacity equations for total manufacturing based on the de Leeuw index are fairly satisfactory (see Table 2), but those based on the McGraw-Hill index are less so. In the McGraw-Hill equations the coefficients of the  $I_{t-1}$  term are not significant at the .05 level. The correlation coefficients are very high, mainly because of the high degree of autocorrelation in the capacity series. The standard errors of estimate of the equations are tolerable, as they amount to about 15 per cent of the average change in capacity.

#### IV. *Investment and Capacity over Time*

An incomplete linear model of the behavior of investment and capacity over time may be obtained by combining equations (1) and (2) into a second-order difference equation in capacity and then assuming various output growth rates. Each of the four possible combinations of investment and capacity equations has roots which are positive and less than one. Assuming an exponential output growth rate, investment and capacity will eventually grow at the same rate, and, starting from any given initial conditions, there will be a gradual approach to the steady-state solution since the roots are positive and less than one.

Steady-state solutions at various assumed output growth rates yield unrealistic amounts of excess capacity at low growth rates, but implied excess-capacity percentages fall sharply as output growth rates rise.<sup>6</sup> The unrealistic solutions reflect the fact that the equations were fitted during a period of increasing excess capacity, in spite of an average 4.5 per cent growth of output. The McGraw-Hill steady-state solutions are much more unrealistic than the de Leeuw solutions because the McGraw-Hill capacity index implies a much greater increase over the period in excess capacity.<sup>7</sup> The McGraw-Hill solutions imply great insensitivity of investment to excess capacity, the de Leeuw solutions a considerable degree of sensitivity. Both sets of equations indicate that output growth rates strongly affect the steady-state solutions.

Iterations of the McGraw-Hill and de Leeuw equations fitted to 1950-62 data, on six different output growth assumptions, are summarized in Table 3. Given the 1962 initial conditions, the first three examples compare fluctuating growth rates with the equivalent steady growth rate. The annual percentage increases in the first two examples are those which occurred from 1953-63; the order has been rearranged to start with the approximate 1963 increase in each case, and yet to follow a pattern of fluctuations similar to that from 1953 to 1963. The third example assumes the equivalent steady growth rate from 1953 to 1963. With fluctuating output growth, investment also fluctuates; steady output growth leads to increases in investment each year. Although these three examples do not warrant generalizations about the effects of steady growth rates, compared to any possible series of fluctuating rates which result in the same total growth, they do suggest that steady growth is likely to produce more investment than

<sup>6</sup> At a zero rate of growth of output the McGraw-Hill equations lead to 50 per cent excess capacity, the de Leeuw equations to 32 per cent.

<sup>7</sup> From 1949-52 to 1959-62 the average percentage of excess capacity implied by the McGraw-Hill index rose 300 per cent and that implied by the de Leeuw index 25 per cent. See Table 6.

TABLE 3--TOTAL MANUFACTURING; PROJECTIONS OF INVESTMENT, OUTPUT, AND CAPACITY ON VARIOUS GROWTH OF OUTPUT ASSUMPTIONS  
Iterations with McGraw-Hill and de Leeuw Equations Estimated from 1950-62 Data

	Assumed Annual Percentage Increases in Output					
	(1)	(2)	(3)	(4)	(5)	(6)
1963	4.9	4.9	2.93	4.0	4.5	5.0
1964	3.0	-6.9	"	"	"	"
1965	0.6	12.7	"	"	"	"
1966	-9.3	3.0	"	"	"	"
1967	13.7	0.6	"	"	"	"
1968	2.8	-9.3	"	"	"	"
1969	0.7	13.7	"	"	"	"
1970	8.2	2.8	"	"	"	"
1971	-6.9	0.7	"	"	"	"
1972	12.7	8.2	"	"	"	"
Indices 1962=100						
	(1)	(2)	(3)	(4)	(5)	(6)
Output						
	M <sup>1</sup>	L <sup>2</sup>	M <sup>1</sup>	L <sup>2</sup>	M <sup>1</sup>	L <sup>2</sup>
Avg. 1963-1972	114	114	118	118	125	125
1972	132	132	133	133	148	148
Investment						
	M <sup>1</sup>	L <sup>2</sup>	M <sup>1</sup>	L <sup>2</sup>	M <sup>1</sup>	L <sup>2</sup>
Avg. 1963-1972	113	106	124	117	144	135
1972	117	106	134	123	172	157
					154	145
					191	175
					164	155
					211	194

<sup>1</sup> M indicates results based on McGraw-Hill equations.

<sup>2</sup> L indicates results based on de Leeuw equations.

TABLE 3—(Continued)

fluctuations of the character experienced from 1953 to 1963. The equations based on the de Leeuw index are more sensitive to output growth rates than those based on the McGraw-Hill index, in the sense that the percentage changes in the steady-state solutions for excess capacity are much greater as the assumed output growth rates rise.

The four examples assuming steady output growth rates indicate that, given 1962 initial conditions, higher growth rates would produce much more investment and much higher ratios of investment to output over the next 10 years. Both the McGraw-Hill and de Leeuw equations show that investment is very sensitive to the output growth rate; the de Leeuw equations are somewhat more sensitive in the sense that the percentage changes in average investment are somewhat higher as output growth rates increase. Sensitivity to excess capacity, on the other hand, is much greater with the de Leeuw index than with the McGraw-Hill index, as already indicated by the steady-state solutions. The percentages of excess capacity reached in 1971 and 1972 bring out the extreme insensitivity to excess capacity of the McGraw-Hill equations. At the 4.5 per cent steady rate of growth, the approximate equivalent of the fluctuating output increases from 1949 to 1962, excess capacity continues to increase with the McGraw-Hill equations, and at 5 per cent it remains at the 1962 level; with the de Leeuw equations at these growth rates excess capacity is lower in 1972, and at 5 per cent growth the steady-state solution of 10 per cent excess capacity is already reached.

### *V. Industry Investment Equations*

The industry investment equations fitted to 1950-61 data, shown in Table 4, are listed roughly in the order of reasonableness of the equations. The first eight equations are fairly satisfactory. The coefficients of the independent variables have appropriate signs; all of the coefficients of the excess capacity and capacity terms are significant at the .05 level, but in two cases the coefficients of the change in output term are not. The correlation coefficients are about as high as can be expected from such a simple model. The standard errors of estimate of the equations are below 10 per cent of the mean investment in all but two cases, and only slightly higher in those cases. In only one of the first eight equations is there any evidence of autocorrelation of residuals.<sup>8</sup>

The first eight industries accounted for 56 per cent of investment in 1954, the last five industries for 29 per cent, and industries for which data are not available, the remaining 16 per cent of manufactur-

<sup>8</sup> Since the equations do not pretend to fully explain investment, it is not surprising that there is some evidence of autocorrelation of residuals. On the whole the coefficients do not seem to be biased on this account.

TABLE 4—INDUSTRY INVESTMENT EQUATIONS FITTED TO 1950-61 DATA

	Coefficients and Standard Errors				Mean Investment	Standard Error of Estimate	Correlation Coefficients		Von Neuman Ratio <sup>1</sup>
	$C_{t-1}-Y_{t-1}$	$C_{it}$	$\Delta Y_t$	$k$			$r^2$	$\bar{r}^2$	
1. Electrical Machinery	-.092 (.014)	+.057 (.007)	+.057 (.012)	-.137	.448	.029	.912	.867	1.40
2. Non-electrical Machinery	-.102 (.021)	+.106 (.019)	+.074 (.022)	-.516	.793	.074	.830	.745	1.13
3. Pulp and Paper	-.599 (.153)	+.213 (.042)	+.362 (.121)	-.538	.510	.061	.789	.684	1.96
4. Petroleum	-5.22 (1.11)	+1.09 (.336)	+2.38 (1.16)	+.536	2.41	.214	.759	.668	1.49
5. Stone, Clay, and Glass	-.390 (.100)	+.248 (.057)	+.309 (.079)	-.604	.421	.055	.719	.579	1.30
6. Textiles <sup>2</sup>	-.389 (.100)	+.134 (.048)	+.186 (.063)	-.143	.369	.040	.714	.522	1.46
7. Chemicals	-.360 (.102)	+.053 (.017)	+.032 (.088)	+1.24	1.22	.122	.704	.555	1.91

<sup>1</sup> At .05 level with 12 observations, limits are 1.23 and 3.13.<sup>2</sup> 10 observations.

Symbols:

 $C$  = capacity measured in output terms $Y$  = output $C_{it}$  = capacity at beginning of year  $t$  $k$  = constant

TABLE 4—(Continued)

	Coefficients and Standard Errors			Mean Investment	Standard Error of Estimate	Correlation Coefficients		Von Neuman Ratio <sup>1</sup>
	$C_{t-1} - Y_{t-1}$	$C_{it}$	$\Delta Y_t$			$r^2$	$r^2$	
8. Rubber and Rubber Products	-.168 (.062)	+.094 (.030)	+.082 (.042)	.154	.018	.573	.359	1.59
9. Transport Equipment (Excluding Motor Vehicles)	-.005 (.017)	+.027 (.008)	+.041 (.021)	.276	.067	.676	.514	1.33
10. Iron and Steel	-.282 (.150)	+.292 (.183)	+.125 (.116)	1.08	.266	.340	.009	1.79
11. Motor Vehicles and Parts	-.284 (.179)	+.165 (.142)	+.099 (.117)	.873	.317	.292	.004	.79
12. Nonferrous Metals	-.589 (.413)	+.346 (.300)	-.016 (.353)	.334	.136	.412	.118	2.31
13. Food and Beverages	+.001 (.184)	-.019 (.081)	-.001 (.153)	.759	.078	.329	.000	.89

ing investment.<sup>9</sup> The reasons why five industries gave poor results are analyzed below.

### VI. *Analysis of Coefficients*

The coefficients of the equations for total manufacturing and for the 13 industries appear unreasonable at first glance. The excess-capacity coefficients in some cases seem much too high, and the coefficients of the  $C_{bt}$  term in most cases seem too high. The high coefficients reflect the fact that capacity is measured in 1954 value added by manufacture-output terms, rather than in capital stock terms. A million dollars of capacity is the capital stock needed for one million dollars a year of value added by manufacture. When the coefficients are roughly adjusted to capital stock terms, they seem quite reasonable (Table 5). The adjustments were made as follows. The estimates of net capital stock in constant 1954 dollars of the National Industrial Conference Board for the years 1953 and 1955 were used to calculate ratios of net capital to value-added output; the lower of the two ratios thus calculated was assumed to be closer to the full-capacity output ratio; the lower ratio was then converted to a gross capital to value-added ratio by means of the relation between the appropriate new Department of Commerce gross and net capital stock estimates for 1953, in constant 1954 dollars.<sup>10</sup>

The ratio of gross capital to value-added for manufacturing seems reasonable. Furthermore the coefficients in the equations for total manufacturing seem reasonable when roughly adjusted to the capital stock basis. In both the McGraw-Hill and de Leeuw equations the speed of adjustment to excess capacity is approximately one-sixth of the excess capacity; the de Leeuw equation shows somewhat greater speed of adjustment. The coefficient of the  $C_{bt}$  term, meant to measure replacement investment, when adjusted to the capital stock basis seems highly reasonable in the de Leeuw equation (5.5 per cent), but is still

<sup>9</sup> The industries for which comparable capacity and output data were not available are other fabricated metal products and various miscellaneous groups such as furniture and apparel. Two of the three industries accounting for the most investment are in the first eight, petroleum and chemicals; the third, motor vehicles, is in the last five industries.

<sup>10</sup> The NICB net estimates are in [2], App. II, Table A-2. The NICB net estimate for total manufacturing is \$95.5 billion for 1953, assuming straight line depreciation; the U.S. Commerce Dept. estimate is 58 billion 1954 dollars for 1953, assuming straight-line depreciation and Bulletin F lives. The principal reason for the higher NICB figure is that their data are on a company basis, rather than on an establishment basis, but there are other important differences in sources and methods. Since the data on plant and equipment expenditure are also on a company basis, the NICB figures for individual industries may not be much too big. The U.S. Dept. of Commerce gross figure for 1953, on the same basis, was 125 billion 1954 dollars, so each of the net ratios was multiplied by a factor of 2.2 to convert it roughly to a gross basis. This amounts to assuming equal plant and equipment lives in all industries. For U.S. Commerce Dept. estimates, see [3].

TABLE 5—ANALYSIS OF COEFFICIENTS OF INVESTMENT EQUATIONS

	(1) Estimated Ratio of Gross Capital Stock to Value-Added, 1954	(2) Coefficient of $C_{t-1} - Y_{t-1}$		(3) K Stock Measure		(4) de Leeuw Basis		(5) Output Measure		(6) K Stock Measure		(7) de Leeuw Basis		(8) Output Measure		(9) K Stock Measure	
		Output	Measure	(2) ÷ (1)	$1.10 \times (3)$	Output	Measure	(5) ÷ (1)	$.66 \times (6)$	Output	Measure	(5) ÷ (1)	$.66 \times (6)$	Output	Measure	(8) ÷ (1)	$(8) + (1)$
Total Manufacturing <sup>1</sup>																	
a) McGraw-Hill	1.67	-.260		-.156	-.172	.139		.088	.055	.142		.055		.142		.09	
b) de Leeuw	1.67	-.287		-.172	—	.092		.055	—	.148		—		.148		.09	
Individual Industries <sup>2</sup>																	
1. Electrical Machinery	.75	-.093		-.12	-.13	.057		.08	.05	.057		.05		.057		.08	
2. Nonelectrical Machinery	1.01	-.102		-.10	-.11	.106		.11	.07	.074		.07		.074		.07	
3. Pulp and Paper	2.00	-.599		-.30	-.33	.213		.11	.07	.362		.07		.362		.18	
4. Petroleum	16.59	-5.22		-.31	-.34	1.09		.07	.04	2.38		.04		2.38		.14	
5. Stone, Clay, and Glass	1.67	-.390		-.33	-.25	.248		.15	.10	.309		.10		.309		.19	
6. Textiles	1.94	-.389		-.20	-.22	.134		.07	.04	.186		.04		.186		.10	
7. Chemicals	2.24	-.360		-.16	-.18	.053		.02	.01	.032		.01		.032		.01	
8. Rubber and Rubber Products	1.32	-.168		-.13	-.14	.094		.07	.04	.082		.04		.082		.06	
9. Transport Equipment (Excluding Motor Vehicles)	.44	-.005		-.01	-.01	.027		.06	.04	.041		.04		.041		.09	
10. Iron and Steel	2.53	-.282		-.11	-.12	.292		.12	.08	.125		.08		.125		.05	
11. Motor Vehicles and Parts	2.09	-.284		-.14	-.15	.165		.08	.05	.099		.05		.099		.05	
12. Nonferrous Metals	4.47	-.589		-.13	-.14	.346		.08	.05	-.016		.05		-.016		-.004	
13. Food and Beverages	1.45	+.001		+.001	+.001	-.019		-.01	-.007	-.001		-.007		-.001		-.001	

<sup>1</sup> Equation based on 1950-62 data.<sup>2</sup> Equations based on 1950-61 data.

Source: For estimated ratio of gross capital stock to value added, see footnote 10.

Symbols:

 $C$  = capacity $Y$  = output $C_{0t}$  = capacity at beginning of year

much too high in the McGraw-Hill equation (8.8 per cent). This is the most important difference between the two equations. The coefficients of the change in output terms are almost identical and suggest that manufacturers plan to expand capacity up to only 9 per cent of the addition to output. Direct effects of output changes are small, but it must be remembered that additions to output decrease excess capacity, and therefore increase investment, and that higher investment increases total capacity, and therefore in turn leads to more replacement investment, but also more excess capacity. The sensitivity of investment to output growth rates brought out in the steady-state solutions and iteration examples results from the direct and indirect effects of the output growth rates assumed. The high coefficient of the  $C_{b1}$  term is primarily responsible for the much higher excess-capacity solutions of the McGraw-Hill equations.

Since the de Leeuw equation results in more reasonable coefficients and more reasonable steady-state solutions for total manufacturing than the McGraw-Hill equation, a further adjustment of the industry equation coefficients may be in order because they are based on McGraw-Hill capacity data. Columns 4 and 7 of Table 5 adjust the coefficients in the industry equations to a "de Leeuw basis." Both of the adjustments made in the coefficients are very rough, but it is abundantly clear that coefficients in the industry equations that seemed completely out of line and unreasonable fall into line when adjusted. The capital stock adjustment is much the most important, except for the  $C_{b1}$  term. In that case the adjustment to the "de Leeuw basis" is also substantial and leads to more reasonable results.

An analysis of the adjusted coefficients brings out the fact that the first eight industries, for which the equations gave good results, are much more sensitive to excess capacity than the last five, and also much more sensitive directly to changes in output. The lower sensitivities are not enough by themselves to explain why the last five equations are not good fits, but they are consistent with the explanation offered below.

### VII. *Industries with Poor Results*

The last five industries, for which the equations gave poor results, include consumers' goods and producers' goods, industries in which demand is stable, and industries in which demand fluctuates violently. Particular circumstances might explain why each of these industries gave poor results.<sup>11</sup> But a more general explanation seems more con-

<sup>11</sup> Some of these might be strikes in iron and steel, defense contracts in transport equipment excluding motor vehicles, and the difficulty of measuring capacity in an industry like food and beverages. Statistical problems may explain the poor results in some cases; for

TABLE 6—EXCESS CAPACITY AS PERCENTAGE OF TOTAL CAPACITY  
MEASURED IN OUTPUT TERMS

Annual Average of Percentages of Excess Capacity

	1949-52	1953-55	1956-58	1959-62	1949-62
<b>A. TOTAL MANUFACTURING</b>					
1. McGraw-Hill	6.56	11.41	21.13	23.53	15.57
2. de Leeuw	13.46	11.30	16.61	16.35	14.50
<b>B. INDIVIDUAL INDUSTRIES</b>					
1. Electrical Machinery	-2.52	1.58	15.00	17.70	7.89
2. Nonelectrical Machinery	13.75	21.34	36.42	41.56	28.18
3. Pulp and Paper	1.94	4.47	7.81	10.72	6.25
4. Petroleum	7.36	7.33	10.15	9.06	8.44
5. Stone, Clay, and Glass	5.49	11.91	19.66	22.53	14.77
6. Textiles	9.49	12.13	16.00	13.91	13.25
7. Chemicals	15.44	18.36	17.31	12.08	15.51
8. Rubber and Rubber Products	11.90	13.17	21.71	22.36	17.25
9. Transport Equipment (Excluding Motor Vehicles)	36.91	20.80	25.59	38.16	31.39
10. Iron and Steel	10.00	16.47	27.87	39.14	23.54
11. Motor Vehicles and Parts	7.19	16.84	33.92	25.04	20.09
12. Nonferrous Metals	3.53	12.03	24.42	30.50	17.53
13. Food and Beverages	11.15	14.66	18.33	21.75	16.47

vincing. These industries tended to have the highest percentages of excess capacity, excluding food and beverages, in recent years (see Table 6). In fact for all 13 industries there is a rough rank correlation between the percentages of excess capacity and the satisfactoriness of the equations.

The absolute magnitude of the excess-capacity percentages shown in Table 6 for individual industries must be considered in the light of the fact that they are based on McGraw-Hill capacity indexes, and that for total manufacturing the percentages of excess capacity in recent years which are implied by the McGraw-Hill index are unreasonable, out of line with reported operating rates, and much lower than those implied by the de Leeuw index. The fact that the McGraw-Hill index rises at a much greater average rate than the de Leeuw index leads to very different patterns of implied excess-capacity percentages over time. For the early years the McGraw-Hill index implies much less excess capacity, for the later years much more.

food and beverages the simple correlation between excess capacity and total capacity was so high that meaningful results were impossible. Also in iron and steel and nonferrous metals, investment data on a company basis include important expenditures outside manufacturing establishments.

It may be suggested that the excess-capacity percentages in the various industries could be adjusted to a "de Leeuw basis," as the coefficients were. This procedure does not seem appropriate, although some rough adjustment may be necessary in the over-all magnitudes. The ranking of industries according to which excess-capacity percentages are the highest would not be affected by such an adjustment, of course. Furthermore, it is well known that very substantial excess capacity existed in at least two of the industries with the highest indicated percentages of excess capacity, iron and steel and motor vehicles.

Whatever the absolute size of the excess percentages in the various industries, the industries with poor results tended to show the highest percentages, on the average, in recent years.<sup>13</sup> These high levels of excess capacity might be the result of low sensitivity to excess capacity and to changes in output, as indicated by the equation coefficients. But it seems much more reasonable to believe that the insensitivities are due to the very high levels of excess capacity in recent years. Reactions to excess capacity may be small when output is near preferred operating rates, and the excess capacity is expected to be temporary. But when excess capacity is 20 or 30 per cent of total capacity for several years in succession, changes in the degree of excess capacity may have almost no effect, and the excess capacity may in fact lead to precisely the type of modernization and cost-reducing investment which cannot be explained by accelerator-type forces.

### VIII. *Conclusions*

1. Linear regression equations for total manufacturing satisfactorily explain most of investment since 1950. The current year's investment is explained by the previous year's excess capacity, total capacity at the beginning of the year (to allow for replacement investment), and the current year's change in output.

2. Equations based on 1950-61 data for total manufacturing and for 13 individual industries performed well in most cases as estimators of 1962 investment. Equations for total manufacturing did not perform so well in estimating 1963 investment.

<sup>13</sup> The average excess capacity for manufacturing as a whole is strongly affected by the five low-ranking industries. The excess-capacity calculations in Table 6 are based on output measures of capacity, and the five low-ranking industries accounted for roughly 30 per cent of manufacturing value-added in 1954, while the eight high-ranking industries accounted for 40 per cent, and uncovered industries for 30 per cent. The relative importance of the three groups of industries for investment expenditures was very different.

The precise meaning of the excess-capacity percentages is not clear, since the definitions of capacity are not clear or precise.

3. Equations for total manufacturing were fitted for 1950-61 and also for 1950-62, using both the McGraw-Hill and the de Leeuw indexes of manufacturing capacity. The de Leeuw equations are better fits and performed better as estimators. Equations relating capacity increases to investment led to much closer and more significant relations with the de Leeuw index than with the McGraw-Hill index.

4. Equations for total manufacturing investment and capacity were combined to form second-order difference equations in capacity and were used to analyze the effects of various output growth rates on investment and excess capacity. Both fluctuating and steady growth rates were assumed, and steady-state solutions were compared with the results of iteration examples starting from 1962 initial conditions. Fluctuating output growth rates seem to produce less investment and lower ratios of investment to output than the equivalent steady growth rate. Both the steady-state solutions and the iteration examples indicate that investment is highly sensitive to the assumed rate of growth of output.

5. Equations based on the McGraw-Hill index of capacity imply that investment is highly insensitive to the degree of excess capacity. Even at 4 or 5 per cent a year rates of growth of output, excess capacity would tend to increase and reach unrealistic levels. The equations based on the de Leeuw index give a very different picture in this respect. Steady growth rates of 4.5 or 5 per cent would tend to reduce excess capacity, compared to average 1959-62 levels, and the amounts of excess capacity implied are reasonable. The strong upward bias in the McGraw-Hill index accounts for the very different results.

6. The coefficients of the equations for total manufacturing and for the 13 industries appear reasonable when they are roughly adjusted to capital stock estimates of capacity, rather than the value-added output measures of capacity used in the original equations. The coefficients of all three terms are of reasonable orders of magnitude, except for two or three industries for which the equations are completely unsatisfactory.

7. Of the 13 industries tested, reasonably satisfactory equations were fitted in eight cases. These eight industries account for 56 per cent of manufacturing investment in 1954. The five industries for which the equations were unsatisfactory present certain statistical problems which may explain the results. However, a more general explanation seems highly plausible. These five industries have experienced the highest percentages of excess capacity since 1956. These are such high percentages, even when adjusted to correct for the upward bias in the McGraw-Hill indexes, that accelerator-type forces could hardly be expected to explain investment.

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## STEEL IMPORTS AND VERTICAL OLIGOPOLY POWER

By WALTER ADAMS AND JOEL B. DIRLAM\*

The so-called steel-import problem, largely a phenomenon of the 1950's, is an amalgam of several causes—the recovery from wartime destruction and dismantling of the European and Japanese steel industries; the installation of ultramodern capacity, reflecting latest technology, outside the United States; a substantial increase of world-wide capacity; a generally export-oriented price policy by European and Japanese producers; a generally insensitive, utility-like, administered price policy by domestic producers. In other words, the steel-import problem is the product of the comparative showing of the U.S. steel industry in an increasingly competitive world economy.

Some basic data on the performance of the domestic industry will make the point:

1. Between 1955 and 1962, U.S. production of steel ingots decreased from 117 million tons to 98 million tons. At the same time production in the rest of the Free World increased from 112 to 175 million tons, in the Communist bloc from 68 to 126 million tons, in the world as a whole from 297 to 399 million tons. The U.S. share of world steel production declined from 39.4 per cent in 1955 to 24.6 per cent in 1962—an unbroken decline in this country's relative share [16, p. 336].

2. The same deterioration is observable in this country's steel-export position—both in absolute terms and in comparison with the world's major steel-exporting countries. Between 1954-56 and 1961, the U.S. share of world export markets (outside the United States itself) declined from 20.0 to 10.1 per cent, while that of the Common Market increased from 53.9 to 62.5, that of the United Kingdom declined from 12.7 to 11.3, that of other European OECD nations increased from 7.2 to 9.9, and that of Japan remained stable at 6.2 per cent. This decline in the U.S. share would appear even greater if tonnage rather than value figures were used; and also it presumably would have been greater but for the artificial stimulus to U.S. steel

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exports by the "Buy American" strings attached to some of our foreign aid programs [16, pp. 490-91].

3. This loss of world export markets by the U.S. steel industry was accompanied by a historically notable invasion of the industry's domestic market by foreign producers. Whereas the United States was traditionally a net steel-exporter, its position was dramatically reversed starting in 1959 (a strike year). Since then, the United States has been a net steel-importer. Whereas in 1955 the United States exported four times as much steel tonnage as it imported, by 1962 it imported twice as much as it exported [16, p. 327].

4. In terms of capacity utilization, i.e., operating rate, the U.S. steel industry also compared unfavorably with the world's major steel-producing countries. The following data make the point [16, p. 339]:

PERCENTAGE UTILIZATION OF STEEL CAPACITY IN SELECTED AREAS, 1955-62

Area	1955	1956	1957	1958	1959	1960	1961	1962
United States	92.1	88.0	82.2	59.2	63.1	66.3	64.5	63.8
United Kingdom and ECSC	96.7	96.2	94.8	85.0	87.7	95.2	89.5	84.4
Japan	N.A.	N.A.	70.6	67.0	90.1	96.4	97.5	82.6

5. In terms of technological innovation, the U.S. industry also lagged behind its foreign competitors. During the 1950's, according to *Business Week* [7, pp. 144-46], "the industry bought 40 million tons of the wrong kind of capacity—the open hearth furnace," while the Europeans and Japanese were installing the cheaper and more productive oxygen converters at a breakneck pace. Ironically, much of the U.S. industry's current investment in plant and equipment is calculated not to add to capacity, but to correct the embarrassing investment errors of the last decade.

6. While foreign producers priced their steel in accordance with the fluctuating vicissitudes of the world market [16, pp. 29, 476, 482], U.S. producers followed an administered price system characterized by upward flexibility. Thus, according to the Bureau of Labor Statistics, the U.S. wholesale price index for iron and steel products increased 88 per cent between 1947 and 1958, while the wholesale index for all commodities (other than farm and food) increased only 32 per cent. From World War II to 1958, "the steel price index continued its virtually unbroken rise even when demand and production declined (as they did in 1949, 1954, and 1957). It also continued its climb even when unit labor costs declined (as they did in 1950 and 1955). No matter what the change in cost or in demand, steel prices since 1947 have moved steadily and regularly in only one direction,

upward" [17, p. 129]. The fact that prices were raised in 1957 and 1958, and subsequently maintained in the face of declining demand, operations at less than 60 per cent of capacity, and in the midst of a general recession; the fact that the industry again attempted to raise prices in April, 1962, with operations lagging at 50 per cent of capacity and the economy far from full employment—these are further indications of the perversity with which the U.S. steel industry reacted to the environmental influence of market conditions.<sup>1</sup>

Clearly, no internal domestic forces have been sufficiently powerful to ameliorate the questionable performance of the U.S. steel industry. Not the discipline of market competition, nor the pressure of public opinion, nor the corporate soul of industry executives, nor the castigations of the Kefauver Committee have succeeded in promoting more competitive behavior. Repeatedly the industry has been urged to reduce prices in order to increase sales, recapture foreign business, stem the import tide, reduce the threat of competing materials, operate at higher levels of capacity, and thus cut unit costs [18, pp. 27-87]. But the industry persisted in its belief that the demand for steel was inelastic and behaved as if this inelasticity applied even in markets where steel was confronted by competition from substitutes and imports.<sup>2</sup>

The industry's behavior in the face of substantial imports seemed to belie the Mancunian assumption among policy makers that "free international trade is the best antimonopoly policy and the best guarantee for the maintenance of a healthy degree of free competition."<sup>3</sup> It is important, therefore, to determine why the ostensible discipline of imports proved to be ineffective.

It is our contention that a vertically integrated oligopoly must maintain its administered price structure with all the economic and political weapons at its disposal. It must defend *à outrance* the vertical succession of keystone prices on which the entire price system rests. It must, therefore, contain, insulate, neutralize, and sterilize imports, especially

<sup>1</sup> This is not to gainsay the fact that transaction prices occasionally deviate from quoted prices, that there is some off-list selling, and that there are some temporary (though short-lived) dips in the steel index. The trend of the steel price structure, however, is unmistakably and unidirectionally upward.

<sup>2</sup> Indeed, during the latest round of "selective" price increases, its spokesmen felt that these increases would "spur demand, instead of retarding it." See "Price Hikes May Spur Demand," *Steel*, Oct. 7, 1963.

<sup>3</sup> Gottfried Haberler, quoted in [12, p. 34]. However, on the basis of a multiple regression analysis of the effect of quantity and price of imported steel products on domestic prices between 1954 and 1958, Krause concluded that "the foreign trade variables were not very important in determining changes in domestic steel prices" [12, p. 41]. Unfortunately, Krause did not consider vertical integration as a conditioning element in the industry's price response to imports.

when they threaten the vertically interwoven strategic prices. It cannot allow imports to have more than a sporadic, peripheral, nonstructural impact. Conversely, if the entire noncompetitive price structure is to be eroded, this must be done through an attack on strategic prices. Only if imports are pinpointed at the focus of vertical price policy (i.e., strategic prices) can they trigger price competition in the unconcentrated segments of the industry. Once such price competition breaks out among the independents, the vertically integrated oligopolists are compelled to meet it—at the cost of sacrificing at least a portion of the administered price structure and the rigidities associated therewith. Whether imports succeed in collapsing the entire structure depends upon the quantitative importance of the imported products, their place in the vertical hierarchy, and the staying power of the independents under a regime of dual distribution.

We propose to demonstrate the validity of this hypothesis by an examination of the wire segment of the steel industry and its response to import competition between 1955 and 1962. This examination will treat (1) the extent of imports, (2) the industry's price response, (3) an explanation for the industry's differential response to raw-material and finished-product imports, (4) the industry's nonprice response, and (5) a final assessment of the import discipline hypothesis.

### *I. The Extent, Trend, and Pattern of Imports*

Steel imports into the United States since 1955 have taken an ever-growing share of the domestic market—rising from 1.2 per cent of the total supply in 1955 to 5.6 per cent in 1962. Among the selected groups of steel products shown in Table 1, wire rods are by far the most important semifinished import. In 1962, for example, the import tonnage of wire rods exceeded that of ingots, blooms, slabs, and billets by a margin of roughly 4 to 1. Other large tonnage imports include pipe and tubing, structural shapes, concrete reinforcing bars, and sheet and strip. With the exception of reinforcing bars, however, none of these imports has ever accounted for more than 15 per cent of domestic consumption. Wire rods and some wire products, on the other hand, have captured substantial shares of domestic markets. It is here, first of all, that we should attempt to measure the impact of steel imports.

#### *Wire Rods*

Imported wire rods have been significant for two reasons. First, as a semifinished material imported in quantity, they threatened to disturb the vertical structure of the U.S. steel industry at midpoint, where shifts of capacity to alternative products were difficult. Second, they

TABLE 1—IMPORTS OF SELECTED STEEL PRODUCTS, 1955-62  
(000's of net tons)

Year	Ingot, Blooms, Billets, Slabs, etc.	Shapes, Plates & Piling	Reinforcing Bars	Pipe and Tubing	Sheets and Strip	Wire Rods	Wire & Wire Products	Total Steel Products
1955	146.1	111.8	158.9	77.1	54.7	47.8	245.3	973.2
1956	28.2	399.5	173.3	140.4	55.3	64.1	247.4	1,340.7
1957	8.1	290.6	160.4	190.8	41.2	54.4	301.1	1,154.8
1958	18.1	171.0	473.0	200.0	50.0	181.3	432.2	1,707.1
1959	91.9	798.1	851.9	553.1	385.9	447.9	703.5	4,396.4
1960	68.5	529.1	515.5	480.0	435.6	408.2	547.3	3,358.8
1961	179.5	330.4	582.8	521.3	171.0	451.2	561.8	3,163.2
1962	174.4	524.6	607.3	654.7	383.7	644.6	655.2	4,100.4

Source: AISI, *Foreign Trade Trends, Iron & Steel*, 1963 edition.

captured a large share of the noncaptive U.S. wire-rod market—that is, rods consumed by firms purchasing, but not producing, rods. The import share of this noncaptive market had reached 17.1 per cent as early as 1958, 39.2 per cent in 1962, and promised to exceed 40 per cent in 1963.

When imports are compared with the total U.S. production of wire rods, their significance shrinks. In 1962, imports amounted to no more than 14.5 per cent of total wire-rod output. This low percentage compared with the share of marketed wire rods reflects the structure of the industry; over the years the vast majority of wire rods have been further fabricated by the firms that produce them, as shown by Table 2. Imports have loomed large only when compared with the noncaptive market.

Wire-rod imports have a third distinguishing characteristic. During the years 1957-62, they increased faster than those of any other steel product. As shown by Table 2, the tremendous acceleration in imports was not accompanied by a parallel growth in U.S. output of wire rods. It seems reasonable to conclude, therefore, that the imports appropriated what would otherwise have been a 400-500 thousand-ton growth in the market for domestically produced wire rods, and that the U.S. markets captured by imports were those belonging to the domestic integrated mills, the giants of the steel industry.

### *Wire Products*

The imports of the most important products of the wire branch of the steel industry, together with their proportion of estimated domestic consumption, from 1955 through 1962, are shown in Table 3. The

TABLE 2—PRODUCTION, CONSUMPTION, AND IMPORTS OF WIRE RODS, 1955-62  
(Thousands of net tons)

Year	Production	Apparent Noncaptive Consumption*	Imports	Imports+ Production (per cent)	Imports+ Noncaptive Consumption (per cent)
1955	6,098.3		47.8		
1956	5,608.9		64.1		
1957	4,695.6	1,002.6	54.4	1.16	5.4
1958	4,342.0	1,059.5	181.3	4.18	17.1
1959	4,628.1	1,424.0	447.9	9.68	31.5
1960	4,265.3	1,316.2	408.2	9.57	31.0
1961	4,415.9	1,380.9	451.2	10.22	32.7
1962	4,435.2	1,644.8	644.6	14.53	39.2

\* Apparent noncaptive consumption equals industry net shipments plus imports minus exports. It is a rough index of the supply to noncaptive buyers.

Source: AISI, *Foreign Trade Trends, Iron & Steel*, 1963 edition.

TABLE 3—IMPORTS AND CONSUMPTION OF WIRE AND SELECTED WIRE PRODUCTS, 1955-62  
(000's of net tons)

Year	Drawn Wire		Nails and Staples		Barbed Wire		Woven Wire Fence	
	Imports	Per cent of Domestic Consumption	Imports	Per cent of Domestic Consumption	Imports	Per cent of Domestic Consumption	Imports	Per cent of Domestic Consumption
1955	48.0	1.5	130.6	16.8	60.1	35.0	13.1	4.1
1956	62.6	2.1	111.1	16.7	62.3	46.0	21.9	7.5
1957	72.9	2.7	134.9	23.4	63.1	52.2	30.2	13.1
1958	135.9	5.4	197.2	32.3	59.3	51.9	39.8	20.5
1959	241.3	8.0	304.8	44.0	78.3	61.9	79.0	36.1
1960	210.6	7.8	231.8	42.3	52.9	52.8	51.9	33.6
1961	174.4	6.6	245.1	42.8	82.3	53.0	59.9	32.9
1962	244.3	8.7	271.3	46.1	66.6	47.7	72.9	38.9

Source: AISI, *Foreign Trade Trends, Iron and Steel*, 1959 and 1963 editions.

table brings out the relative importance (in tonnage terms) of imports of wire and nails, with other wire-product imports such as barbed wire and wire fencing running far behind. When the proportions of imports to domestic consumption are considered, however, barbed wire and wire fencing are high on the list, while wire itself<sup>4</sup> in 1962 amounted to no more than 9 per cent of domestic consumption. Nevertheless, to the extent that wire is an intermediate product in further fabrication (since it may be galvanized and be sold as fencing wire, or woven into woven wire fence, or made into chain link fence), even the relatively low volume of wire imports has disturbed integrated or partially integrated firms selling wire on the domestic market.

### *Conclusion*

The products representative of every stage of the wire division of the steel industry have been increasing their absolute import tonnage into the United States since 1955, but at differential rates. For our purposes, it is sufficient to note that in 1955 wire rods constituted only about 16 per cent of all wire imports into the United States, whereas by 1962 this percentage had climbed to almost 50 per cent. Whether or not this phenomenon can be explained in terms of the industry's differential price response is discussed in the following section.

### *II. The Price Response to Imports*

The response of the U.S. steel market to wire-rod and wire-products imports is illustrated in Table 4 and Chart 1. Table 4 presents BLS price indices for wire rods, drawn carbon wire, welded wire fabric, bale ties, chain link fence, woven wire fence, nails and barbed wire, with 1955 as the base year.

The pattern of response makes an interesting contrast with the trend of imports. The price of wire rods moved up in each year from 1955 through 1959 and remained unchanged through 1962. Carbon wire prices followed the same time sequence of changes, but the 1959 high did not represent such a steep rise from 1955 as the wire-rod high. Prices of many products, by contrast, seem to have reached a peak about 1958 and to have declined thereafter. Without further discussing the details of price movements, it is clear that the raw material—wire rod—and the semifinished material—wire—both rose in price more than fabricated products and were later maintained in price, while the fabricated products were not. This has led to widespread allegations by nonintegrated fabricators that their integrated

<sup>4</sup> "Wire" includes many varieties. Unfortunately, Census statistics do not show separate shipments or consumption of multifarious types of wire used in different end products.

TABLE 4—WHOLESALE PRICE INDEX FOR SELECTED STEEL PRODUCTS, 1955-1962  
(1955=100)

	Semi-finished Steel Products (10-13)	Billets, Rerolling, Carbon (10-13-01)	Wire Rods (10-13-11)	Finished Steel Products (10-14)	Drawn Wire, Carbon (10-14-76)	Bale Ties, Carbon (10-14-82)	Nails, Wire & Common (10-14-86)	Barbed Wire, Galvan. (10-14-91)	Woven Wire Fence, Galvan. (10-14-96)	Welded Wire Fabric (10-82-51)*	Chain Link Fence (10-82-61)
1955	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00
56	107.00	106.00	111.80	108.40	106.50	110.30	108.80	108.00	109.40	n.a.	106.60
57	118.50	118.50	122.60	118.70	119.70	123.90	117.10	114.30	119.10	126.20	116.20
58	122.90	122.70	128.90	122.90	125.10	129.20	119.90	116.50	122.50	130.60	121.60
59	124.80	124.60	131.60	124.90	127.50	129.20	119.90	116.70	122.60	130.60	121.60
60	124.50	124.60	131.60	124.70	127.50	126.20	117.10	114.40	120.00	130.60	121.60
61	124.20	124.60	131.60	124.30	127.50	124.00	115.10	111.90	117.60	130.60	121.60
62	124.20	124.60	131.60	123.90	127.50	124.00	115.10	112.00	117.70	125.50	121.60

\* January 1955=100.

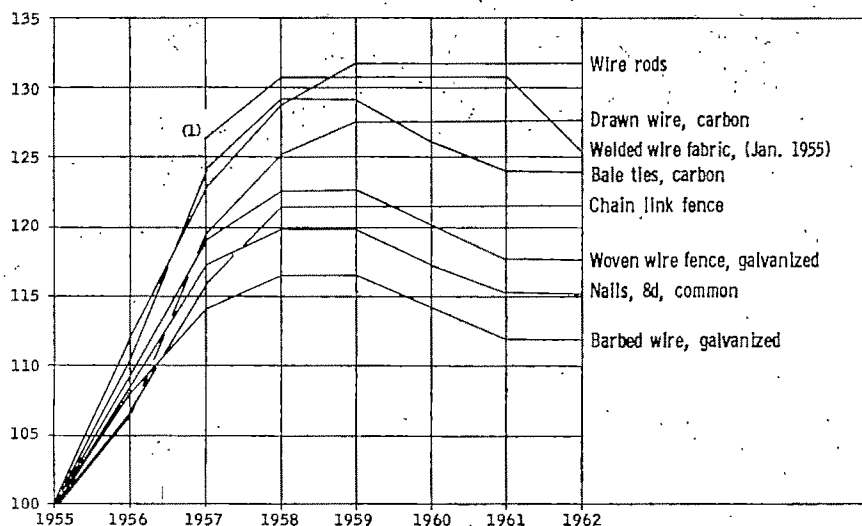
Source: Recomputed from Bureau of Labor Statistics, *Wholesale Prices and Price Indexes*, using 1955 as a base.

Notes: The price of welded wire fabric was not included among the BLS sample of wholesale commodities until 1957, although the index was computed on a January 1955 base. In the years 1960-62, published prices have not been representative; a large proportion of sales has been made on a negotiated basis, and at prices lower than indicated by the BLS index.

Although chain-link-fence prices appear to be accurate for sales by mills to jobbers, prices charged by nonintegrated mills directly to consumers (and these account for most of their sales) have often been less than 50 per cent of the prices used in the BLS index for the years 1961-62.

It should be noted that, with one exception, BLS respondents are all integrated mills making wire rods.

CHART 1. WHOLESALE PRICE INDEX FOR SELECTED STEEL PRODUCTS, 1955-62  
(1955 = 100)



<sup>1</sup> January 1955 = 100.

Source: Recomputed from Bureau of Labor Statistics, *Wholesale Prices and Price Indexes*, using 1955 as base.

Notes: Same as notes to Table 4.

rivals were applying both a simple and (after 1959) a double price squeeze [19, pp. 224-373].

### Wire Rods

There are two features of wire-rod price behavior that stand out most sharply against the dramatic increase in imports. First, through 1962, the price of wire rods rose *more* than the price of any other steel product surveyed by the Bureau of Labor Statistics in the post-war period [16, p. 234]. It was the most "upward mobile" steel price in the industry's administered price complex. This in itself is curious, not only because half the postwar rise took place in 1955-62, when imports were increasing, but because there was a persistent unutilized capacity in wire rods from 1956 on—an excess that came close to 40 per cent. The second feature is the price inflexibility, 1958-62, when imports of wire rods were rising at an average annual rate of over 60 per cent.

There are exceptions to the above generalizations, but they are so slight as to underscore the extraordinary behavior of wire-rod prices. One firm, Colorado Fuel and Iron, with an unusually large part of its investment in wire-rod capacity, attempted (apparently in 1958 and

TABLE 5—IMPORT AND DOMESTIC PRICES FOR WIRE RODS  
1958-62<sup>a</sup>  
(Dollars per net ton)

	Ports of Entry	Domestic Basing Points			
	North Atlantic	Sparrows Point	Margin	Worcester	Margin
1958	125.22	141.58	16.36	145.58	20.36
1959	116.02	144.50	28.48	148.50	32.48
1960	127.80	144.50	16.70	148.50	20.70
1961	115.40	144.50	29.10	148.50	33.10
1962	108.00 <sup>b</sup>	144.50	36.50	148.50	40.50
	Gulf Coast	Houston	Margin	Kansas City	Margin
1958	124.97	144.58	19.61	144.58	19.61
1959	113.93	147.50	33.57	147.50	33.57
1960	124.60	147.50	22.90	147.50	22.90
1961	114.40	147.50	33.10	147.50	33.10
1962	107.00 <sup>b</sup>	147.50	40.50	147.50	40.50

Source: *Steel* magazine.

<sup>a</sup> Imports from Western Continental Europe; imported wire rods are Thomas quality. Imported open-hearth wire rods are usually quoted \$4-10 above the Thomas-quality rods.

<sup>b</sup> *Steel* magazine quotes the price for January and February. Price is quoted as "nominal" from March through August. The series is discontinued after August, 1962.

1959) to make concessions to retain wire-rod customers. It soon abandoned such attempts.<sup>6</sup> Also, there is reason to believe that a few other integrated firms have made sporadic concessions on wire rod, but their quantitative significance was *de minimis*.<sup>7</sup>

Over a five-year period, when domestic prices were perfectly rigid at \$144.50 per ton, comparable foreign wire rod was being sold at \$110, \$105, or, at times, even less than \$100 per ton.<sup>7</sup> Table 5 shows

<sup>6</sup> According to its vice-president in charge of sales, "When C.F.&I. found that its wire rod customers were changing to foreign rods because of the lower price, we attempted to be at least partially competitive with these lower prices. The extent of our competitive price varied, depending on the competitive necessity in each particular case. Although our competitive prices were partially successful, in many cases we lost customers to foreign rods because they were purchasing at prices lower than those which we could meet" [20, CF&I, p. 6]. The stated opinion of the other integrated steel companies was that any reduction they might make in rod prices would be followed by their foreign competitors, and hence prove self-defeating [20, Armco, p. 6; Youngstown, p. 6; J & L., p. 9; Pittsburgh, p. 6]. The companies also justified their refusal to meet import prices on the grounds that the latter represented sales at less than fair value and hence were discriminatory [20, Bethlehem, pp. 7-8].

<sup>7</sup> Members of the industry claim that even though f.o.b. mill prices on wire rods were maintained, there was a decline in mill net prices because of an increase in freight absorption and a "reaching out" into distant territories to offset the import-induced loss of customers in nearby ("natural") market territories.

<sup>8</sup> In the spring of 1963, independent fabricators bought imported wire rods, Thomas quality, for \$93 per ton, and open-hearth rods for \$100-102 per ton [19, pp. 250, 351].

the margins between import and domestic prices for wire rod at representative ports. The evidence is incontrovertible that the domestic mills sold wire rod continuously at differentials sometimes exceeding 40 per cent above the price of imported rods. Indeed, steel-mill spokesmen were quick to boast of their unwillingness to meet foreign competition. There was, in short, no domestic price response to wire-rod imports.

### *Wire*

The rigidity in the price of wire, after a rapid increase from 1955 to 1958, is brought out in Table 4. The integrated steel firms pointed with pride to the maintenance of wire prices in the face of imports. Users of wire, they insisted, could count on the integrated firms preserving a fair spread between manufacturers' bright wire and the more expensive varieties [20, J & L, pp. 9-10].

At the same time that the large integrated wire producers (who accounted for well over half of the wire shipments) were maintaining prices, imports of wire were available at margins substantially below the domestic price. In 1963, at North Atlantic ports, manufacturers' bright wire sold at about \$20 a ton below the domestic product, while imported galvanized fencing wire was sold at an even greater differential.

Until recently, the failure of the domestic integrated firms to respond to the lower price of imports did not seriously affect their volume of wire sales, although it did contribute to the erosion of their noncaptive market for wire rod. In the market for wire, it seems, there have been a number of nonprice considerations that led potential customers of imported wire to prefer domestic sources even at substantial premiums.<sup>8</sup>

The rigidity of wire prices, therefore, may be tentatively ascribed to two aspects of the U.S. market: the dominance of the vertically integrated firms, who avowedly wished to maintain differentials and levels in the wire market, and the relatively small impact on U.S. sales of low-priced wire imports.

### *Wire Products*

Prices of representative wire products as shown in Chart 1, and Table 4, have fallen since 1958 and 1959, compared with the price of

<sup>8</sup> Some wires, like plating wire and certain varieties of bright wire, are not readily imported because they must be free from surface blemishes due to oxidation. Other wires are shipped on huge carrier spools, and hence do not lend themselves to shipment from abroad. Moreover, many wire purchasers buy in small quantities, often as little as 50-100 lbs. Such customers cannot easily or accurately forecast their needs months ahead of delivery, and warehouses are unwilling to stock vast varieties of specialty wire to accommodate these "retail" purchasers.

wire rod and wire, producing the "double squeeze" that has accelerated the fabricators' move toward imported rod.

Here again, as in the case of rods, the movement of domestic-products prices cannot be correlated with the import pattern and, indeed, shows signs of perversity. Barbed wire and nail prices declined, but it is far from evident that the drops should have come when they did, since imports had been a substantial factor as early as 1955. By the time these price declines became manifest, several integrated firms had permanently abandoned sizable segments of the barbed wire and nails markets, and the capacity devoted to it. Also, the price decline in chain link fence and welded wire fabric could hardly have been caused by imports, since neither product came from abroad in significant commercial quantities. On the other hand, the import of bale ties and automatic baling wire may have had an impact on the domestic price level, because these imports accounted for roughly 22 per cent of total U.S. consumption in 1962.

Summing up, there was no correlation between the proportion of imports to domestic open-market consumption and the degree of price response. Wire-rod and wire prices moved perversely to rising imports. Chain link fence and welded wire fabric prices tumbled, though imports were inconsequential. The apparent paradox, we submit, can be explained only by the structure of the industry—a subject to which we now turn.

### III. *Explanation of the Price Response*

#### *Vertical Integration and Prices*

In order to understand the pattern of response of steel prices to imports—or, perhaps more aptly, to understand that there is a pattern of response in what seems to be inconsistency—it is necessary to begin with an analysis of steel pricing and its principal determinants. As is only natural, the structure of the steel industry plays a large part in shaping its price behavior.

First, steel is an oligopoly in the horizontal sense. A relatively small number of firms sell the most important semifinished and finished products. Four firms, for example, produce 71 per cent of steel ingots, 56 per cent of sheet and strip, 64 per cent of hot rolled bars, 65 per cent of pipe and tubes, etc. In this oligopoly, the accent is on price uniformity and price stability. Cost movements and target returns—more so than demand—are prime determinants of price policy, perhaps because of the industry's expressed belief that the market for steel is price-inelastic [10, pp. 13-24].

The steel industry is, of course, vertically integrated as well. Its oligopolistic structure strongly influences the vertical relationship of

prices. If integration were complete, market prices of raw and semi-finished materials would disappear, but because at every stage of production there are incompletely ("disproportionately") integrated firms that purchase products for further fabrication or conversion, these intermediate prices are present in the steel-pricing structure. Pricing policy of the steel industry, and of its price leaders, includes, therefore, not only the achievement of goals set for individual prices, but also maintenance of the proportions, ratios, and margins between prices at successive levels of production that seem to be in the best interests of the industry.

Some of the prices in the vertical steel price structure are of more importance than others. These are what we have earlier called strategic prices. They are the keystone on which a portion of the price edifice rests. If they change, it will be necessary to shift the entire structure. Like the Gulf Coast or Group 3 spot prices for gasoline, they serve as a "peg to hang the price structure on."<sup>9</sup> Thus the price for wire rods is strategic because it is the base on which the super-structure of wire and wire-products prices are built.

If the steel oligopoly lived in a hermetically sealed world, it could maintain what it regards as the appropriate relations among prices in the vertical sequences with little difficulty. But no such world exists. The oligopoly has to permit the price structure to respond to outside stimuli, which constantly threaten it at various points. Some of the exogenous forces can be ignored as temporary aberrations. Others may be "contained" by minor adjustments. When a strategic price is threatened, however, the threat must be dealt with at almost any cost, not simply because of the necessity for re-evaluating the specific price, but because of the danger to the market structure dependent upon it.

The vertically organized giants in the steel industry, therefore, seldom consider a price in isolation. A change in any price introduces uncertainties up and down the line. If a product price is reduced, sales of semifinished steel may fall off because fabricators' margins have been compressed. The same consequence can result from

<sup>9</sup> U.S. vs. Socony-Vacuum Oil Co., 310 U.S. 150 (1940), p. 192. See also [8, pp. 386-427]. Vertically integrated steel companies, like the major oil companies, are partially dependent on the sale of an intermediate product to customers with whom they compete. The integrated oil companies dispose of unbranded gasoline to independent distributors in order to run their refineries at close-to-capacity levels. They tolerate "conventional" price differentials between their own and the independent gasolines, but they are quick to support retaliatory action whenever an independent attempts to enlarge his tacitly fixed market share through price cutting. Neither in oil nor steel, can nonintegrated customers be allowed to get out of hand. Otherwise the price level of final products may be reduced, with no increase in sales, and with the burden falling on the integrated companies at the intermediate product stage.

an increase in prices of semifinished materials while product prices remain unchanged. The vertically integrated firms must consider these aspects of policy because they sell varying proportions of their products to nonintegrated firms at different stages of production. Their own customers may compete directly or indirectly with them on the finished product. Yet, for a variety of reasons associated with the individuality of customers and vagaries of location and technology, it does not ordinarily pay to eliminate all outside sales. For all these reasons, a decision to meet or not meet the lower price of a semifinished steel product cannot be made by integrated giants simply on the facts about direct costs and extent of unutilized capacity for that product alone.

The nonintegrated fabricator, on the other hand, lives in an entirely different world. He is aware of the vertical structure of prices only insofar as it affects his margin. When the margin is disturbed—increasing or decreasing—he will respond. But in normal times he lives within the margins permitted by the structure and cooperates with its preservation by not undercutting the prices of fabricated products published by his suppliers of raw or semifinished materials.

The small firms usually refrain from endangering the price structure because they, too, are instilled with the “standard cost—target return” philosophy and accept the margin that is left to them. They understand the power and influence of the firms shaping the price structure. Nevertheless, their shorter investment and price policy time-horizon, and their lack of sensitivity to the vertical price relationships gives them a sense of irresponsibility. It makes them a source of potential danger to the structure.

### *Wire-Rod Pricing*

The pricing of wire rods, wire, and wire products illustrates the imperatives of vertical integration—the manner in which industry structure tends to determine price behavior. At the top of the “wire rod-wire-wire products” pyramid are 15 integrated producers. Of these, 12 firms, including such industry giants as U.S. Steel, Bethlehem, and Republic, account for 93 per cent of rod capacity. Without imports, their control over the wire-rod stage of the industry seems secure; entry is effectively deterred by the cost of erecting a wire-rod plant (\$20 million) [19, p. 67] and the persistence of excess capacity almost equal to U.S. production of wire rods. But, as Table 6 shows, the integrated firms also have very substantial capacity in wire products. Indeed, they customarily dispose only of some 20 per cent of their wire-rod output on the open market, the rest going into their fabrication plants [19, pp. 99-107]. Put differently, the principal con-

TABLE 6—CAPACITY AND PRODUCTION, WIRE ROD, WIRE, AND SELECTED WIRE PRODUCTS

1957-1960  
(in net tons)

Products	1957			1960		
	Capacity	Production	Production ÷ Capacity	Capacity	Production	Production ÷ Capacity
Wire rod	7,374,850	4,695,595	63.7%	7,340,480	4,265,289	58.1%
Plain wire	7,235,160	3,680,871	50.9%	7,256,320	3,263,110	45.0%
Galvanized wire	1,771,740	376,204	21.2%	1,613,890	348,658	21.6%
Nails and staples	1,263,480	481,427	38.1%	1,155,960	368,914	31.9%
Barbed wire	576,590	58,793	10.2%	388,790	48,613	12.5%
Woven wire fence	985,650	210,965	21.4%	835,570	115,033	13.8%
Reinforcing fabric	578,420	N.A.	—	1,115,760	N.A.	—
Bale ties	137,860	22,062	16.0%	123,050	14,820	12.0%

Sources: Capacity: American Iron and Steel Institute, *Directory of Iron and Steel Works of the U.S. and Canada*, 1957 and 1960 editions.  
 Production: American Iron and Steel Institute, *Annual Statistical Report*, 1957 and 1960. For galvanized wire, production as shown in Bureau of the Census, *Facts For Industry M 335*.

cern of the integrated firms is not with wire rods, but with the whole wire segment of the steel industry. They cannot afford to think of any one price in isolation—at the peril of disturbing the price structure and introducing uncertainties.

At the next stage of the industry, some 150 firms in 1958—and probably twice that number in 1962—purchase wire rod and draw it into wire. These firms sell wire and, in many cases, also engage in further fabrication of wire products. They account for less than half of the open-market sales of wire, and hence tend to disregard the impact of their behavior on the industry's vertical price structure. Entry into this phase of production seems to be relatively easy as the backward integration of many wire-products producers between 1955-62 tends to indicate. In no case, however, did this backward integration reach as far as the wire-rod phase of the production process.

Finally, there are the fabricators who buy wire and who make hundreds of wire products sold to industry and the ultimate consumer. These firms number perhaps 1,200-1,300—and it seems likely that the Census underestimates the total. These wholly nonintegrated fabricators—manufacturers of such items as upholstery springs, paper clips, safety pins, and coat hangers—are totally unconcerned with the industry's vertical price structure, except to the extent that this structure implies an expansion or compression of the margin between the price of the wire they buy and the price of the products they sell. This segment of the industry is markedly unconcentrated.<sup>10</sup> Entry is easy, and price competition is not foreclosed by oligopoly dominance. Lower raw-material costs trigger price reductions on individual transactions, and such reductions eventually bring about a lowering of the whole price level. Unless the integrated companies, therefore, can control strategic prices farther back in the production process, they cannot effectively meet competition either for disciplinary or defensive purposes; that is, they cannot prevent cost reductions from being translated into price reductions.

Given this framework, we can better appraise the price response of the "wire rod-wire-wire products" segment of the steel industry to the threat of imports. Beginning in 1955, two phenomena made their appearance: a supply squeeze and a price squeeze. Many of the integrated firms making wire rods failed to take care of their nonintegrated customers in this period of intense demand for steel. Some

<sup>10</sup> This is not to suggest that all fabricated-products markets approach perfect competition in terms of numbers of firms. Thus, some 14 fabricators account for the bulk of independently produced reinforcing mesh [22, p. 1]. Independent chain link fence manufacturers number approximately 20, but the market for this product is restricted by heavy transportation costs [19, p. 224]. On the other hand, there appear to be many more firms making other fabricated items.

small wire-drawers, therefore, looked around for a source of rods that would not disappear just when business was good and began turning to imported rods. Coincidentally, as part of successive, across-the-board, and industry-wide price increases, the big steel firms raised wire-rod prices in 1955, 1956, 1957, and 1958. Yet the price of wire and wire products, all dependent upon wire rod as a raw material, did not rise proportionately, uncomfortably reducing the percentage margin of the nonintegrated firms—at a time when wages and other costs were rising.<sup>11</sup> This, too, provided a motive for shifting to foreign wire rods.

The price response to imports differed in different stages of the wire segment of the industry, as well as among different wire products. First, with respect to wire rods, there was no price response. This was the strategic price which the integrated majors attempted to protect almost at any cost.

Second, with respect to wire products not imported in significant quantities (e.g., chain link fence and welded wire fabric), the independent fabricators were suffering from a price squeeze resulting from the disproportionate rise of domestic wire-rod prices. Given the lower price of imported rods, these fabricators turned to imported rods as a solution to both their supply and margin problems. As time went on, more and more fabricators did so. The accompanying reduction in cost led inevitably—given the lack of concern of the nonintegrated firms for the integrity of the price structure—to price shading on those products that the most competitive sector of the industry found to be price-elastic. Even though these items were not imported in appreciable quantities, the competitive drive among the fabricators sufficed to bring down the domestic price. The major integrated firms found it advisable to meet these lower prices, hoping to exercise some restraint on the smaller firms by so doing and, perhaps, to discourage them from embarking on such activities in the future. If the small fabricators realized that their integrated competitors were going to meet, on the nose, whatever prices were charged, there would be an incentive to limit cuts and to try to maintain a reasonable margin above the price of imported rods. Such seems to have been the case with chain link fence and welded wire fabric.

<sup>11</sup> Where customers were located at a considerable distance from wire-rod basing points, the delivered price of rods often exceeded the price of the fabricated product. In Miami, for example, U.S. Steel quoted wire rods at \$156 per ton, but sold the reinforcing mesh made from such rod at \$150 per ton [19, pp. 265-66]. For other evidence on the compression of the independent-fabricators' margin, including zero and negative margins, see *The Impact Upon Small Business of Dual Distribution and Related Vertical Integration*, Hearings before Subcommittee No. 4, House Small Business Committee, 88th Cong., 1st Sess., 1963, Vol. 2, pp. 438-39.

Third, with respect to those products imported in large quantities but whose domestic production is dominated by the integrated majors, there seems to have been little significant price response. In barbed wire and nails, for example, the giants did not meet foreign competition, thus abandoning a substantial market share to foreigners while maintaining the conventional margin between wire prices, on the one hand, and nail and barbed wire prices, on the other.<sup>12</sup> The independent fabricators, in turn, seem to have been content to enjoy the higher margin provided by the domestic-products price and to sell in the relatively protected geographical areas where nail and barbed wire imports cannot be sold at substantial differentials below the domestic price. Such price declines as the BLS reports in nails and barbed wire were not reflected in the published prices of the major integrated companies.

Fourth, where products were imported in such amounts as to threaten the independents (woven wire fence, automatic baling wire, and bale ties) there was a pronounced price response. The independents felt impelled to meet foreign competition, and the vertically integrated majors matched these price reductions, both to retain customers and to prevent the independents from cutting even further (and thus threatening the price structure). They claimed they did not cut to meet foreign competition because they believed this was an ineffective check on competitive inroads from abroad; the Belgians and Japanese would continue to engage in price competition with each other in the U.S. market—regardless of the price policy of domestic producers [20, Pittsburgh, p. 3; Republic, p. 7].

In general, then, in those segments of the industry where there was a competitive structure, that is, where nonintegrated firms were willing to pass on savings from foreign wire rods, prices tended to fall. But what of wire itself? Wire—or more properly wires—were never imported in substantial enough quantities to capture a significant segment of the domestic noncaptive market. Moreover, the backward integration of theretofore nonintegrated fabricators into wire drawing that occurred during the years 1958-63 did not, for the most part, lead to domestic price cutting in wire because these new entrants engaged in further fabrication; they became purchasers of imported wire rod mainly to protect themselves against the squeeze between higher wire-rod prices and lower prices for such items as chain link

<sup>12</sup> Several vertically integrated steel mills abandoned the production of products which faced import competition. Bethlehem discontinued making nails, fence, and barbed wire at its Johnstown plant [20, Bethlehem, p. 7]. Pittsburgh Steel discontinued the making of barbed wire, nails, and fencing in 1959 [20, Pittsburgh, p. 5]. In 1961, Detroit Steel withdrew from the production of building mesh, except for a few customers near its Portsmouth, Ohio, plant [20, Detroit, p. 7].

and woven wire fence. Finally, the integrated companies explicitly assumed responsibility for supporting the price structure in wire [20, Bethlehem, p. 10; J & L, p. 9]. One variety of wire—automatic baling wire—was imported in sufficient quantities at lower prices to bring a direct response from the independents and hence, the industry as a whole, as shown by the reductions in published prices in *Steel* magazine. But this was clearly an exception to the general upward flexibility of wire prices.

Why, then, did the vertically integrated major firms decide to change their own price structure—including the margins between rod and wire—by insisting on a disproportionate upward movement in rod prices during the years 1955-58, inclusive?

There are several alternative hypotheses that could shed light on the behavior of the wire-rod makers. First, and this is the hypothesis advanced by some fabricators, the vertically integrated firms were simply inept. They pursued a public-utility, cost-plus pricing policy in the conviction that price has nothing to do with sales. Thus, they tried to saddle the nonintegrated fabricators with the sunk costs of excess and antiquated wire-rod capacity. Moreover, they never squarely faced the supply problems of the nonintegrated fabricators and never fully understood the abject dependence of these fabricators on a survival margin between rod and product prices. There is an element of truth in this view.

A second hypothesis is that the squeeze—particularly the delicate but excruciating additional twist of the maintenance of rod prices between 1959 and 1962—was deliberate, and designed to rid the fabricating end of the industry of price competition by independents. This explanation cannot be wholly discarded.<sup>13</sup>

A third, and probably the most valid, hypothesis is based on the structural implications of vertical integration. Given the tapered integration and dual distribution in the wire segment of the steel industry, the majors had to compete with their nonintegrated customers. Before reducing rod prices, therefore, they had to consider not only the implications for rod revenues, but also the impact of this "cost" reduction on the independent fabricators and the indirect impact on the level of product prices. As long as the independent fabricators continued to buy domestic rod—and most of them used a 50/50 mix of imported and domestic rods in order not to cut themselves off from domestic supply sources—the majors felt that price cutting in the

<sup>13</sup> There are intimations, for example, that the integrated majors have initiated the proposal to reduce the tariff on welded wire fabric in order to tighten the squeeze on the independents [22]. The integrated companies were also complainants of record in the dumping charges before the Tariff Commission on wire rod and pipe and tube imports.

products markets would not get out of hand. On the other hand, if the majors had matched the lower price of imported rods, this would have lowered costs for the nonintegrated fabricators and served as a *carte blanche* invitation to cut product prices further than they had already been cut. In short, if the integrated steel companies were at all rational, we must presume that they concluded during the 1957-62 period that their losses from cuts in wire-rod prices, including the effect on products prices, would exceed the losses they suffered from relinquishing part of the noncaptive wire-rod market to imports. And this had to be a decision in which vertical considerations were paramount.

#### IV. *The Nonprice Response*

In a manner consistent with homogeneous oligopoly behavior, the domestic steel producers reacted to the import threat with a technological and political counterattack. The technological move was designed to introduce cost-reducing equipment and thus render the industry more competitive and less vulnerable to foreign steel produced under modern, technologically progressive conditions. The political stratagems were designed to insulate the domestic market from the incursions of foreign competitors—i.e., to restore the *status quo ante*, when the U.S. market was beyond the reach of less efficient, higher-cost foreign competitors.

#### *Technology*

The rising volume of steel imports—as well as the inroads made into steel markets by competing materials like aluminum, glass, cement, etc.—eventually persuaded domestic steelmakers to introduce modern production techniques already in widespread use in other major steel-producing countries. Most notable among these were the oxygen process and continuous casting. Both were adopted by major U.S. producers belatedly and with apparent reluctance.

The oxygen furnace, hailed as the greatest technical breakthrough since the shift from the Bessemer converter to the open hearth, requires only half the capital investment of the open-hearth furnace and yields operating savings of \$3 to \$10 per ton (depending on the facilities being replaced) [14, p. 14]. “Industry sources,” said the *Wall Street Journal*, “estimate that an oxygen unit can convert raw materials into steel for about \$8 to \$9 a ton. In an open hearth . . . the cost ranges from \$10 to \$15 a ton.” Equally impressive is its speed. According to Roger Blough, the oxygen process “is about eight times faster than the conventional open-hearth method” [26, p. 4]. Other estimates are more conservative: the oxygen furnace is said to produce twice as much steel as the open hearth and to shave \$3 to \$6 per ton off production costs [24, p. 1].

Despite these apparent advantages of the oxygen technique, and despite its widespread use in Europe more than ten years ago, the U.S. steel industry was slow to adopt it. McLouth, a minor producer, was the first to introduce an oxygen furnace in 1955, followed by Kaiser, another minor producer. The most oxygen capacity since then added in any one year was 3.3 million ingot tons in 1962. This year, another 10.2 million tons of capacity are scheduled to go into operation, and next year an additional 10 million tons are likely to be completed [25, p. 26]. The companies seem now to be persuaded that the oxygen process is here to stay and that it can provide sorely needed production economies.

The second major innovation recently adopted is continuous casting, which by-passes a series of steps currently needed in U.S. mills to shape liquid steel into semifinished solid metal. Without it, steel must first be molded and hardened into ingots; ingots must then be reheated in soaking pits, and the reheated ingots must then be sent through a rolling mill for squashing into slabs, billets, and blooms. Some of the equipment-makers claim remarkable cost savings for continuous casting—"a 30 to 50 per cent saving in capital costs and average operating savings of \$4 to \$6 a ton over today's standard techniques" [14, p. 14].

Continuous casting, according to *Iron Age*, "began moving in high gear abroad about eight years ago. Close to 40 machines were installed in the period from 1955 through 1962." The U.S. steel industry, however, did not take a second look at the new process until 1961. Except for a pilot plant at U.S. Steel's Chicago works, the first commercial continuous-casting unit went into operation in 1962—and, like the oxygen process, at the plant of a minor producer (Roanoke Electric Steel Corp.). With cost savings typically estimated at \$8 to \$10 per ton, it is not surprising that steel men forecast a \$1 billion investment in continuous-casting facilities over the next 10 to 20 years. "Within the next five years," says one, "every major mill and most minor ones will have at least one installation" [13].

In sum, both continuous casting and the oxygen process began to be introduced in U.S. steel mills at a significant rate only after the import threat became serious. They were adopted by major mills in the United States at least five years after European mills had recognized them as major cost-saving techniques. They were adopted after the U.S. industry had installed approximately 40 millions tons of new capacity that was obsolete almost the moment it was put into place.<sup>14</sup>

<sup>14</sup> [7, pp. 144-46]. Even though the integrated companies have begun to install oxygen and continuous-casting facilities, their wire-rod capacity still seems to lag behind the most modern European and Japanese plants. See, e.g., Kurt Orban, "A Discussion of Free World Wire Rod Mills Outside of the United States and Canada," *Wire and Wire Products*, Nov. 1962, 1603-12.

*Politics*

Faced with rising imports and possible erosion of its administered price structure, yet unwilling to meet foreign competition in the market place, the steel industry turned for relief to the government. Invoking the provisions of the Antidumping Act of 1921, eight major steel companies requested the Secretary of the Treasury to certify that West European and Japanese wire rods were being imported into the United States at less than fair value; they requested the Tariff Commission to find that a U.S. industry was being injured or likely to be injured by reason of the importation of such merchandise; finally, complainants asked for the assessment of antidumping duties in an amount equal to the difference between the import price and the foreign market value in the exporting country [23]. The obvious purpose of the complaint was to alleviate pressure on the domestic price structure and to build a *cordon sanitaire* around the domestic wire-rod market.<sup>15</sup>

The rationale of the complaint was the doctrine of fair competition. Dumping, according to the U.S. producers, was a simple matter of arithmetic; whenever a foreign producer charges a higher price in his home market than in the export market, he is guilty of unfair discrimination and unfair competition. He can practice dumping only because he enjoys a privileged position in his home market—through artificial restraints like tariffs, quotas, cartels, etc.—and this enables him to wage an unequal competitive battle against his dumping victims. (“The Dodgers,” as Roger Blough explains it [3, p. 4], “would experience difficulty maintaining a semblance of league leadership if the teams they compete against were allowed to take four strikes per batter against their three.”) Dumping, therefore, should be prohibited because it promotes unfair competition; because it is inherently discriminatory; because it interferes with the continuity and efficiency of domestic production; and because it does not provide domestic consumers with a dependable “low-cost” source of supply. Dumping, in short, undermines the integrity of *competition* and has a deleterious effect on domestic *competitors*.

In response to the complaint, the European exporters readily conceded that they were discriminating—that the price charged in the United States was lower than that in their home market. They pointed out, however, that their U.S. price was simply designed to meet the equally low price of their Japanese competitors. The Japanese, it was es-

<sup>15</sup> Nevertheless, M. A. Adelman states: “It must be said, to the credit of the steel executives, that they have refrained from cry-baby complaints about ‘unfair’ competition from foreign steel. This is a privilege reserved generally to small business industries.” “Reply,” *Quart. Jour. Econ.*, May 1962, p. 327.

tablished, were the price leaders on imported wire rod in the U.S. market and, ironically enough, they could not be convicted of dumping—even in an arithmetic sense—because their U.S. prices were identical to their home-market prices. Moreover, the exporters asked the Tariff Commission how the domestic steel industry could argue that the influx of 644,000 tons of foreign wire rod, valued at roughly \$65 million could cause it serious injury—when a Bethlehem executive said that the loss of his corporation's shipyards (accounting for 12-15 per cent of Bethlehem's billings, 10 per cent of its net, 6 per cent of its total investment, and \$152 million of its revenues) would "hardly cause a ripple in Bethlehem" [5, p. 248]. How could the domestic producers claim injury through unfair competition when the entire Belgian steel *industry* was hardly bigger than a single *plant* (Sparrows Point) of a single U.S. producer? Furthermore, how could the domestic industry claim injury from imported wire rods, when it failed to respond in any way to meet the threat of such imports through price reductions in the market place? Finally, the exporters asked, how can the domestic industry blame others for an injury which, if it exists at all, is self-inflicted—an injury which stems from the fact that wire rods happen to be the most extreme and dramatic example of the U.S. steel industry's noncompetitive, insensitive, administered price policy? Such injury, the exporters concluded, as the steel industry may have suffered, it has inflicted on itself by choosing *sua sponte* to price itself out of domestic and world markets.

On a more analytical plane, of course, the exporters could have shown that discrimination between the home and export markets is not undesirable per se; that domestic concerns frequently resort to both personal and geographical price discrimination; that a variant of basing-point pricing has always been a hallowed tradition in American steel circles; and that, under U.S. law, price discrimination is perfectly lawful where there is no substantial lessening of competition or tendency to monopoly, where differences in price can be accounted for by differences in cost, or where a discriminatorily low price represents the meeting of competition in good faith. Moreover, they could have argued that, in industries dominated by few sellers, discrimination is perhaps the only technique for eroding an oligopolistic and inflexible price structure—that in the absence of such discrimination there might be no competition at all. Indeed, they could have pointed out, as their U.S. counterparts had so often done before various antitrust agencies, that, in situations where sellers are few, the absence of discriminatory pricing is not an index of competition but collusion—evidence that some follow-the-leader, live-and-let-live scheme is in effective operation. To protect the domestic industry from "dumping" would have

meant its insulation from needed competitive pressures. It would have protected the industry from the bargaining power of its customers rather than affording its customers (the independent fabricators) protection against oligopolistic exploitation. This would have been tantamount to fostering the Robinson-Patman style of "soft" competition that the U.S. steel industry had always decried.

In any event, the Tariff Commission dismissed the dumping complaint on the ground that it could not find actual or probable injury to the domestic steel industry [21]. In so doing, the Commission, ironically enough, accepted some of the very arguments in the field of international competition that the industry had intended only for domestic consumption.

Defeated in its dumping action before the Tariff Commission, the industry began to press its case in two new forums. It pleaded with Congress to amend the antidumping legislation with a view to giving the domestic market more adequate protection against "unfair" foreign competition.<sup>16</sup> At the same time, steel spokesmen called for international industrial diplomacy to clean up what Roger Blough called the "international trade jungle" [3, p. 15].

European Coal & Steel Community (ECSC) producers have voiced similar demands. Complaining about the "market disruption" resulting from burgeoning imports, especially from the Soviet bloc, Japan, Great Britain, and Austria,<sup>17</sup> they have begun to call for ententes, accords, understandings, and accommodations—i.e., the reconstitution of what in impolite society would be called an international steel cartel.<sup>18</sup>

<sup>16</sup> In a spirit of bipartisanship, Senators Humphrey (Dem.-Minn.) and Scott (Rep.-Pa.) have introduced S.1318 to "improve the substance of the (Antidumping) Act through elimination of loopholes" and to "provide more effective procedures." In some quarters, however, their effort is perceived as part of a new drive for U.S. protectionism. See, e.g., *The American Swedish Monthly*, July 1963, Dec. 1963, and Feb. 1964, and *Norwegian American Commerce*, May-June 1963 and July-Aug. 1963. Also noteworthy in this connection are the current deliberations on "dumping" by the U.S. Treasury, the Tariff Commission, and the Trade Information Committee of the Office of the Special Representative for Trade Negotiations (the Herter Committee). *Wall Street Journal*, March 18, 1964, p. 30.

<sup>17</sup> "This increase in the volume of imports, linked to the world steel surplus," reports the ECSC High Authority, "is effected at marginal prices. The joint effect of the increase in imports and the low prices at which they are sold, is a constant and growing pressure on the prices charged on their own internal market by Community steel firms. The steel firms are obliged to offer important reductions, through the system of price alignments, in order to maintain their position in the market." Press Release, Washington, Jan. 16, 1964. See also *Wall Street Journal*, April 29, 1963, p. 5; Jan. 8, 1964, p. 5; Jan. 17, 1964, p. 5; Bureau of National Affairs, International Trade Reporter's Common Market Survey, Dec. 6, 1963.

<sup>18</sup> See, e.g., address by Dr. Hans-Guenther Sohl before the Biennial Meeting of the German Iron and Steel Industry Association, Duesseldorf, July 9, 1963; "Stahlkrise: Der Tonnenmacher," *Der Spiegel*, July 10, 1963, pp. 22-34, and the readers' comments thereon

### V. Conclusion

The foregoing analysis has dealt with the behavior of both the vertically integrated and the nonintegrated segments of the steel industry under the impact of substantial imports of raw materials and fabricated products. We have catalogued the diverse responses of the industry to these imports and described the resulting modifications in market behavior. If it has shown nothing else, our case study has demonstrated that vertical market power leaves its characteristic imprint on business policies.

Our findings, it should be noted, are at odds with recent literature on the subject. Spengler, for example, notes that if restraint of trade exists, "it is the horizontal elements that need to be singled out for remedial treatment" [15, p. 352]. Adelman claims that "Vertical integration has no economic importance except as an extension of a pre-existing horizontal situation: market share or market control" [2, p. 509] [1]. To Bork, vertical integration is "almost entirely lacking in significance as an analytical tool." He argues that monopoly power is the power to alter market price; that such power depends on percentage control of the market and ease of entry; that vertical integration does not change either the degree of market control or the ease of entry; that vertical integration does not affect price policy because an integrated firm will maximize profits at each level and set output as though each level at which it operates were independent from all other levels (except in the bilateral monopoly case); that the vertical squeeze, therefore, is an optical illusion—representing nothing more than price cutting at one level in the vertical chain [4, pp. 194-201].

The Spengler-Adelman-Bork position is not in accord with the facts of life in the steel industry. To be sure, the nonintegrated fabricators responded to lower import prices on the individual products they produced because failure to do so would have meant extinction. As producers of a single product—or, at most, a handful of products—they had no alternative than to meet the price reductions precipitated by imports. In addition, they responded to the compression of their fabricating margins by turning in ever-increasing numbers to imported wire rods—that is, to lower-priced supplies of their basic raw material. Their reaction in both the product and raw-material markets, therefore, was roughly in accord with the profit-maximizing calculus applied in a horizontal context.

The integrated firms, by contrast, in a calculated effort to protect the industry's price structure refused to meet the import prices either

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in *Der Spiegel*, July 31, 1963, pp. 6-9, which blame the European, and especially the German, steel "crisis" on the Eurocrat planners in Brussels and the industry leaders who believed their overoptimistic prognostications on steel demand.

of products or rods. On some products, the integrated giants lowered prices to meet price cuts of their domestic nonintegrated competitors. Elsewhere, they preferred to abandon the field to imports altogether rather than to reduce their product prices. For the integrated firms, abandonment was a substitute for adjustment in response to exogenous market stimuli. Moreover, the integrated majors—unlike the horizontally oriented fabricators—never turned to imported rods in order to improve their competitive position. Given their vertical organization, they were quite understandably committed to an investment in their own supply channels [6, p. 432] [9, pp. 97-99] [11, pp. 120-23], and therefore resisted *in extremis* any resort to lower-cost supply alternatives. While the independents were successful—at least temporarily—in meeting the import challenge in products by using lower-priced foreign rods, their integrated rivals could not embrace this solution, which for them would have meant a denial of the *raison d'être* of vertical integration. To the integrated firms it obviously seemed preferable to abandon a single-product line than to jeopardize their investment in an entire branch of the industry.

At the wire-rod level, the integrated firms seemed to react perversely to the import threat. They first raised prices (1955-58) and then maintained them with catatonic insensitivity (1958-63.)<sup>19</sup> They did so, not

<sup>19</sup> According to the Spengler theory on vertical integration, rational monopolists or oligopolists would transfer semifinished products from one stage to another at prices free from monopolistic profits, thereby permitting the final price to be set at a lower figure than a price resulting from a series of transactions between independent bilateral oligopolists. If the steel industry conformed to Spengler's hypothesis, the transfer price for wire rods should have been affected by, if not identical with, the cost of imported rods of commensurate quality. The consequent reduction in the cost of wire-rod inputs to the wire divisions should have depressed the price of wire and, ultimately, wire-products prices.

That prices did not behave as the hypothesis suggests is obvious. It is instructive to inquire why. First, the structure of the industry was not that envisaged by Spengler. Integration is not complete, but is tapered forward, so that prices for wire rod and wire are not merely accounting transfer prices, but have a function vis-a-vis independent customers. Second, these prices are fixed by an administrative process, in the course of which a number of forces are brought to bear on price determination. Not only do the industry leaders take into account target return, but also the possible interactions of prices of wire rod, wire, and wire products, and the attitudes of customers to changes in accustomed differentials. Third, we may doubt whether, in any event, Spengler's optimism is warranted. There is little to show that transfer prices within a vertically integrated firm would, in actuality, be set at lower levels than market prices. In most U.S. industries, oligopolistic pricing policies, which might be expected to set prices in the market, would be regarded as proper procedures by managers responsible for transfer pricing within a vertically integrated firm. Hence, we would not anticipate that a shift to vertical integration in an industry already characterized by horizontal oligopoly would be reflected in appreciable price reductions because of elimination of interstitial profits. In fact, the probabilities would seem to lie in the opposite direction. While a horizontal oligopoly might be immediately affected by import competition, and forced to modify its pricing procedures and standards, a partially vertically integrated industry could continue to resist such pressures at one level, maintaining "cost" constituents that a horizontal oligopoly would have been forced to abandon.

only because of their addiction to cost-plus, target-rate-of-return pricing, but more importantly in an attempt to maintain the vertical relationship between wire-rod and wire-product prices. Indeed, their crucial counterstrategy was to stop the swelling tide of rod imports by political means, i.e., the antidumping proceeding, in order to cut off the vital supply of (cheap) raw materials to their independent customer-competitors and thereby to protect the vertical price structure from erosion. Their objective was not to safeguard their wire-rod price *per se* or their products prices *per se*, but to preserve their dominance and control over the entire vertical chain. And this they could only accomplish as long as they were in a position to parlay their horizontal market control in the rod stage into strategic leverage over the product stage.

If vertical integration were irrelevant to price behavior, i.e., if the steel industry were organized in the form of successive horizontal oligopolies, the import phenomenon would clearly have had a different impact. At each stage, i.e., wire rods and wire products, the horizontal oligopolies would have greeted substantial imports in the same manner as they would the appearance of a nonconformist price-cutter. The majors would have had to respond in some fashion to the price threat—by (1) meeting it frontally, in the form of price reductions; (2) indirect cost reduction through turning to cheaper supply alternatives; (3) the construction of more up-to-date efficient production facilities; or (4) absorption of the interloper into the oligopolistic fraternity. Unless response (4) was feasible, the appearance of imports at, e.g., the fabricated-products level would have elicited not only a price response on products, but would have affected the derived demand for rods and, even with horizontal oligopoly in rods, sent shock waves through the entire vertical structure of the industry—forcing a sympathetic reaction in wire, wire-rod, billet, ingot prices, etc. A substantial volume of rod imports would likewise have had an immediate (or, at least, delayed) price response from a horizontally oriented oligopoly.<sup>20</sup>

Certainly no horizontal oligopolist would have preferred abandonment to adjustment as a market strategy; no rational profit-maximizer would have insisted on using a captive raw-material supply—on pain of dismantling his fabrication facilities and forfeiting his market position. The horizontally oriented nonintegrated fabricators could not

<sup>20</sup> Horizontally oriented oligopolies appear to have a rather low tolerance level for nonconformists. When imports captured 10 per cent of the automobile market in 1959, the Big Three suddenly discovered that the American public liked compact cars. In cigarettes, the Big Four instituted drastic price cuts directed at the so-called 10-cent brands when the latter's market share rose to more than 20 per cent in 1932. Even in a regulated oligopoly like airlines, the majors could not afford to ignore the marginal competition of the nonscheduled carriers, and felt compelled to introduce coach service and cut rates.

have afforded such a grand gesture; and it is significant that they did not make it.

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## DIVIDENDS AND STOCK PRICES

By IRWIN FRIEND AND MARSHALL PUCKETT\*

The recent literature has been characterized by considerable controversy and confusion over the relative importance of dividends and retained earnings in determining the price-earnings ratios of common stocks.<sup>1</sup> The disagreement over theoretical specifications of the expected relationship seems to us to have reached a point of rapidly diminishing returns, with much of the disagreement reflecting differences in interpretation of the questions being raised. However, there do seem to be very real difficulties in the reconciliation of available empirical findings with almost any sensible theory, and in the derivation of more definitive tests to choose among different specifications. This paper, after briefly reviewing the relevant theory and earlier findings, will discuss the limitations of these findings, describe various approaches to avoiding these limitations, and present new results that seem more in accord with theoretical preconceptions.

Relative prices of different issues of stock at a point of time are presumably determined by suitable discounting of expected future returns. These returns may take the form of dividend income or capital gains, both of which, assuming rational behavior, should be estimated on an after-tax basis with a higher average tax applicable to dividend income than to capital gains.<sup>2</sup> The discount factor relevant to these expectations of future return is a function of both the pure rate (or rates) of interest and the degree of risk associated with a particular issue—the evaluation of risk reflecting both the subjective probability distribution of expected return on total capital of the issuer and the degree of financial leverage in the issuer's capital structure.

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<sup>1</sup> See Modigliani and Miller [14], Durand's "Comment" [3], and Modigliani and Miller's "Reply" [15]; Gordon [6] [7]; Fisher [4]; Benishay [1]; Miller and Modigliani [13]; Lintner [10]; and Solomon [17].

<sup>2</sup> The magnitude of this tax differential is difficult to evaluate, but available evidence indicates it may be smaller than commonly supposed. For instance, past yield spreads between institutional grade corporates and tax-exempt bonds are explained by an investor tax bracket of approximately 20 per cent-25 per cent. However, in view of the institutional forces affecting the corporate bond market, this figure is undoubtedly lower than the tax rate characterizing the marginal investor in the stock market—but we have no idea just how much lower.

The fact that investors are willing to hold (or buy) a company's shares at the prevailing price implies that the rate of discount which equates their income expectation with market price constitutes a rate of return at least as high as could be obtained in alternative investments of comparable risk. Now, if these investors are willing to increase their holdings of shares at the same rate of market return, they should also be willing to forego current dividends insofar as the added equity investment yields this rate. Stated another way, investors should be indifferent if the present value of the additional future returns resulting from earnings retention equals the amount of dividends foregone. Moreover, because increases in present value (market price) are realizable as capital gains, earnings retention carries a tax advantage that lowers the rate of return on corporate investment necessary for shareholder indifference between current dividends and earnings retention.

The influence of earnings retention on share prices should therefore be a function of the profitability of corporate investment opportunities, *ceteris paribus*, in view of the fact that external equity financing is generally not a completely satisfactory substitute for internal financing. When this corporate rate of profit exceeds the minimum rate required by stockholders, price should increase as the proportion of earnings retained increases (though, since profitability is presumably a decreasing function of the amount of investment, beyond some point increased retention associated with excessive investment may depress the marginal return on investment below the required rate). Conversely, when the corporation's profit rate is less than the market rate, price should decrease with increasing earnings retention.<sup>3</sup>

Despite these theoretical conclusions, empirical findings indicate that, when stock prices are related to current dividends and retained earnings, higher dividend payout is usually associated with higher price-earnings ratios. This result, it might be noted, is found just about as often in highly profitable "growth" industries as it is in less profitable ones. Probably the earliest and best-known observation of this "dividend effect" was made a generation ago by Graham and Dodd [8], who went so far as to assert that a dollar of dividends has four times the average impact on price as does a dollar of retained earnings.<sup>4</sup> More recent statistical studies by Myron Gordon, David Dur-

<sup>3</sup> As Miller and Modigliani [13] have shown, external equity financing destroys this relationship under conditions of costless flotation and no capital gains tax advantage, if changes in dividends are compensated by changes in external equity financing of equal magnitude and opposite direction (rate of equity investment held constant). However, flotation is not costless, capital gains tax advantages do exist, and the rate of earnings retention is in all likelihood greatly influenced by the rate of investment.

<sup>4</sup> The fourth edition (1962) of this text on security analysis modifies this conclusion only moderately.

and, and others indicate that the dividend multiplier is still several times the retained earnings multiplier, with Gordon [6] finding little change on the average in the four-to-one ratio of the two multipliers, though the ratio varies widely and inconsistently from industry to industry and from year to year. These statistical results, it might be noted, are based on a large number of cross-section studies utilizing linear and logarithmic (and occasionally even other)<sup>6</sup> relationships between prices and both dividends and retained earnings to explain price variations in samples of companies drawn from particular industries.

Despite the massive array of statistical results tending to confirm the existence of a strong dividend effect, many market analysts have become increasingly skeptical of their validity. With the rise in market emphasis on growth in recent years, and the presumed close relationship between growth and retention of earnings in the minds of investors and managements, it seems strange to many analysts that a dollar of retained earnings (or of total earnings) should be valued so low relative to a dollar of dividends—and even stranger that there seems to have been no substantial shift in the relationship in recent years. Moreover, these doubts are supported quite strongly by several past surveys of shareholder opinion that indicate earnings and capital gains do, in fact, weigh more heavily than dividends in evaluating the relative desirability of alternative stock investments: "Investors who say a change in corporate earnings would influence their investment decisions outnumber by three to one those who would be influenced by a change in dividends."<sup>7</sup>

The behavioral assumptions necessary for theoretical support of a consistently lower market valuation of retained earnings than of dividends are also quite suspect. This lower valuation could exist if any one of the following situations is present: (1) the average holder of common stock possesses, at the *margin* of his portfolio, a very strong preference for current income over future income (a situation which hardly could be expected to persist over time); (2) the expected increase in earnings arising from increased per-share investment is viewed as involving a much higher degree of risk than that attaching to earnings on existing corporate assets; (3) the profitability of incremental corporate investment, as viewed by shareholders, is extremely low relative to the competitive yield prevailing in the stock market.

Each of the first two of these assumptions implies high rates of discount on incremental investment which would result in little short-run

<sup>6</sup> See, e.g., Johnson, Shapito, and O'Meara, Jr. [9].

<sup>7</sup> Friend and Parker [5]. Also see Merrill Lynch, Pierce, Fenner and Smith [12]: "Just about all surveys of shareholders in recent years show investors are primarily interested in one thing—capital gains—almost two-thirds of our customers placed this at the top of their list. . . . The emphasis on appreciation rises as income rises."

price appreciation from earnings retention, even though the expected profitability of additional investment may be quite high. However, neither of these assumptions is consistent with observed behavior of the market. Contrary to what might be expected from both of these assumptions, we do not normally witness perceptible drops in the market price level when the aggregate supply of corporate stock is increased by new issues, requiring for their absorption the substitution of current for future income and potentially raising the risk premium demanded by investors; nor do we typically witness sharp drops in per-share price when the supply of an individual company's shares is increased. It is possible to infer of course that these increases in supply are precisely timed so as to be automatically offset by upward shifts in investor expectations, but this seems completely unrealistic. Thus, both of these first two assumptions can be questioned on the basis of market behavior as well as logical content.

The third assumption—that investors view the profitability of incremental investment as being quite low—also seems highly suspect. Marginal profit rates in a substantial number of industries appear to be quite high, and undue pessimism is hardly consistent with the accepted image of the average shareholder. Moreover, the generally favorable market reaction to new public stock offerings in recent years further belies the prevalence of any pessimistic beliefs about marginal profit rates.

In view of all this, it is our opinion that those statistical studies purporting to show a strong market preference for dividends are in error—especially since the analysis typically employed (described in the following section) includes as a part of the market's valuation of retained earnings the price paid for the relatively high internal rates of return which might be expected to be associated with high retention. Nonetheless, we would still not expect to find a uniform preference for dividends even if internal rates of return were held constant over the sample of companies being examined.<sup>7</sup> We do not, however, deny the existence of instances in which retained earnings are valued less than dividends. Certainly, some companies may be controlled by managements who knowingly do not act in the shareholder's best interest, or there may be sharp disagreement between these two groups over how that interest is defined. However, we feel that these instances are likely to be the exception rather than the rule. Moreover, we would

<sup>7</sup> Lintner [10] maintains that under certain types of uncertainty retained earnings would be preferred to dividends if the alternative is a new stock issue to finance a given investment. If this is true, though we have some doubts, it would reinforce our argument. On the other hand, again as a result of uncertainty, Lintner argues that for firms with low leverage, investors will prefer higher dividends associated with higher corporate debt to greater retention and lower dividends.

expect that for the average firm, irrespective of investor preferences between dividends and capital gains, payout policies are such that at the margin a dollar of retained earnings should be approximately equal in market value to the dollar of dividends foregone.

### I. *Possible Sources of Statistical Bias*

In support of our position, we will now outline a considerable number of reasons why previous statistical studies yielded biased results. These comments are directed in particular at the following regression equation, which is the one most commonly applied to cross-section data, but most of them also apply to past variations in this equation:

$$(1) \quad P_{it} = a + b D_{it} + c R_{it} + e_{it}.$$

Reading from left to right, the variables represent per-share price, dividends, and retained earnings. The subscript  $i$  denotes the  $i^{\text{th}}$  company in a sample of  $n$  companies selected from a particular industry, and all variables are measured in the  $t^{\text{th}}$  time period.

Before discussing the reasons for bias in the application of this relation, we might point out that even those who believe that a higher  $b$  than  $c$ —the typical result—indicates investor preference for dividends seem nonetheless to feel that the optimum earnings payout ratio is normally less than one, which could be regarded as inconsistent with the result obtained. The implicit assumption in the above equation that optimal earnings payout—i.e., that giving rise to the highest price-earnings ratio—is either all, none, or a matter of indifference is, of course, highly questionable. However, even though this equation is quite deficient in that it does not admit of a unique optimum payout between the extremes of zero and 100 per cent, it may still be quite useful for estimating price behavior within the observed range of dividend payout.

Thus, if the companies in a sample tend, *on the average*, to pay out less than the optimum,  $b$  should be greater than  $c$ ; if they pay out more than the optimum,  $b$  should be less than  $c$ ; and if they pay just the optimum, regardless of what that optimum is, or if the payout is a matter of indifference,  $b$  should equal  $c$ . Theory would suggest that regardless of the optimum payout for any individual company, at that optimum \$1 of dividends would on the average have the same effect on stock price as \$1 of retained earnings.<sup>8</sup> Any difference between the values of  $b$  and  $c$  therefore represents either a disequilibrium payout position or a statistical limitation of the analysis employed, including most notably a correlation of dividends or retained earnings with omitted

<sup>8</sup> This conclusion does not depend on whether, under uncertainty, the relevant discount rate of investors is a function of the dividend rate.

factors affecting price. Our analysis will attempt to distinguish between these two possibilities, though the persistence of the relative importance of payout on price, and its apparent invariance to such factors as the rise in market emphasis on growth through earnings retention, argues against the disequilibrium explanation even if this condition is regarded as fairly permanent.

While the linear form used does not provide measures of optimum payout, it can give useful insights into the size of such payout through examination of the residuals over the range of payout experienced. However, an adequate study of the size of optimal payout ratios under varying circumstances requires a more complex statistical analysis, allowing for many more variables than have been incorporated in the framework used in this paper, and will be deferred to a subsequent study. Our analysis in this paper is directed only to an assessment of the general rationality of company payout policy in view of the usual findings (i.e.,  $b$  systematically higher than  $c$ ) which imply that management typically tends to maintain too low a payout.

It will be noted that the following discussion of regression problems centers exclusively on those encountered in cross-section analysis. While it is possible to work with time-series information, and we shall do so to a limited extent, virtually all of our predecessors used cross-section data in their regression studies, and it is these studies with which we take issue.

#### *A. Omitted Variables*

The above equation assumes, among other things: (1) either that risk is held constant by restricting the sample to a particular industry or else that dividend payout is uncorrelated with risk; and (2) that expectations of growth are determined solely by the relative amount of current earnings retention or alternatively that growth from other sources is uncorrelated with the relative amount of retention.

With regard to the first of these assumptions, a typical sample of 20 or so companies drawn from industries such as foods, steels, or chemicals contains enormous variations in company size, financial structure, and product mix; therefore, it appears totally unwarranted to assume that risk variations within these industry samples are negligible. In view of what we know about managerial desire to avoid dividend cuts, it certainly seems logical to expect that companies facing greater uncertainty about future profit performance would adopt lower current dividend payout as a means of hedging the risk of being forced to cut their dividend. Thus, high risk may *result* in both low payout and low price-earnings ratios, whereas low risk may *result* in high payout and high price-earnings ratios. Consequently, omission of a risk variable

from the regression equation could conceivably impart a substantial upward bias to the dividend coefficient, depending upon both the extent to which risk varies between companies and the strength of risk in determining current payout.

It might be noted that a few previous writers have incorporated a risk variable in the regression equations. However, this variable is usually measured in terms of earnings fluctuations, and is heavily influenced by cyclical factors in the company's operating performance. We are inclined to doubt that cyclical earnings variability is a good proxy measure of investor uncertainty about future long-run performance, and therefore are dubious that the problem of risk has been handled properly.

Now, the second assumption—that growth deriving from sources other than earnings retention is either nonexistent or nonbiasing—is equally questionable. Growth in per-share earnings, apart from that arising from more efficient use of existing capital, can occur through externally as well as internally financed investment, and there may be a biasing correlation within a sample of companies between internally and externally financed growth. Suppose, for instance, that some companies prefer (or in the case of public utilities are encouraged by regulatory agencies to have) low earnings retention compensated by large amounts of external financing, whereas other companies prefer high earnings retention and small amounts of external financing. This would impair the relationship between earnings retention and growth, thereby reducing the coefficient on retained earnings when the variable measuring externally financed growth is omitted.\*

However, it is equally possible that the relationship between internal and external financing could run the other way—that is, that high rates of retention are associated with relatively heavy external financing and low rates of retention with small amounts of external financing. The inherent advantages of retained earnings undoubtedly encourage maximum use of this source of funds before resorting to the capital markets. Thus, external financing may be associated with high earnings retention for companies with abundant investment opportunities, whereas the absence of external financing may be associated with lower earnings retention for other companies. In this case the rate of earnings retention is positively correlated with external financing, and

\* If the objective were to arrive at the relative evaluation of dividends and retained earnings *with a given internal rate of return*, then the problem of bias would arise from the omission of the expected growth rate rather than from that of externally financed growth. An analysis which includes past growth rates (as proxies for expected future growth) is Benishay [1]. Gordon [7] does introduce both internally and externally financed growth, but the form of the relation he uses does not permit the isolation of dividends from retained earnings effects.

the retained earnings coefficient is biased upward by omission of any consideration of externally financed growth.

In summary, whereas the bias due to omission of a risk variable is reasonably clear in direction (but not in magnitude), the nature of the bias resulting from omission of a variable to take account of externally financed growth is not clear. Nonetheless, both of these variables are potentially too important to be omitted from the regression equation without proof of the negligibility of their influence on regression results.

### *B. Regression Weights*

Contributing to the difficulties imposed by omitted variables is the separate but correlated problem of regression weighting. An individual observation influences regression results according to the extent to which that observation departs from the sample average. Thus, extreme values are much more important in determining regression results than are those values centered more closely about the sample average.

Now, it is a generally accepted fact that high-quality stocks tend to be characterized by high per-share values (prices, dividends, and earnings), whereas low-quality stocks are characterized by low per-share values. Further, as discussed immediately above, high-quality stocks may tend to pay out a higher proportion of earnings than do the low-quality issues. Thus, the association between regression weights and investment quality may exaggerate whatever regression bias exists due to an association between investment quality and dividend payout. This problem of regression weights is reduced when regression variables are converted to logarithms, which was done in some earlier studies.

### *C. Random Variations in Income*

The income reported by a corporation in any particular period is subject to a host of short-run economic and accounting factors that render it different from what would have been reported under "normal" conditions. Now if prices are related to normal rather than reported income, and if the short-run disturbances in reported earnings do not produce equiproportional disturbances in dividends, then a regression equation of the standard form will be biased in favor of dividend payout influences—a point touched upon by earlier writers but in a rather different context. To clarify, companies with above-normal earnings at a point of time will be characterized by both low price-earnings and dividend payout ratios, whereas companies reporting below-normal earnings will be characterized by both high price-earnings and high payout ratios.

The two conditions essential to the above argument are: (1) that dividends are a more stable time series than reported earnings; and (2) that the elasticity of shareholder expectations with respect to short-run income movements is less than unity. The existence of the first of these conditions has been proved quite conclusively by several studies of corporate dividend payout policy which show a rather slow adaptation of dividends to changes in reported earnings.<sup>10</sup> The second condition has, to our knowledge, not been proved, but in view of the great amount of background data available for the formulation of per-share income expectations, it seems reasonable to suppose that expectations are rather insensitive to those short-run movements in relative earnings which are unrelated to fundamental changes in long-run prospects for the economy, the industry, or the company.

#### *D. Income Measurement Errors*

The diversity of accounting procedures employed in estimating business earnings gives rise to consistent measurement errors that can bias regression results in favor of dividends on two counts. First, the simple fact that retained earnings are measured imprecisely exerts a downward bias on the coefficient for that variable (dividends are measured precisely, and therefore no such bias exists here). Since retained earnings are generally of the order of magnitude of one-third total earnings, and all of the probably sizable measurement error in total earnings affects the estimate of retained earnings, the downward bias in the retained earnings coefficient could be quite large.<sup>11</sup> Second, if both prices and dividends are geared to the economically "correct" value of earnings, then firms that employ accounting methods yielding relatively high reported earnings will be characterized by both low payout and low price-earnings ratios compared to the sample average, whereas those firms which maximize current deductions will show both high payout and high price-earnings ratios relative to the sample average. As a result, the retained earnings coefficient is again biased downward relative to the dividends coefficient.

Strictly speaking, the second of these accounting effects could logically have been discussed under the heading "omitted variables," above. However, the apparent lack of appreciation for the interrelation between accounting biases in reported earnings and the measured effect of dividends and retained earnings on share prices prompted this separate discussion as a means of drawing attention to what is

<sup>10</sup> See especially Lintner [11], and also Darling [2].

<sup>11</sup> See, e.g., Wold [18]. On the other hand, several analyses using average dividends and same relative importance to dividends in explaining stock prices, though this use of average retained income as explanatory variables (e.g., see Gordon [6]) attach about the averages would be expected to reduce the bias in the estimated influence of retained earnings.

potentially one of the most glaring weaknesses in those studies. In subsequent discussions of regression models and results, accounting limitations of the type described above will generally be incorporated in the "omitted variable" category.

It might be noted that the above discussion does not include accounting errors of the type that, because of fluctuations in certain costs such as write-offs, advertisement, research and development, etc., would result in random disturbances to an individual company's earnings. These are properly included in the "random income movements" category. We should also mention the problem of general over- or understatement of earnings. While the procedures we will use do adjust for differences among firms (which are consistent over time) in their reporting of earnings, they do not adjust for any general over- or understatement of earnings because of the formidable conceptual and statistical problems associated with identification and correction of this type error. We might emphasize, however, that if accounting estimates of earnings are generally less than those "estimated" by investors, the retained earnings coefficient will be biased upward.

#### *E. Least-Squares Bias*

Assuming that management and shareholder expectations are substantially independent, it is entirely plausible that a price-earnings ratio which is regarded as high by management will result in external stock financing and high payout, while capitalization ratios regarded as low will result in heavier reliance on internal financing and consequently low payout. Moreover, a similar result would occur if management were motivated only by a desire to maintain dividend-price ratios close to the average for the industry.

If these market relationships are reflected in cross-section data, the standard regression equation will yield results biased in favor of dividend payout because it assumes one-way causality between dividends and prices. That is, the equation fails to take account of the fact that dividend payout differences are, at least in part, the result rather than the cause of differences in price-earnings ratios. Resolution of this problem of dual causality requires the use of a complete model employing both demand and supply schedules for dividends.

## *II. Regression Models*

The problem of omitted variables could, of course, be directly handled by expanding the regression equation to include these variables. However, measurement of such slippery concepts as subjective risk evaluation, profitability of investment opportunities, sources of expected future financing, and accounting differences is both difficult and subject

to large error. Indirect approaches thus seem particularly attractive.

Theoretically speaking, continuous cross-section techniques are the most appealing of these approaches. If the separate effects on price of all omitted variables are aggregated, and this composite ("company" or "firm") effect given the designation  $F_i$ , then the basic regression equation (1) can be modified as follows:

$$(2) \quad P_{it} = a_i + b_i D_{it} + c_i R_{it} + F_i + e_{it}.$$

The assumption implicit in this equation is that firm effects, which cannot of course be measured directly, are both additive and constant over time. Such firm effects include those relevant to investor assessment of both profit prospects and risk, only some of which (e.g., size of firm and past trend and variability in book earnings) could alternatively be measured directly. Now if an identical equation is written for an earlier time period, and this last equation is then subtracted from the one above, the following result is achieved:

$$(3) \quad \begin{aligned} P_{it} - P_{i(t-1)} = & a_i - a_{i-1} + b_i D_{it} - b_{i-1} D_{i(t-1)} + c_i R_{it} \\ & - c_{i-1} R_{i(t-1)} + e_{it} - e_{i(t-1)}. \end{aligned}$$

The  $F_i$  do not appear in this equation, so the coefficients on dividends and retained earnings ought to be free of the bias due to firm effects.

Despite its theoretical appeal, this procedure runs into two complications: (1) in this particular application the error terms in the continuous cross-section difference equation become quite large; and (2) period-to-period movements in the variables can contain random elements and serial correlations that greatly impair the meaning and reliability of the coefficients.<sup>12</sup> The random movements in earnings are much more likely to be reflected in retained earnings than in dividends or in stock prices, depressing the apparent influence of retained earnings on stock prices. Moreover, the firm effects are more likely to be multiplicative than additive, and continuous cross-section techniques pose special problems in the multiplicative case. Consequently we feel other approaches are more promising for our purposes, at least at this stage of development of continuous cross-section techniques. In connection with these other approaches, we will henceforth assume a multiplicative relationship for the firm effects.

Thus suppose

$$(4) \quad F_{it} = f_i E_{it}$$

where  $f_i$  is now the firm-effect "multiplier" and  $E$  is per-share earnings.

<sup>12</sup> These complications can be avoided in part by the application of this type of cross-section difference equation to group rather than individual firm data, a procedure which has not been utilized previously and which we plan to experiment with.

The aggregate firm effect ( $F_{it}$ ) for any firm is assumed proportional to its per-share earnings, but the factor of proportionality ( $f_i$ ) differs among firms. If it is tentatively assumed that earnings payout effects are negligible, then

$$(5) \quad P_{it} = (k_i + f_i)E_{it},$$

where  $k_i$  is the average price-earnings ratio for the sample. Making the additional assumption that the  $f_i$  are constant over any two adjacent time periods:

$$(6) \quad f_{it} = f_{i(t-1)} = \frac{P_{i(t-1)}}{E_{i(t-1)}} - k_{t-1} = (P/E)'_{i(t-1)}.$$

Therefore, under the stated assumptions, firm effects can be held constant by introducing into the regression equation a variable  $[(P/E)'_{i(t-1)}]$  which measures individual deviations from the sample average price-earnings ratio in the previous time periods, i.e.,

$$(7) \quad P_{it} = a + bD_{it} + cR_{it} + d(P/E)'_{i(t-1)} + e_{it}.^{13}$$

In this equation (as well as in the continuous cross-section model) firm effects include the profitability of investment opportunities as assessed by the market.

The problem of least-squares bias can be handled by specifying a complete model including a dividend supply function as well as the customary price relation. For instance, let

$$(8) \quad P_{it} = a + bD_{it} + cR_{it} + d(P/E)'_{i(t-1)}$$

be the relation determining price; and let

$$(9) \quad D_{it} = e + fE_{it} + gD_{i(t-1)} + h(P/E)'_{i(t-1)}$$

be the dividend supply equation (error terms have been omitted). The dividend supply equation is developed by adding to the best type of relationship developed by previous writers<sup>14</sup> (which stresses the importance of past dividends on current dividends) a variable permitting the firm to adjust its current payout to past market valuation of its future earnings. The system is completed by the identity:

$$(10) \quad E_{it} = D_{it} + R_{it},$$

<sup>13</sup> In the regressions fitted subsequently,  $\{E\}_{i(t-1)}$  is used instead of  $\{E\}'_{i(t-1)}$  for statistical reasons. In these regressions, annual data are used as measures of the variables involved, though it can be argued that, at least for earnings, an average over a number of years might be preferable. While this was not tested, it might be pointed out that the use of such averages in another context did not affect the results significantly (see footnote 11) and that the assumed constancy of the firm effects becomes more tenuous the longer the period covered. Another approach to holding firm effects constant is to substitute equation (6) in equation (5), which results in equation (12), except that theoretically for this purpose the  $P_{i(t-1)}$  term in equation (12) should be multiplied by the ratio of  $E_{it}$  to  $E_{i(t-1)}$ .

<sup>14</sup> See Lintner [11].

where  $E_{it}$  may be considered exogenous but, of course, not  $D_{it}$  or  $R_{it}$ . Solving these equations (8), (9), and (10) we have:

$$(11) \quad P_{it} = [a + e(b - c)] + [c + f(b - c)]E_{it} + [g(b - c)]D_{i(t-1)} \\ + [d + h(b - c)](P/E)'_{i(t-1)}.$$

After obtaining the regression coefficients of equations (9) and (11) in the usual manner, the coefficients of equation (8) can be derived and are theoretically free of bias due to effects of price on dividend supply.

Turning to the problem of random income movements, the argument here, it will be recalled, implies that short-run changes in income evoke relatively small short-run changes in relative price. Thus, an equation of the form

$$(12) \quad P_{it} = a + bD_{it} + cR_{it} + dP_{i(t-1)}$$

in which previous expectations, measured by  $P_{i(t-1)}$ , as well as current dividend and earnings experience, are assumed to determine current price is consistent with this assumption about price behavior. The  $b$  and  $c$  coefficients in this equation measure the extent of short-run price adjustment to short-run changes in dividends and earnings retention, respectively. Long-run values of these coefficients can, after correcting  $d$  for movements in the general market price level, be obtained by dividing each through by  $1 - d$ , provided it can be assumed that  $d$  measures only the influence of past expectations on current expectations. One of the additional merits of this equation is that lagged price to some extent also holds constant any firm effects that exist; as a result, however, long-run coefficients computed by this method may be somewhat in error.

A more direct approach to the problem of short-run earnings movements lies in the "normalization" of those earnings. Other authors have attempted this—usually by use of centered or weighted averages—but we feel that these methods are somewhat less satisfactory than available alternatives. A simple average of earnings over, say, a three-year period may still contain large short-run components (relative to future expectations) if the adjustment process is slow. The same comment also applies to weighted averages and, in view of the relatively high weight usually given to current earnings, this technique may be even less satisfactory than the use of a simple average.

Of several alternatives available for earnings normalization, an approach that seems especially promising is one in which market estimates are employed. To illustrate, assume that price and dividends are always taken as "normal," and that short-run earnings abnormalities sum to zero over the sample of companies in question (the latter being a reasonable assumption for most industries over most periods if we avoid

major cyclical disturbances). Thus, the dividend-price ratio is assumed to be always normal but the earnings-price ratio is subject to short-run fluctuations. Variations about trend values of the ratio  $(E/P)_{it}/(E/P)_{kt}$  are, by assumption, due solely to short-run components of the  $i^{\text{th}}$  company's earnings, because the average earnings-price ratio for the sample  $[(E/P)_{kt}]$  is defined to be free of earnings disturbances. Examination of the scatter diagrams of the time-series behavior of this "relative earnings yield" ratio failed to indicate any reason for assigning anything other than a linear equation to the underlying time trend. Therefore, on an empirical basis,

$$(13) \quad \frac{(E/P)_{it}}{(E/P)_{kt}} = a_i + b_{it} + e_{it}.$$

Now normal value of company earnings-price ratios can be computed as follows (the superscript  $n$  denoting normalized value):

$$(14) \quad (E/P)_{it}^n = [a_i + b_{it}](E/P)_{kt}.$$

Having obtained a normalized value of the earnings-price ratio, normalized earnings are found by simply multiplying this ratio by per-share price. Normalized retained earnings are then obtained by subtracting observed dividends from normalized earnings.

The assumptions underlying this approach can, of course, be questioned—dividends do react to year-to-year fluctuations in earnings; price does contain speculative components; and earnings fluctuations may not sum to zero over the sample. However, dividends are likely to react only to fluctuations in earnings regarded as relatively permanent. Moreover, averaging price over a year should remove most relative speculative components of individual share prices since, in theory, arbitragers can be expected to hold these to short duration. Any error attaching to the assumption that sample average earnings are normal is likely to be small and thus its effect on the regression equation will also be small.<sup>15</sup> Nevertheless, prices, as we measure them, probably do contain individual speculative components of small magnitude. Thus normal earnings computed from measured prices will contain errors of the same relative magnitude and direction as those contained in price, so that some small bias is probably introduced in favor of the retained earnings coefficient.

All of the statistical models presented earlier can, of course, be modified to specify normalized values of those variables in which earn-

<sup>15</sup> This earnings normalization procedure was developed in Mr. Puckett's forthcoming Ph.D. dissertation, and the validity of its assumptions are subjected to critical evaluation in that work. Alternative methods of earnings normalization—for instance, by derivation of a normal dividend payout ratio—are also presented.

ings fluctuations play a distorting part. The regression results presented later embody two such modifications.

Finally, the influence of dividend payout on price can be subjected to time-series analysis. For instance, the above equations relating relative earnings yield to time can be compared with regression equations of the following form,

$$(15) \quad \frac{(D/E)_{it}}{(D/E)_{kt}} = a_i + b_{it} + e_{it},$$

to ascertain if changes over time in relative earnings-price ratios are consistently associated with changes in relative dividend payout ratios—and, if so, the nature of the association. This procedure, it might be noted, involves none of the assumptions necessary for derivation of normalized earnings and, hence, constitutes an independent check on their validity. Many other time-series tests of the relative importance of dividends and retained earnings are also possible.

### III. Regression Results

For most of our statistical analysis we worked with five industry samples, viz., chemicals, electronics, electric utilities, foods, and steels, in each of two years, 1956 and 1958. The industries were selected to permit a distinction to be made between the results for growth and non-growth industries and to provide a basis for comparison with results by other authors for earlier years. Both cyclical and noncyclical industries are covered. Ready accessibility of data and resource availability were also factors both in industry and year selection. An attempt was made to conform to a fairly narrow definition of the industries chosen so that the sample companies would be reasonably homogeneous in industrial composition.<sup>16</sup> The periods covered include a boom year for the economy when stock prices leveled off after a substantial rise (1956) and a somewhat depressed year for the economy when stock prices, however, rose strongly (1958). Originally we had intended including several earlier years as well, but it was not feasible to do this systematically.

Table 1 presents the usual simple linear relationships between average prices and dividends and retained earnings to show with the data we are using the kinds of results typically obtained, and to provide a basis of comparison with alternative regression models. In this analysis, we find the customary strong dividend and relatively weak retained earnings effect in three of the five industries—i.e., chemicals, foods, and steels. In these three industries, there is little evidence of any significant shift in the relative importance of retained earnings from earlier years as a result of increasing market emphasis on growth.<sup>17</sup> While these re-

<sup>16</sup> A list of the corporations included may be obtained on request.

<sup>17</sup> See, e.g., Gordon [6] for comparable 1951 and 1954 regressions for these industries.

TABLE 1—REGRESSION EQUATION:  $P_t = a + bD_t + cR_t$ 

Industry (sample size)	Regression Coefficients (standard errors)				
	<i>t</i>	<i>a</i>	<i>b</i>	<i>c</i>	$\bar{R}^2$
Chemicals ( <i>n</i> =20)	1956	-0.86	+29.94 (3.00)	+ 2.91 (4.98)	.868
	1958	-5.29	+27.72 (2.22)	+13.15 (5.65)	.910
Electronics ( <i>n</i> =20)	1956	+7.32	+ 7.27 (9.77)	+17.87 (6.60)	.410
	1958	+8.53	+13.56 (12.80)	+26.85 (6.57)	.524
Electric Utilities ( <i>n</i> =25)	1956	+0.85	+13.86 (2.35)	+14.91 (3.42)	.842
	1958	+1.11	+14.29 (3.36)	+18.54 (5.22)	.772
Foods ( <i>n</i> =25)	1956	+0.78	+15.56 (1.70)	+ 5.23 (1.30)	.834
	1958	+1.50	+17.73 (2.10)	+ 4.35 (1.56)	.805
Steels ( <i>n</i> =20)	1956	-2.28	+17.60 (2.65)	+ 2.45 (1.42)	.869
	1958	+8.55	+15.23 (1.63)	+ 5.98 (2.08)	.881

*Note:* Per-share price (average for year), dividends, and retained earnings are represented by  $P$ ,  $D$  and  $R$ , respectively;  $t$  designates year;  $n$ , size of sample;  $\bar{R}^2$ , coefficient of determination adjusted for degrees of freedom; and standard errors of regression coefficients are indicated under coefficients in parentheses.

sults may not be altogether surprising for steels and foods, they seem highly questionable for chemicals, which was probably regarded as a growth industry in this period. Even for steels and foods the magnitude of the difference between the dividend and retained earnings coefficients seems implausible.

The fact that electric utilities show fully as high a coefficient and electronics a higher coefficient for retained earnings than for dividends is in closer agreement with what might be expected. Unfortunately, we know of no earlier results for electronics that can be used as a basis of comparison.

For electric utilities, there are two earlier analyses that can be compared with our results, though the forms of the relationships used are somewhat different. The first, by Morrissey [16], relates the earnings-

price and dividends-price ratios separately to the dividend payout ratios for each of the years 1950-57 and finds that the payout ratio in this industry did affect stock prices significantly in the early part of this period (with higher prices associated with higher payout) but very little in the latter part of this period. Moreover, this study found a steady reduction in the importance of the payout ratio from year to year and some evidence that the direction of its effect had changed by the end of the period, i.e., that higher payout may have been associated with lower prices for given earnings by 1957. These results are consistent with investors' changing evaluations of the growth potential of electric utilities and, for the end of the period covered, with our results in 1956 and 1958. However, the second earlier analysis referred to, which derives a logarithmic relation between the ratio of stock prices to book value and both the ratio of earnings to book value and the payout ratio, yields a different result for utilities in early 1955 showing a positive relation between the price to book ratio and payout.<sup>18</sup>

A recomputation of the electric utilities regressions in Table 1 utilizing logarithms for all the variables again points to a higher dividend than retained earnings effect, unlike the result obtained in the linear form.<sup>19</sup> It is not possible to choose conclusively between the linear and logarithmic results on statistical or a priori grounds. The logarithmic relations do reduce the problem of regression weights referred to earlier (with a correlation between these weights and investment quality exaggerating any regression bias due to a correlation between quality and dividend payout). However, so do both the ratio relations discussed above and the more complex linear relations in subsequent tables, all of which give the same type of results for the electric utilities as the simple linear relations. Moreover, the ratio and linear regressions, unlike the logarithmic relations, can handle satisfactorily very small and negative retained earnings. We feel that the major difference between the logarithmic and nonlogarithmic regressions may be due to the differing degrees of bias in the regression coefficients produced by short-run income disturbances, and we shall attempt to hold these constant in some of the following statistical analyses.

To summarize the results so far, they provide a little more evidence than has existed heretofore that in growth industries (chemicals, electronics and electric utilities) more weight relatively is given to retained earnings than in nongrowth industries (steels and foods), but the evidence is not uniform (chemicals) and for one of the two remaining industries (electric utilities) depends partly on the mathematical form of the regression used. In any event, for three of five groups including one

<sup>18</sup> See Durand [3]. It is not clear whether utilities other than electric are included.

<sup>19</sup> The only other industry regressions which were recomputed were for chemicals and here the logarithmic results were quite close to the linear results.

presumably growth industry, we find again the same peculiar result obtained by our numerous predecessors using similar kinds of analysis—i.e., a predominant dividend effect.

One simple approach to holding firm effects constant (and in the process to reduce the problem of regression weights) is to add a lagged earnings-price ratio to the equations in Table 1. The results presented in Table 2 again indicate that dividends have a predominant influence on stock prices in the same three out of five industries but the differences

TABLE 2—REGRESSION EQUATION:  $P_t = a + bD_t + cR_t + d(E/P)_{t-1}$ 

Industry (sample size)	Regression Coefficients (standard errors)					$\bar{R}^2$
	$t$	$a$	$b$	$c$	$d$	
Chemicals ( $n=20$ )	1956	+58.21	+25.19 (1.69)	+13.81 (3.00)	−0.97 (0.14)	.967
	1958	+21.75	+26.93 (1.55)	+15.20 (3.95)	−0.45 (0.10)	.959
Electronics ( $n=20$ )	1956	+32.83	+15.78 (7.76)	+19.14 (5.02)	−0.49 (0.13)	.681
	1958	+54.59	+27.18 (8.14)	+28.06 (12.60)	−0.87 (1.57)	.836
Electric Utilities ( $n=25$ )	1956	+30.26	+12.42 (2.04)	+15.50 (2.89)	−0.41 (0.13)	.892
	1958	+41.59	+13.12 (2.32)	+14.88 (3.65)	−0.49 (0.10)	.897
Foods ( $n=25$ )	1956	+15.66	+13.68 (1.44)	+ 7.52 (1.16)	−0.17 (0.04)	.905
	1958	+16.93	+16.75 (6.27)	+ 5.68 (4.91)	−0.16 (0.07)	.843
Steels ( $n=20$ )	1956	− 6.39	+17.85 (2.78)	+ 2.21 (1.55)	+ .03 (0.07)	.870
	1958	+ 4.33	+15.06 (1.68)	+ 5.67 (2.17)	+0.05 (0.07)	.885

Note:  $E$  represents per-share earnings. See Table 1 for other symbols.

between the dividends and retained earnings coefficients are not quite so marked as in the first set of regressions. The dividends and retained earnings coefficients are closer to each other for all industries in both years except for steels in 1956, and the correlations are higher, again except for steels.

These new regressions, however, are not too satisfactory for a variety of reasons, of which the potential bias arising from short-run income disturbances is probably the most important. Moreover, an additional possible source of bias involves another question that was raised previously—i.e., the influence of stock price on dividend payout levels set by managements. Table 3 presents for four industry groups in 1958

TABLE 3—DIVIDEND SUPPLY AND DERIVED PRICE REGRESSIONS, 1958

Dividend Supply Equation: $D_t = e + fE_t + gD_{t-1} + h(E/P)_{t-1}$					
Industry (sample size)	$e$	Parameter Estimates			$\bar{R}^2$
		$f$	$g$	$h$	
Chemicals ( $n=20$ )	+ .0282	+ .0850 (.0320)	+ .8334 (.0416)	+ .0007 (.0017)	.995
Electric Utilities ( $n=25$ )	— .1163	+ .1440 (.0326)	+ .7989 (.0502)	+ .0024 (.0018)	.989
Foods ( $n=25$ )	+ .1836	+ .0735 (.0322)	+ .8435 (.0579)	— .0004 (.0013)	.962
Steels ( $n=20$ )	+ .6261	+ .1456 (.0329)	+ .6589 (.0868)	— .0027 (.0020)	.942
Derived Price Equation: $P_t = a + bD_t + cR_t + d(E/P)_{t-1}$					
Industry (sample size)	$a$	Parameter Estimates			
		$b$	$c$	$d$	
Chemicals ( $n=20$ )	+19.18	+27.02	+17.46	—0.45	
Electric Utilities ( $n=25$ )	+41.16	+11.02	+18.34	—0.46	
Foods ( $n=25$ )	+14.31	+17.65	+ 5.70	—0.16	
Steels ( $n=20$ )	— 1.06	+22.41	+13.81	+0.07	

the relevant dividend supply equations and the derived price equations obtained from the solution of the complete model, which contains three equations—the price equation (8), the dividend supply equation (9), and the identity (10). The derived price equations show no significant changes from those obtained from the single equation approach in Table 2, reflecting the fact that stock price, or more accurately the price-earnings ratio, does not seem to have a significant effect on dividend payout. On the other hand, it might be noted that, in three of the four cases tested, the retained earnings effect is increased relatively,

with no change in the fourth case. These results suggest that price effects on dividend supply are probably not a serious source of bias in the customary derivation of dividend and retained earnings effects on stock prices, though such a bias might be masked if the disturbing effects of short-run income movements are sufficiently great.

To provide some direct evidence on the potential bias arising from short-run income movements, the standard linear equation in Table 1 can be modified to include a lagged price variable which allows for slow short-run adjustment in prices to current levels of income. As noted earlier, to some extent the lagged price variable also holds firm effects constant; it also minimizes the problem of regression weights. These results are presented in Table 4.

Examination of this table shows that retained earnings receive greater relative weight than dividends in the majority of cases. The only exceptions are steels and foods in 1958. In all three groups which would normally be considered growth industries (chemicals, electronics, and utilities) the retained earnings effect is larger than the dividend effect for both years covered. For the other two industries (steels and foods) there no longer seems to be any significant systematic differences between the retained earnings and dividend coefficients, though there is some suggestion that dividends became relatively more attractive in 1958 than in 1956, which may not be surprising in view of the possible change in outlook for these industries at that time. This set of regressions has a number of attractive features apart from eliminating the customary anomalous result for dividends and retained earnings. The correlation coefficients as a whole have been substantially improved. The regression coefficients of the constant terms are in general close to zero, which is in accord with theoretical expectations, and the magnitudes of the lagged price coefficients seem generally sensible. Interestingly also, relationships for earlier years (1949, 1950, and 1952), identical in form to those in Table 4, show relatively weaker retained earnings effects than in the later years, a finding fully in accord with the changing emphasis on growth characteristics of stocks in this period.<sup>20</sup>

However, the regressions in Table 4 also have numerous limitations, including the possibility that the lagged price variable may serve in part as a proxy for dividends. Perhaps as a consequence, the regressions exhibit such undesirable properties as frequently negative dividend coefficients (though these are generally insignificant) and large standard errors for both dividends and retained earnings. These regressions, nevertheless, do seem to point to the weakness of the response of price to short-run changes in earnings, and to the fact that this response is not greatly affected by whether such changes in earnings are paid out

<sup>20</sup> It was possible to derive such regressions in the earlier years for all industry samples except the electronics.

TABLE 4—REGRESSION EQUATION:  $P_t = a + bD_t + cR_t + dP_{t-1}$ 

Industry (sample size)	Regression Coefficients (standard errors)					
	<i>t</i>	<i>a</i>	<i>b</i>	<i>c</i>	<i>d</i>	$\bar{R}^2$
Chemicals ( <i>n</i> =20)	1956	-1.52	-2.52 (3.78)	+6.36 (2.10)	+1.06 (0.12)	.979
	1958	-4.06	+3.75 (3.23)	+6.45 (2.78)	+0.88 (0.11)	.981
Electronics ( <i>n</i> =20)	1956	-4.92	-7.49 (3.05)	+5.18 (2.14)	+1.27 (0.09)	.953
	1958	+0.68	-5.84 (3.02)	+9.20 (1.74)	+1.13 (0.06)	.978
Electric Utilities ( <i>n</i> =25)	1956	+1.62	-8.19 (3.67)	+2.79 (2.75)	+1.30 (0.20)	.947
	1958	+2.49	-1.36 (2.06)	+3.84 (2.59)	+1.11 (0.11)	.963
Foods ( <i>n</i> =25)	1956	+1.74	+0.31 (1.84)	+2.56 (0.66)	+0.81 (0.09)	.967
	1958	-1.15	+1.75 (2.93)	+1.43 (1.08)	+1.08 (0.18)	.929
Steels ( <i>n</i> =20)	1956	+3.89	-0.17 (4.30)	+3.16 (0.98)	+0.88 (0.19)	.943
	1958	+0.94	+7.73 (1.55)	+3.34 (1.26)	+0.55 (0.09)	.964

as dividends or retained. There is some indication that retained earnings are more important than dividends (particularly for growth industries) but in half the cases neither the short-run dividend nor the retained earnings coefficient is significantly different from zero.<sup>21</sup>

Another and independent approach to the problem of short-run income movements which was described earlier is to normalize earnings by deriving time-series regressions of the form of equation (13) for each of the *i* companies in the *k*<sup>th</sup> industry group, and obtaining normalized retained earnings by subtracting dividends from normalized earnings, i.e., from  $[a_i + b_i d] (E/P)_{it} \cdot P_{it}$ . This normalization procedure was based on the period 1950-61, and prices were then related to dividends and normalized retained earnings for chemicals, foods, and steels in 1956 and 1958, with the results presented in Table 5. Subsequently, the

<sup>21</sup> Logarithmic regressions otherwise identical in form with those in Table 4 were computed for chemicals and electric utilities and showed very similar results to the linear relations.

TABLE 5—REGRESSION EQUATION:  $P_t = a + bD_t + cR_t^n$ 

Industry (sample size)	Regression Coefficients (standard errors)				
	<i>t</i>	<i>a</i>	<i>b</i>	<i>c</i>	$\bar{R}^2$
Chemicals ( <i>n</i> = 20)	1956	-6.37	+27.84 (2.66)	+10.96 (5.22)	.89
	1958	-5.87	+25.78 (2.58)	+18.82 (7.46)	.91
Foods ( <i>n</i> = 25)	1956	+3.00	+15.11 (1.28)	+ 3.83 (1.46)	.93
	1958	+2.20	+15.96 (1.40)	+ 4.91 (1.17)	.94
Steels ( <i>n</i> = 20)	1956	+0.34	+15.36 (1.41)	+ 4.85 (0.80)	.98
	1958	+6.11	+14.37 (1.29)	+ 8.24 (1.93)	.93

Note: The superscript *n* denotes normalized value. (See text.)

prior year's normalized earnings-price variable was also added to hold firm effects constant; these results are presented in Table 6.

A comparison of the results in Tables 5 and 6 with the corresponding regressions in Tables 1 and 2 shows the significant role of normalized earnings in eliminating part of the usual understatement of the relative importance of retained earnings, while a comparison of Tables 5 and 6 again indicates the similar role of a device (in this case, the normalized price-earnings ratio) holding firm effects constant. An examination of Table 6 shows that for the industry groups covered most, but not all, of the differences between dividend and retained earnings coefficients disappear when earnings are normalized and firm effects held constant. Similar regressions were not computed for the electronics sample since a sufficiently long time period for earnings normalization was not available, or for the utilities sample since a different and more satisfactory sample is being analyzed separately by one of the authors. However, it might be noted that for this somewhat different (and somewhat larger) sample of electric utilities, normalizing earnings and holding firm effects constant reduce the dividend coefficient slightly and increase the retained earnings coefficient fairly markedly.

The results of Table 6 seem considerably more plausible to us than the usual findings for foods and steels, suggesting that in these industries a somewhat (but not drastically) higher investor valuation may be placed on dividends than on retained earnings within the range of pay-

TABLE 6—REGRESSION EQUATION:  $P_t = a + bD_t + cR_t + d(E/P)_{t-1}$ 

Industry (sample size)	Regression Coefficients (standard errors)					
	<i>t</i>	<i>a</i>	<i>b</i>	<i>c</i>	<i>d</i>	$\bar{R}^2$
Chemicals ( <i>n</i> =20)	1956	+37.46	+25.33 (1.77)	+13.81 (3.62)	-621.53 (137.11)	.95
	1958	+26.64	+24.38 (1.95)	+19.29 (5.55)	-605.42 (157.64)	.95
Foods ( <i>n</i> =25)	1956	+21.78	+13.20 (0.57)	+ 9.22 (0.80)	-241.80 (23.51)	.98
	1958	+23.30	+13.26 (0.81)	+ 8.95 (0.81)	-227.40 (29.57)	.97
Steels ( <i>n</i> =20)	1956	+18.42	+12.10 (0.90)	+ 7.66 (0.70)	-164.26 (34.10)	.99
	1958	+34.82	+13.59 (0.87)	+12.19 (1.58)	-353.54 (79.34)	.96

out experienced so that management might be able to increase prices somewhat by raising dividends.<sup>22</sup> However, the regressions for chemicals in the table, though more satisfactory than those customarily obtained, do not seem quite so plausible since they imply the same type of result as for foods and steels.

A more detailed examination of our chemicals sample disclosed that the results obtained largely reflected the undue regression weighting given the three firms with prices deviating most from the average price in the sample of 20 firms. If these three firms are omitted, the results are changed substantially. As Table 7 indicates, retained earnings now become somewhat more important than dividends as a price determinant (again within the range of payout experienced).

In view of the possible bias in favor of the retained earnings coefficient that may be introduced by the earnings normalization procedure adopted, it was considered desirable to compare the time-series behavior of the relative earnings yield ratios previously discussed, i.e., equation (13), with that of relative dividend payout ratios obtained by deriving time-series regressions of the form

$$\frac{(D/E)_{it}}{(D/E)_{it}} = a_i + b_{it}$$

over the same time period. In view of resource limitations, this was done

<sup>22</sup> In this connection, it might be noted that foods and steels are characterized by lower payout than chemicals.

TABLE 7—REGRESSION EQUATIONS FOR 17 CHEMICAL COMPANIES

$$P_t = a + bD_t + cR^*_t$$

$$P_t = a + bD_t + cR^*_t + d(E/P)^*_t$$

Regression Coefficients (standard errors)					
<i>t</i>	<i>a</i>	<i>b</i>	<i>c</i>	<i>d</i>	$\bar{R}^2$
1956	+11.12	+10.56 (4.17)	+14.64 (3.79)		.81
1958	+ 6.42	+11.33 (4.28)	+16.63 (5.52)		.79
1956	+39.12	+12.14 (2.40)	+17.23 (2.22)	-539.32 (313.11)	.89
1958	+38.01	+12.18 (1.76)	+18.62 (2.27)	-553.87 (66.04)	.94

only for chemicals, which is considered the industry for which the customary results are most in question. The results in Table 8 show that in 12 out of 20 cases the time-slope coefficients for the relative earnings yield and relative payout regressions have the same sign, while in eight cases they are of opposite sign. This suggests that as relative payout increases, relative earnings yield increases somewhat more often than otherwise. In other words, the price-earnings ratio may have some tendency to move inversely to the payout ratio in contrast to the customary assertion of a direct relation. The correlation between the two slope coefficients is not very high but is significant ( $\bar{R}^2 = .543$ ). This is fairly strong evidence that the customary results are invalid, since the comparison of trends in relative yield and relative payout over time largely avoids both problems of short-run income fluctuations and problems of consistent firm effects. However, it should be noted that one difficulty may remain. If relative earnings yield goes down (i.e., relative price-earnings go up) because of a higher prospective rate of return on new corporate investment, the relative payout may go down because lower payout is associated with higher profit prospects, but the decline in payout would have no causative relation to the decline in yield. On the other hand, the customary finding that investors pay a premium for dividends as against retained earnings in the market even when the profitability of corporate investment opportunities is not held constant would still be invalidated.

#### IV. Some Concluding Remarks

Our analysis suggests that there is little basis for the customary view that in the stock market generally, except for unusual growth stocks, a

dollar of dividends has several times the impact on price of a dollar of retained earnings. There is some indication that in nongrowth industries as a whole, a somewhat (but only moderately) higher investor

TABLE 8—TIME-SLOPE COEFFICIENTS FROM RELATIVE EARNINGS YIELD AND RELATIVE PAYOUT REGRESSIONS FOR COMPANIES IN THE CHEMICALS INDUSTRY

Company	$b$	$b'$
1. Dupont	+0.014	+0.009
2. Union Carbide	+0.012	+0.015
3. Allied	+0.015	+0.016
4. Dow	+0.023	+0.019
5. American Cyanamid	+0.031	+0.036
6. Monsanto	-0.032	-0.014
7. Olin	+0.045	-0.022
8. Air Reduction	+0.026	+0.039
9. Koppers	+0.037	+0.039
10. Hercules	+0.017	-0.034
11. Texas Gulf Sulfur	-0.028	+0.012
12. Columbian Carbon	-0.038	+0.020
13. Hooker	+0.006	+0.016
14. Diamond Alkali	-0.010	+0.005
15. Pennsalt	-0.013	+0.002
16. Atlas Chem.	+0.019	-0.008
17. Commercial Solvents	-0.053	-0.072
18. Celanese	-0.128	-0.135
19. American Potash	+0.046	-0.002
20. Imperial Chem.	+0.038	+0.055

Note:  $b$  is the time-slope coefficient from the relative earnings yield regressions and  $b'$  is the corresponding coefficient from the relative payout regressions. (See text.)

valuation may be placed on dividends than on retained earnings within the range of payout experienced, but that the opposite may be true in growth industries. To the extent that this conclusion is valid, it is possible that management might be able, at least in some measure, to increase stock prices in nongrowth industries by raising dividends, and in growth industries by greater retention. However, the evidence that such possibilities exist to any important degree is rather tenuous, and there is no convincing indication of widespread management irrationality or irresponsibility in payout policy.

Unfortunately, the analysis we have carried out is limited not only in coverage of industries and time periods, but also in the linearity assumed. While the latter restriction can be justified on the grounds that a major objective was to question as expeditiously as possible the customary—and to us implausible—results obtained from similar mathematical forms, our results do not go very far in indicating what payout

ratios are regarded as optimal by investors for various types of stocks with different profitability of investment opportunities, risk, sources of financing, etc., or even in indicating whether an optimal ratio exists which to some extent is independent of profit prospects. Thus it would not surprise us if investors as a rule prefer at least a small nonzero (and preferably a stable or rising) payout, even at the cost of foregoing otherwise desirable investment. However, the further study of optimal ratios, while relatively simple in theoretical terms, involves much more complicated empirical analysis than has been attempted here.

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## THE INTEGRATION OF CAPITAL BUDGETING AND STOCK VALUATION

By EUGENE M. LERNER AND WILLARD T. CARLETON\*

It is widely recognized that a corporation's cost of capital cannot be determined until an analysis is made of how the market values the firm's common stock [5, p. 423] [6, p. 143]. There is, however, no widely accepted theoretical apparatus linking the market valuation of common stock to a corporation's investment-opportunities schedule, dividend payout function, and capital structure.

It is the position of this paper that a fundamental reason for the current stalemate over the theoretical apparatus is the single-equation nature of recent capital-budgeting and security-valuation models [3] [7] [11]. Since one equation can determine at most one unknown, manipulation of these models has, for the generation of results, necessitated a variety of *ad hoc* restrictions to reduce each equation to a relationship between *two* variables only—as, for example, between share price and capital structure.<sup>1</sup> Two consequences emerge: (a) Since there is no consensus, such restrictions tend to be different. (b) More importantly, such variables as share price, capital budget, dividend payout, and capital structure are in the real world jointly determined, and the suppression of this dependency unnecessarily limits the relevance of any theoretical results. That is, whether the problem is simply simultaneous determination of all the variables or maximization of one (e.g., share price) subject to one or more constraints, enumeration of the relevant relationships contained in the budgeting-valuation nexus must be explicit.

In this paper we depart from the single-equation convention and explicitly introduce two equations: an investment-opportunities (or capital-budgeting) schedule and a stock-valuation equation. Under reasonable assumptions these two equations are shown to determine

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<sup>1</sup> For example, see Lintner [7, pp. 250-51]. In order to generate the conclusion that investors will be indifferent to substitutions between elements in a corporation's time vector of dividends, it is necessary for Lintner to assume (among other things) given time vectors of earnings, capital budgets, and market discount rates.

simultaneously a corporation's internal rate of return, the percentage of earnings that it retains and the percentage that it distributes in dividends, and the price of its common stock. Furthermore, if the stock-valuation equation is treated as the corporation's objective function and the capital-budgeting equation as an internal constraint, we show that a unique maximum price can be found as a tangency solution.

The article falls into three major parts. Section I analyzes and modifies in two respects the stock-valuation model made popular by Myron Gordon in his *The Investment, Financing and Valuation of the Corporation* [3]. The Gordon model treats the price of a corporation's common stock as the present value of the expected future dividends, discounted at some given rate,  $k$ . Under the condition that the firm engages in no outside financing, retained earnings are reinvested by definition, so that a fixed relationship between dividend payout and capital budget emerges. A major difficulty of this model is that it generates unacceptable results when it is used as the sole determinant of the retention (hence, payout) rate which maximizes the value of the common stock. For example, the conclusion is reached that the corporation should retain and reinvest until the point at which the market value of stock equals book, or asset, value. The reason for anomalies of this sort is shown to be the implicit suppression of the mutual interaction within the firm of rate of return and size of capital budget. When a price map is drawn illustrating the substitutability of rate of return and dividend payout for shareholders at a given discount rate, the need for further specification of shareholder and corporation behavior becomes manifest. However, it is shown that even when  $k$  is treated as an increasing linear function of expected growth (in recognition of the growth-stock paradox) and the corporation's capital structure is formally introduced into the price model, the problem of what capital budget maximizes share price remains indeterminate.

Section II introduces an investment-opportunities schedule, positing the traditional inverse relationship between capital-budget size and internal rate of return. It then becomes obvious that there are two sets of rate of return and retention-rate expectations, one for the corporation and one for the shareholders. Under the necessary equilibrium condition that these two sets of expectations be equal, share price, dividend payout rate (and by construction, retention rate and capital budget), and internal rate of return are simultaneously determined. Finally, we demonstrate that there is a unique maximum share price attainable at some point along the corporation's investment-opportunities schedule. The more important implications of this tangency solution are: (1) a firm should not in general invest until internal rate of return equals the market discount rate; (2) most capital-budgeting "rules of thumb"

are likely to be suboptimal; and (3) the consequences of changing the dividend payout rate depend upon whether the firm is operating above or below the point of tangency.

Finally, in Section III the obtained results are evaluated in an equilibrium economics context, subject to amendment in a dynamic framework. In particular, the implied prescription that the corporation adopt shareholders' risk preferences is not unreasonable when the problem of disappointed expectations is suppressed (as is necessary in comparative statics). Such a decision rule might need to be modified for a dynamic world. Also, the introduction of capital structure as a variable to be determined is shown to require another equation, perhaps linking capital financing to dividend payout. The most reasonable context in which to do this may turn out to be an adjustment model in which expectations are generated over time.

### I. *The Stock-Valuation Equation*

#### *The Gordon Model*

The basic ingredients of any stock-valuation formula include: the particular stream of returns which is to be capitalized plus any time-dependent characteristics, such as a growth rate, that the stream possesses; the capitalization period (or investor time horizon); and the discount rate, or function. Academic debate over the proper definition of these ingredients has been exhaustive in recent years without being conclusive. The unfortunate fact is that normative results for corporate management depend rather strongly upon what definitions are adopted—whether the investor horizon is short-run or essentially infinite, for example. Rather than entering this terminological debate, we simply choose as a point of departure Myron Gordon's stock-valuation model [3, Ch. 4], which is a particularly well-developed and internally consistent approach to the valuation problem. Gordon defines investors' returns to be a growing stream of dividends, the market horizon as infinite, and the discount rate as a function, variously specified. He assumes no outside financing in his basic model, so that the establishment of a dividend payout automatically determines the capital budget as retained earnings.

Proceeding formally, assume that the corporation's dividends are expected to grow at a rate proportional to their present level,

$$(1.1) \quad \frac{dD}{dt} = gD,$$

and that, at time 0, the dividend payment is  $D_0$ . (Because new equity financing is excluded,  $D$  in the subsequent development can be thought of in terms of either a per-share or a firm basis.)

Assume further that the value of a dividend stream decays at a rate proportional to its existing value,

$$(1.2) \quad \frac{dV}{dt} = -kV,$$

and that at time 0 the value of the stream is  $V_0=1$ ;  $k$  can be taken as the market rate of discount.

The solutions of these two differential equations are

$$(1.1') \quad D_t = D_0 e^{gt},$$

$$(1.2') \quad V_t = V_0 e^{-kt}.$$

Since the price of a share is equal to the present value of the stream of all future dividends, equations 1.1' and 1.2' can be combined as follows:

$$(1.3) \quad P_0 = \int_0^{\infty} D_0 e^{gt} e^{-kt} dt$$

where  $t=0$  is now the date of valuation. A necessary condition for performing the integration is that  $k > g$ , for if  $k \leq g$ , the value of the expression equals infinity, the well-known problem of growth-stock valuation [2].

Solving equation 1.3 yields

$$(1.4) \quad P_0 = \frac{D_0}{k - g}.$$

The assumption that dividends grow at a constant rate is itself based on two assumed expectations; first, that the corporation is expected to earn a constant rate of return,  $r$ , on assets; and second, that the corporation is expected to retain a constant percentage,  $b$ , of its income. With the assumption that there is no debt in the capital structure of the corporation and that all growth of future dividends must come via retained earnings, equation 1.4 can then be rewritten in the form that Gordon gave it:

$$(1.4') \quad P_0 = \frac{(1 - b)Y_0}{k - rb}$$

where  $Y$  stands for income and  $g=rb$ , or as<sup>2</sup>

<sup>2</sup> There is an implicit assumption that  $g \geq 0$  and, hence,  $rb \geq 0$ . Negative values of  $g$ , the expected rate of growth of dividends, are possible, but they contradict the assumption either of a constant rate of growth or of no outside financing. For example, consider a  $g < 0$ . This means that  $r < 0$ , since  $0 \leq b < 1$ . However,  $r < 0$  implies a constant rate of deficit.  $(1-b)$  times a deficit is a negative expected dividend, or forced capital subscription, which contradicts the outside financing assumption. To allow outside financing only of this sort does not enhance the realism of the model. A heuristic resolution of the problem might be that the infinite-horizon model is an approximation, that the expectation of constant  $r$  and  $b$  is approximate, and that temporary negative  $r$  (and therefore  $g$ ) for a going concern is possible.

$$(1.4'') \quad P_0 = \frac{(1-b)rA_0}{k-rb}$$

where  $A_0$  stands for total assets.

### *The "Optimum" Retention Rate*

The model described in equations 1.4 and 1.4' suggested ways in which the question of the optimum dividend rate (or optimum retention rate) could be approached. Assume  $r$  is fixed, for clearly it is not independent of  $b$ . If the partial derivative of  $P$  with respect to  $b$  is set at zero, the price of the stock will be at a maximum if the second derivative is negative at this value. Taking the first partial derivative, it can be seen that  $\partial P/\partial b = 0$  when  $r$ , the internal rate of return on assets, equals  $k$ , the market rate of discount. Upon taking the second derivative, however, it is seen that  $\partial P/\partial b > 0$  only where  $r > k$ , and  $\partial B/\partial b < 0$  only where  $r < k$ . Stated differently, where the first-order condition,  $\partial P/\partial b = 0$ , is satisfied, the second-order condition cannot be because the second and higher derivatives disappear. Therefore, the conclusion is reached that if  $r$  is greater (less) than  $k$ , the price of the stock rises (falls) with a rise in retained earnings.<sup>3</sup>

Further implications can be derived from this unsuccessful attempt to find the optimum retention rate. Substituting  $k$  for  $r$ ,<sup>4</sup> its value when  $\partial P/\partial b = 0$ , in the denominator of equation 1.4' yields

$$(1.5) \quad P = \frac{(1-b)Y}{k-kb} = \frac{Y}{k}.$$

Thus, in a single-equation valuation model such as 1.4'', the capitalized value of a corporation's dividend stream equals the capitalized value of its earnings stream if the corporation continues to invest until the rate of return on assets equals the market rate of discount.

This should not be considered a surprising result, considering the fixed definitional relationship between earnings and dividends. That an earnings model and a dividend model may lead to identical valuation of share prices has been demonstrated by Gordon [3, Ch. 5], Lintner [7, p. 256], and Miller and Modigliani [5]<sup>5</sup> under various conditions and

<sup>3</sup> Gordon proceeded in this manner and went on to state, "A moment's reflection on the conclusion just reached with respect to the variation in share price with  $b$  reveals that a corporation should retain all of its income or liquidate depending on whether  $r \leq k$ " [3, p. 48]. He then speculated that this curious result probably stemmed from the assumption of independence of  $r$  and  $b$ , but with only one equation he was unable to follow the implications of this insight.

<sup>4</sup> Having developed present value valuation, subscripts from this point on refer to different values rather than different time periods.

<sup>5</sup> This study goes on to show that the valuation of shares will be identical not only under a dividend model and an earnings model, but also under a discounted cash-flow approach and the current earnings plus future investment-opportunities approach.

with different purposes in mind. What is significant is that  $P$  in equation 1.5 does not depend upon  $b$ , nor in fact does it depend upon  $r$  or  $k$ . If  $r$  is substituted for  $k$  in equation 1.4'', then at the point where  $\partial P/\partial b = 0$ ,

$$(1.5') \quad P = \frac{(1-b)rA_0}{r-rb} = A_0,$$

or the price of the stock equals the book value of the corporation (since the assumption of no debt was made). With  $r$  held constant and greater than  $k$ , the price of the stock commands a premium over book value and rises as  $b$  increases. If  $k$  is greater than a constant  $r$ , the price of the stock is less than book value and falls with a rise in  $b$ . While intuitively reasonable, the applicability of these results is limited because the manner in which they could be reached is unspecified. That is, if the retention rate is viewed as the firm's decision variable, it is reasonable to suppose that the value of the firm's stock would rise with an increase in its retention rate if internal opportunities remain greater than the shareholders' capitalization rate. On the other hand, the attempt of corporate management to proceed in its retention policy to the point where  $r=k$  yields the unacceptable conclusion of equation 1.5'. The difficulty lies in not spelling out how  $r$  and  $k$  might, in fact, change with changes in  $b$ . Plainly, shareholders' discount rate and the rate of return on assets are not invariant with respect to changes in dividends and reinvestment of earnings. Instead of holding  $r$  and  $k$  fixed, and then tracing the effects of varying  $b$  on  $P$ , a more useful way of working with equation 1.4'' may be to allow all four variables to change. As a first step, we continue to hold  $k$  constant and map the  $r, b$ , and  $P$  relationship.

If 1.4'' is written as

$$(1.4''') \quad r = \frac{Pk}{b(P-A) + A},$$

iso-price lines can be traced on a graph whose axes are  $r$  and  $b$ .

$P_2$  is higher than  $P_1$  and both are greater than the corporation's book value.  $P_{-1}$  is higher than  $P_{-2}$  and both are less than book value. Figure 1 illustrates that for a given  $r$ , say  $r_0 > k$ , the price of the stock rises as  $b$  increases from  $b_1$  to  $b_2$ . If  $r_0$  were less than  $k$ , the price would fall with increases in  $b$ . Moreover, for a given  $b$ , the price of the stock rises as  $r$  increases.

Figure 1 highlights the fact that all the iso-price lines approach  $k$  as  $b$  approaches 1.<sup>6</sup> It therefore follows that a given absolute change in  $r$

<sup>6</sup> Since  $k > rb$  in general, but for  $P > A$ ,  $k < r$ , it is clear that  $b$  can only approach 1 for  $k < r$ . The integrability constraint would be violated if this were not the case.

will lead to a larger price change, the higher the level of  $b$ . This can be seen analytically by considering  $\partial P / \partial r = (1-b)kA / (k-rb)^2$  in the limiting cases where  $b=0$  and  $b$  is close to 1. Phrased differently, stocks that have a high retention rate (low dividend payout rate) will fluctuate more in price for given changes in  $r$  than securities with a low retention rate. A similar conclusion was reached by Malkiel [8].

A shift in  $k$ , the market rate of discount, will change the location of the iso-price lines. For example, if  $k$  should fall as a result of a general decline in interest rates, and if  $r$  and  $b$  remain constant, some iso-price lines that formerly had positive slopes will now acquire negative slopes. Thus, if  $k$  changes, the shares of a corporation may sell at a premium over book value (or any other specified price) at one time and at a discount from this price at another, even though the corporation continues to earn the same internal rate of return and follows the same dividend policy. More importantly, a corporation whose present price line shifted from negative to positive slope, say, would find that the consequence of an increase in the retention rate was a fall in share price, whereas formerly a rise would have resulted.

The difficulty with Figure 1 and its supporting equation as they now

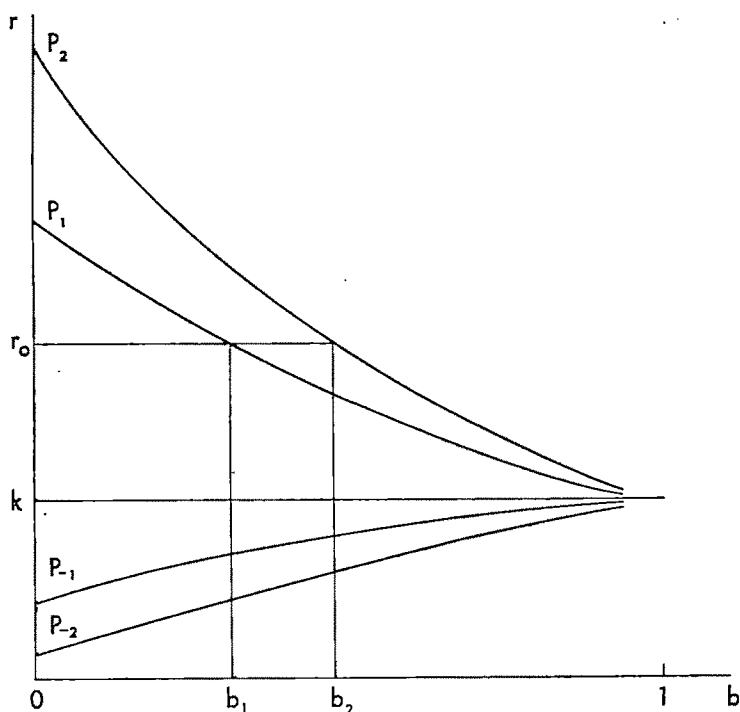


FIGURE 1

stand is that no provision is made for  $k$  to vary and no bounds are placed on the interaction of  $r$  and  $b$  within the firm; hence,  $P$  is indeterminate. Such also is the type of problem faced in the capital-budgeting decision. In that context,  $P$  is usually assumed to be given and the question is asked: "What rate of return must be earned by the corporation if  $P$  is to remain at its assumed level?" The more relevant question centers on the behavior of  $P$  under various combinations of  $r$  and  $b$ <sup>7</sup> and, more specifically, on the capital budget that will make  $P$  a maximum. Without internal constraints on the firm or specification of how  $k$  depends on  $r$  and  $b$ , the problem cannot be resolved. Our next step is to specify  $k$ .

### *Specifying $k$*

Except for the constraint that  $k$ , the rate of discount, be greater than  $rb$ , the rate of growth of dividends,  $k$  has not been specified. If for no other reason than to avoid the growth-stock paradox,  $k$  should be related to the firm's expected growth. More importantly from a practical point of view, as indicated above, the suggestion that  $k$  is a constant rather than a function leads to unacceptable capital-budgeting-decision results.

As a manageable form for such a function we propose:

$$(1.6) \quad k = \alpha + \psi rb$$

where  $\alpha$  is the market interest rate on growthless shares<sup>8</sup> and  $\psi$  is a risk class specification.<sup>9</sup>  $\psi$  is a constant which can assume values between 0 and 1.<sup>10</sup>

At this stage of the analysis there is some parallel between Modigliani and Miller's risk-class concept and our own. That is, a  $\psi$  class would be made up of corporations whose growth rates ( $rb$ 's) would be discounted in like fashion because in other respects they were similar. Since  $\psi$  has not yet been specified (except by its range of possible values) and since debt has not yet been introduced into the capital structure of the firm, principal sources of expected instability in the firm may in-

<sup>7</sup> One method of handling this problem was suggested by Cheng and Shelton [1].

<sup>8</sup> Under the assumption of an infinite stream of returns, yield to maturity equals current yield, a characteristic, for example, of consols. Because the dividend stream in this valuation model is discounted over an infinite horizon,  $\alpha$ , the market interest rate, is equivalent to a consol yield and might be approximated by the current yield on a growthless long-term security.

<sup>9</sup> Other forms for a risk-discount function are of course possible.

<sup>10</sup> This condition gives the limits of  $\psi$  when  $\alpha = 0$ . Since in the real world  $\alpha > 0$ , equation 1.3 could still be integrated for some  $\psi < 0$ . Of course, if  $\psi < 0$ , this would imply that future dividends are worth more than current dividends. Furthermore, although  $rb < 0$  gives rise to the logical problems of negative growth, as discussed in footnote 2, it is clear that with  $0 \leq \psi \leq 1$ , integrability conditions are not violated:  $k - rb > 0$ .

clude previous instability of growth due to industry competition, demand elasticity, vulnerability to technological change, and the like. The greater the degree to which these forces were operative in the past, the more likely, *ceteris paribus*, they are to be operative in the future. Put slightly differently, when such forces have been significant, there is a greater presumption that they will continue to be so. For example, a high expected growth rate for a firm is more likely to attract competition if barriers to entry have been low, and this possibility creates uncertainty with respect to the mean-value expectation of growth. Since Modigliani and Miller were interested primarily in the effect of leverage on the discount rate, they removed dividend payout from center stage by assuming it already decided in the stockholders' best interests [11, p. 266]. They then defined the discount rate as a linear function of leverage alone, suppressing the growth rate-discount relationship. We prefer to start at a more fundamental level and define  $k$  as a linear function of growth even before introducing the leverage problem. At one extreme a  $\psi$  value of 0 implies the uniform rate of discount we have already discussed. If  $\psi = 1$ , the rate of discount rises one-for-one with the expected rate of growth, and stockholders will pay no more for the high growth company than for the zero growth firm. As an example of the latter possibility, if the only source of growth was expected inflation of dividend dollars and shareholders insisted on evaluating their prospects in real terms,  $\psi$  would equal 1.

Substituting equation 1.6 into 1.4<sup>9</sup> yields

$$(1.7) \quad P = \frac{(1-b)rA}{\alpha + \psi rb - rb}.$$

The first partial derivative of  $P$  with respect to  $b$  equals 0 when  $r = \alpha / (1-\psi)$ , and once again the second derivative will be positive or negative depending upon whether  $r(1-\psi) \gtrless \alpha$ .<sup>11</sup> A graph of this expression would be similar to that of Figure 1 above with the difference that  $k$  would be replaced by  $\alpha / (1-\psi)$ . Similarly, substituting the value  $\alpha$  for  $r(1-\psi)$  in equation 1.7 yields

$$(1.8) \quad P = \frac{rA}{\alpha}.$$

Once again, with  $r$  a fixed number, there is a point ( $\partial P / \partial b = 0$ ) at which  $P$  is independent of  $b$ , or the dividend model produces the same price as an earnings model. More importantly, at this point the risk class falls out. This result makes intuitive good sense and justifies the form given the discount function for, if  $r$  is fixed and  $b$  has been set by dif-

<sup>11</sup> Again, under the assumption that  $dr/db = 0$  or that  $r$  is fixed.

ferentiation at the point at which it does not influence share price, then growth ( $r$ ) does not affect capitalization.

Equation 1.8 can also be rewritten as

$$(1.8') \quad r = \frac{\alpha P}{A}.$$

In this form the equation focuses attention on the rate of return that a corporation must earn on its assets if its shares are to maintain their existing price. Equation 1.8' also serves to put to rest the alleged "IBM paradox": Should not a corporation that sells at 50 times earnings invest in any asset yielding over 2 per cent? If  $\alpha$ , the market rate of interest on securities with no growth, equals 4 per cent, if the security sells at five times its book value, and if the retention rate is that which makes  $\partial P/\partial b = 0$ , then the required rate of return necessary to keep the price of the stock at its present level is 20 per cent, not any value greater than 2 per cent. As indicated earlier, however, equation 1.8' does not provide an answer to what rate must be earned to maximize  $P$ , let alone to what happens to  $r$  when  $b$  is determined.

A further interesting result can be obtained from equation 1.8'. When the firm's capital budget is set at  $\partial P/\partial b = 0$ , then  $\alpha = r(1 - \psi)$ , or

$$(1.9) \quad \psi = 1 - \frac{\alpha}{r} = 1 - \frac{\alpha A}{P}.$$

Since, under the assumption of an infinite shareholder time horizon,  $\alpha$  can be replaced by the current earnings yield, the  $\psi$  class of a security when  $\partial P/\partial b = 0$  will be given by:

$$(1.10) \quad \psi = \frac{P - A}{P}.$$

Note that  $\psi = 0$  when the price of the stock equals its book value.<sup>12</sup> As the price rises from book value,  $\psi$  increases. Equation 1.10, or some variant, has a long history of use among practitioners of security analysis and portfolio management. In the language of "The Street," risk rises when the security commands a substantial premium over book value. Indeed, one portfolio manager has gone so far as to state that when securities sell for more than six times book value (a  $\psi$  class of .83 under our assumptions), they no longer belong in a prudent man's portfolio [4].

<sup>12</sup> Since  $0 \leq \psi \leq 1$ ,  $P \geq A$ , which might seem to be an unnecessarily strong result. On the other hand, if  $P < A$  and the firm is doing the best it can, then it probably should liquidate. This conclusion is equivalent to Gordon's and follows from essentially the same situation as his case of  $r < k$ .

*Introduction of Debt into the Capital Structure*

Debt can be formally introduced into the single-equation valuation model as follows:

Let the change in assets in a continuous model be represented by the change in equity plus the change in liabilities:

$$(1.11) \quad dA = dE + dL.$$

The change in equity equals retained earnings:

$$(1.12) \quad dE = A r_L b,$$

where  $r_L$  is now the levered internal rate of return, or rate of return on assets less interest expenses imposed by the existence of debt. Let the change in debt be represented by  $dL = L\mu$  where  $\mu$  is some constant. If the ratio of debt to equity is assumed to remain constant,<sup>13</sup> then

$$\frac{dE}{dL} = \frac{E}{L} = \frac{A r_L b}{L\mu}.$$

Therefore,

$$\mu = \frac{A r_L b}{E}.$$

If  $z = L/A$ ,

$$(1.13) \quad \mu = \frac{A r_L b}{A - L} = \frac{A r_L b}{A - Az} = \frac{r_L b}{1 - z}.$$

Substituting 1.12 and 1.13 into 1.11:

$$\begin{aligned} dA &= A r_L b + \frac{L(r_L b)}{(1 - z)} \\ &= r_L b \left[ A + \frac{Az}{1 - z} \right] = \frac{A r_L b}{1 - z}, \end{aligned}$$

or

$$(1.14) \quad \frac{dA}{A} = \frac{r_L b}{1 - z}.$$

The percentage change in assets is precisely the term we have called  $g$  in equation 1.4, by virtue of equation 1.1' and the assumption that  $D = (1 - b)rA$ . Substitution of equation 1.14 into 1.4 yields a valuation model for a corporation with a fixed percentage of debt in its capital

<sup>13</sup> For an empirical justification of this assumption, see Lindsay and Sametz [6, Chs. 18 and 19], and Miller [9].

structure ( $k$ , however, is unspecified at this point):

$$(1.15) \quad P = \frac{(1-b)r_L A}{k - \frac{(r_L b)}{(1-z)}}.$$

Equation 1.15 suggests the following question: What is the optimum ratio of debt to assets for a corporation, abstracting from taxes? The first derivative of  $P$  with respect to  $z$  shows that so long as  $k(1-z) > r_L b$ , the price of the security will rise with a rise in the ratio of debt to total assets, and as  $k(1-z)$  approaches  $r_L b$ , the change in price associated with a change in debt approaches infinity. The assumption that  $k$  is independent of  $z$ , however, is not very useful. Indeed, one of the classic controversies in finance centers on the behavior of  $k$  over various ranges of  $z$ . Modigliani and Miller's famous proposition—in terms of equation 1.15—is, of course, that  $k$  rises linearly with  $z$  so that the average yield of the sum of debt plus equity remains constant. The more conventional position is that  $k$  does not change for low levels of  $z$ ; however, as  $z$  continues to rise,  $k$  rises faster than  $r_L b/(1-z)$ . Under this conventional view, the price of a stock, therefore, first rises and then falls as progressively larger amounts of debt are introduced into the capital structure.

An alternative behavioral assumption allows  $k$  to change with changes in the debt-asset ratio. Such a specification may be given by

$$(1.16) \quad k = \alpha + \psi r_L b + \phi z$$

where  $\phi$  is same constant greater than zero. Substituting equation 1.16 into 1.15 gives a stock-valuation model that incorporates debt:

$$(1.17) \quad P = \frac{(1-b)r_L A}{\alpha + \psi r_L b + \phi z - \frac{(r_L b)}{(1-z)}}.$$

For corporations that have no debt, it will be recalled that  $\partial P/\partial b = 0$  when  $r = \alpha/(1-\psi)$ , where the absence of a subscript means that  $r$  is unlevered. The change in  $P$  with respect to a change in  $b$  for corporations with debt equals zero when

$$(1.18) \quad r_L = \frac{(1-z)(\alpha + \phi z)}{1 - \psi + \psi z}.$$

If  $z=0$ , equation 1.18 is identical to the case of a corporation with no debt;  $r_L$  is the rate of return after interest that a corporation must earn on assets if the price of the stock is to remain constant with respect to a change in retention rates.

The treatment of debt, however, suffers from being essentially arbitrary. Moreover, the implication of debt in the capital structure has not been carried through to the stream of dividends. Even if these difficulties are accounted for, the retention rate which maximizes  $P$  is indeterminate.

## II. A General Model

In Section I we developed a stock-valuation model that specified  $k$  and incorporated debt as part of the capital structure. While useful insights are obtained from this model, its usefulness is limited. The principal kinds of questions that can be answered within the model's framework are of the sort: given a  $P$  and a  $z$ , what is the  $b$  which will just maintain the present  $P$ ? Of greater interest is a model which allows more logical "degrees of freedom." In this section we develop a set of two equations in  $r$  and  $P$ , one the stock-valuation equation, the other an investment-opportunities schedule. Simultaneous solution of these for some  $b$  yields  $r$  and  $P$ .

### *The LC Schedule*

The investment-opportunities schedule facing a corporation at a point in time can, with reliance upon traditional economic thinking on the subject, be taken as a function linking inversely the expected rate of return on total assets and size of capital budget.<sup>14</sup>

The development of a stock-valuation model invoked shareholders' expectations of  $r$  and  $b$ ; the present section deals with management's (expected) constraints on  $r$  and  $b$ . Apart from equilibrium requirements, and undoubtedly in the everyday world, the possibility exists that shareholder and management point estimates of  $r$  and  $b$  will diverge. It, therefore, becomes useful to introduce new notation:  $r_I$  and  $b_I$  to stand for investor (shareholder) expectations of rate of return and retention rate, and  $r_C$  and  $b_C$  to be expectations of the same variables held by the corporation (management).

With this convention, and recalling that the capital budget is defined identically by the corporation's earnings-retention rate,<sup>15</sup> a reasonable form for the investment opportunity schedule would be:

$$(2.1) \quad LC(r_C, b_C) = \gamma_0 \geq \frac{r_C}{1 + \gamma_1 b_C},$$

<sup>14</sup> The more common statement links the return on the marginal investment to the marginal investment. It is clear that our formulation is consistent, for if  $r$ , which is the average return on total assets, declines, then so does  $r'$ , the marginal return. The reason for our formulation rests on the fact that (from the stock-valuation equation)  $r$  rather than  $r'$  is the focus of capital-budget decisions designed to maximize share price.

<sup>15</sup> Depreciation is not considered a source of funds because we are interested only in net investment.

where  $\gamma_0(\geq 0)$  is the average rate of return expected when retained earnings are zero, and  $\gamma_1(<0)$  reflects the declining return associated with movement down an opportunities schedule as  $b$  increases. To show the nature of the functional relationship of  $r_c$  and  $b_c$ , inequality 2.1 may be written as

$$(2.2) \quad r_c \leq \gamma_0 + \gamma_0\gamma_1 b_c.$$

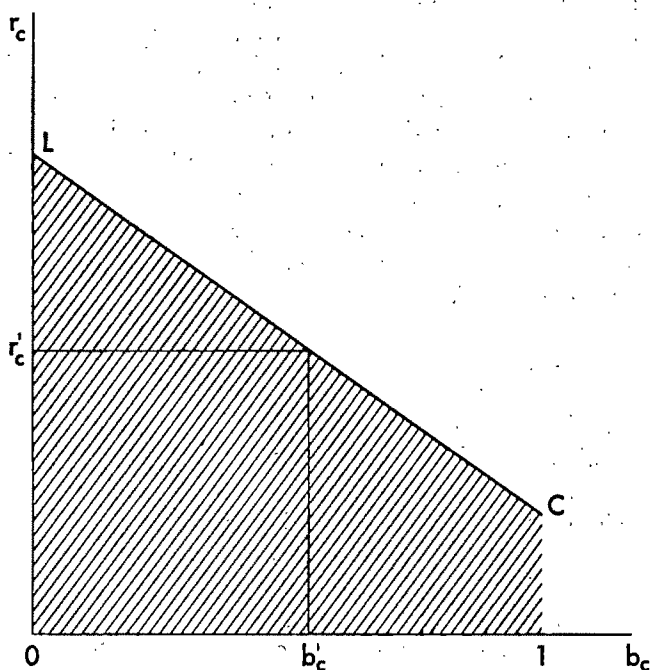


FIGURE 2

The shaded area in Figure 2 is feasible; for the efficient firm, 2.2 may be written as an equality:

$$(2.3) \quad r_c = \gamma_0 + \gamma_0\gamma_1 b_c.$$

It should be emphasized that this function is a set of equilibrium points, not a demand curve. For the corporation with no debt, no external financing, and no liquidating dividends, the domain of  $LC$  is from  $b=0$  to  $b=1$ . The choice of  $b_c$  (and, hence, of  $r_c$ ) remains indeterminate for the corporation until it is known which retention rate will, in fact, maximize share price.

### *The Complete Model*

Recall Figure 1 in which, from equation 1.4'',  $r$  was expressed as a

unique function of  $b$ , given  $P$  and  $k$ . In terms of this section:

$$(1.4'') \quad P = \frac{(1 - b_I)r_I A}{k - r_I b_I},$$

or

$$(1.4''') \quad r_I = \frac{Pk}{b_I(P - A) + A} = \frac{P\alpha}{b_I[P(1 - \psi) - A] + A}.$$

As with equation 2.3, it is clear that the valuation model is incomplete. To state the matter differently, we can fix  $P$  and  $b_I$  and find  $r_I$ , but how do we arrive at a  $P$ , much less at a maximum  $P$ ?

The answer lies in the simultaneous solution of equations 2.3 and 1.7. This can most easily be seen by tracing the steps from choice of  $b$  (the corporation's decision variable) to final determination of  $P$ . As a first step, it is necessary to impose the equilibrium conditions that

$$(2.4) \quad r_C = r_I$$

$$(2.5) \quad b_C = b_I$$

in order that there be no divergence in expectations. That is, shareholders accept the firm's retention-rate decision and agree with its assessment of the consequences on rate of return (whether or not they like it).

Then it can be seen that, with exogenous  $A$ ,  $\alpha$ , and  $\psi$ , equation 1.7 expresses share price as a function of  $r$  and  $b$  and that equation 2.3 is also a function in the same two variables. Once the firm chooses its retention rate,  $b$ , then  $r$  is determined from the latter function. However, substitution of the values for these variables in the stock-valuation equation 1.7 produces a unique share price.

The problem of suboptimum price arises in an equilibrium context because, although shareholders may agree with the corporation's assessment of its retention policy, they would be better off with some other policy that produced some other return, which might be either higher or lower. Solution of maximum price is somewhat more easily described with equations 1.4''' and 2.3. The maximum price then is given by the familiar tangency condition:

$$(2.6) \quad \left( \frac{\partial r}{\partial b} \right)_{\text{eq. 1.4'''}} = \left( \frac{\partial r}{\partial b} \right)_{\text{eq. 2.3}}.$$

Under the conditions assumed, only one  $P$ , a unique maximum, satisfies the condition that these two slopes be equal.

$$\left( \frac{\partial r}{\partial b} \right)_{\text{eq. 2.3}} = \gamma_0 \gamma_1 < 0$$

and

$$\left(\frac{\partial r}{\partial b}\right)_{\text{eq. 1.4}'''} < 0 \quad \text{for } P(1 - \psi) > A.$$

Furthermore, because the second partial derivative of  $r$  with respect to  $b$  is zero for the right-hand side and positive for the left-hand side of 2.6, for  $P(1 - \psi) > A$ ,<sup>16</sup> the appropriate convexity of the iso-price lines within the allowable range of the  $rb$  plane is satisfied.<sup>17</sup> Figure 3 de-

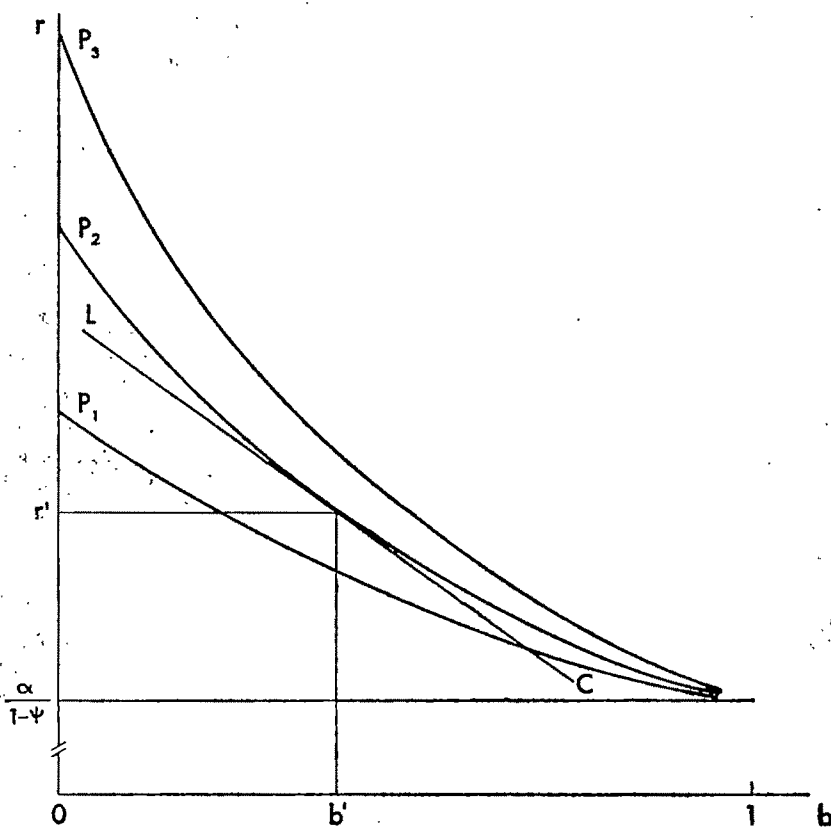


FIGURE 3

<sup>16</sup> It may be asked why this statement of a general model was not handled by the more familiar technique of Lagrange multipliers: maximize equation 1.4 subject to equation 2.1. While such a method yields the same results plus valid economic insights into the nature of the valuation *cum* capital budget problem, a comment is in order. In general  $b_0 \neq b_1$  and  $r_0 \neq r_1$ . Satisfaction of conditions 2.4 and 2.5 is necessary to secure simultaneous firm and market equilibrium *at any price*. Although the consequences of not meeting these conditions are best dealt with in dynamic models (and will be so handled in a forthcoming paper), we prefer not to suppress them this early in the game.

<sup>17</sup> We have drawn the  $LC$  function *above*  $\alpha/(1 - \psi)$ , indicating the firm has opportunity for

scribes the solution of the stock-maximization problem, where  $b'$  is the solution value of the corporation's decision variable.

The firm faces only one investment-opportunities schedule, downward-sloping, at a point in time, but an entire family of share prices. Without condition 2.6 an infinite number of prices are possible since the  $LC$  function intersects infinitely many iso-price lines. Condition 2.6 allows us to find a maximum price, i.e., where the slope of the  $LC$  function equals the slope of an iso-price line.

Maximizing profits, that is, carrying on investment projects until  $r = \alpha/(1-\psi)$ , is an inadequate criterion for either capital budgeting or dividend payout policy. In the case of Figure 3, investment to the point where  $r = \alpha/(1-\psi)$  (or equivalently until  $r = k$ ) would clearly not be optimal, for once the point of tangency with an iso-price line is passed, movement down the  $LC$  function toward  $b = 1$  produces lower and lower share prices as (implicitly) less-profitable projects are undertaken. Such a course would only be another example of the alleged IBM paradox: Investment to the point where  $r = \alpha/(1-\psi)$  (or  $r = k$ ) represents the undertaking of projects whose returns are not regarded by the market as valuable as using the funds to pay greater dividends.

This conclusion that firms should not in general invest until  $r = \alpha/(1-\psi)$  is admittedly at variance with traditional statements on the subject. Such statements have assumed either a single equation which took  $P$  as given or an infinitely elastic investment-opportunities schedule. Positing a downward-sloping investment-opportunities schedule and a separate valuation equation leads to the results described above.

Figure 3 and the supporting equations also illustrate the fallacy of the proposition that a firm should retain all earnings or distribute all earnings depending upon whether  $r \geq \alpha/(1-\psi)$ . The idea that such a policy could be relevant arises only because the requirements of internal (rate of return) and external (share price) equilibrium are not dealt

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an average internal return at least equal to the absolute required yield in the market adjusted for risk. For the firm whose  $LC$  function is below  $\alpha/(1-\psi)$ , the iso-price lines are concave downward, indicating that the best the firm can do is distribute all earnings; retaining and re-investing serves to lower  $P$ . As in Gordon's model, however,  $b$  can only approach 1. For  $r > \alpha/(1-\psi)$ , if  $b = 1$  the integrability constraint,  $r < \alpha/b(1-\psi)$ , is violated. The economic sense of this case—where the tangency is at or beyond the point where  $b = 1$ —is that a switch to external financing is called for. For  $r < \alpha/(1-\psi)$ , any  $b > 0$  lowers  $P$ . Hence the case of  $b = 1$  is not meaningful. Satisfaction of the second-order condition is guaranteed by the choice of a downward-sloping linear  $LC$  function. It therefore may be argued that our specification of the investment-opportunities schedule is fortuitous. On the other hand, other specifications would produce the same results, and the one chosen is consistent with economic tradition. It is interesting to note that the condition  $P(1-\psi) > A$  which is necessary for negative first derivative and positive second derivative is at variance with the results of equation 1.9 and 1.10 above, in which  $P(1-\psi) = A$  for  $\partial P/\partial b = 0$ . The reason for this difference is, of course, that the earlier result ignored the  $r, b$  relationship by considering  $r$  to be fixed.

with simultaneously. For the growing firm ( $P > A$ ), equilibrium at a maximum  $P$  is obtained only with an  $r > \alpha/(1-\psi)$  (i.e.,  $r > k$ ), which could require a  $b$  anywhere between 0 and 1. As discussed in footnote 17, for  $r < \alpha/(1-\psi)$ , the maximum obtainable  $P$  is at  $b=0$ .

This analysis also indicates the limitations of two other "rules of thumb" that allegedly characterize some corporate actions: adopting only those projects that yield more than a given rate of return and paying out a fixed percentage of earnings in dividends. If a corporation rejects all projects that yield less than an arbitrary set  $r_0$ , a dividend payment is dictated which is likely to be suboptimal with respect to share prices. Similarly, if a corporation adopts a fixed payout (retention) policy, the rate of return that will be earned may not maximize share prices.

A variety of possible situations are manageable within the comparative statics framework of this model. Consider first the effect of an upward shift in  $LC$ , the investment-opportunities schedule. The result is an increase in  $b$  and  $P$ , and possibly in  $r$ .

Consider now the selection of a retention rate higher than  $b'$ . Equilibrium for shareholders is possible now only at a lower  $P$  because the exploitation of too many internal opportunities lowers the average rate of return. Similarly, too low a retention rate will not maximize  $P$  because dividends are not valued as highly as some margin of the remaining internal opportunities. Securing equilibrium  $r$  and  $b$  together with a maximum  $P$  requires the firm to find a unique retention rate and its associated unique rate of return.

Consider finally the effect of a change in  $k$ . If, for example,  $k$  should rise because of a change in the demand for and supply of loanable funds (equivalently, if  $\alpha$  should rise), the iso-price lines will shift so that any given  $r$  and  $b$  will be associated with lower iso-price lines. With an unchanged  $LC$  schedule, the equilibrium  $P$  will fall. Moreover, the slopes of the iso-price lines will change for given rates of return. A new price line will now be tangent to the  $LC$  function and only by chance will the point of tangency be the same as where it was before the change in  $k$  occurred. The implication of this is that no single capital budget will be optimal under diverse external market circumstances.

### III. *Implications for Research*

In the previous section we defined a static equilibrium stock valuation-capital budgeting model. We demonstrated how  $r$  and  $b$  must vary if the price of a share is to be optimized under the assumption of a given  $k$  function and investment-opportunities schedule. We demonstrated the consequence of a shift in either the  $LC$  (investment opportunities) schedule, or  $k$ , as well as the consequences of a corporation's

choosing an  $r$  or  $b$  other than the one dictated by the tangency of the  $LC$  schedules and an iso-price line.

If the concept of the  $\psi$  class is introduced into the  $k$  function, the essential features of our equilibrium model are not altered. While the location and slope of the particular iso-price lines will change, the price of the security will still be optimized at the point of tangency between the  $LC$  function and the iso-price line.

The model assumed no ranking problems for investment projects: because  $r$  is the average return per year on total assets expected by firm and investor and is expected to be a constant, the related problem of finding the marginal profitability of projects having different time streams of returns is suppressed.

Risk was not introduced into the  $LC$  function because, arguing heuristically, the risk of the investor's discount function ( $\psi$  class) is the relevant consideration for firms trying to maximize  $P$ . Put another way, if firm and investors have the same mean-value forecast of  $r$ , the risk factor which influences  $P$  is that of the investors. Since the model presented in this paper is an equilibrium model, it cannot handle problems in disappointed expectations or consider measured variability as synonymous with risk. In an adjustment model,  $\psi$  might best be specified as a function of observed variance around  $r$ .

The introduction of debt as a variable, however, is more complicated. In this paper it was handled in an arbitrary manner and not treated as a corporate-decision variable. If debt is treated as a variable, another equation, linking debt to the dividend payout, is required. It also may be useful to consider debt in a dynamic context. In an adjustment model there will be an investor  $b$ -expectation function such as:  $b_t^* = b(b_{t-1}, b_{t-2}, \dots)$ . A decline in  $b^*$  will tend, *ceteris paribus*, to lower  $P$ . The question then arises: given an investment-opportunities schedule with tangency position at  $b > b^*$ , should the corporation borrow or reduce dividend payout? Quite possibly the former. Put another way, if investors do not discount increases in leverage too heavily (i.e., if  $\phi$  in equation 1.17 is not too large) and the opportunities schedule dictates asset growth, then maximum  $P$  suggests both borrowing and paying a  $(1 - b^*)$  dividend rate.

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## THE PRICE OF U.S. EXPORTS AND THE MIX OF U.S. DIRECT AND INDIRECT TAXES

By ROBERT Z. ALIBER AND HERBERT STEIN\*

It is sometimes said that U.S. products could be made more competitive in foreign markets if the United States relied less on direct taxes and more on indirect taxes.<sup>1</sup> The reason is that prevailing practice and GATT rules permit indirect taxes, but not direct taxes, to be rebated at the border. This paper explains how a change in the mix of U.S. taxes might improve the international competitiveness of U.S. goods, and gives a rough indication of the likely magnitude of the improvement in different circumstances.

In 1961, the components of \$1.00 of U.S. Gross National Product were of the relative magnitudes shown in column 1 of Table 1.

If (a) the corporate profits tax is repealed; (b) a new indirect tax of equal yield is imposed; (c) everything else—that is, wages, interest and other costs, and other indirect taxes, and *corporate profits after tax*—remains the same; (d) the new indirect tax is rebated on exports; (e) the components of the price of \$1.00 of exports, before the tax change, are the same as the components of \$1.00 of GNP; then: (a) the U.S. price level will remain unchanged<sup>2</sup> (column 2), and (b) the price of U.S. exports will decline by 4.4 per cent (column 3).

It is unlikely, however, that the components of the price of \$1.00 of exports are the same as the components of the price of \$1.00 of GNP.

\*The authors are members of the research staff of the Committee for Economic Development. The views of this paper are their own and do not necessarily reflect the views of the Committee for Economic Development or of any of its committees.

<sup>1</sup>This issue was one of several recently examined in the United Kingdom by Gordon Richardson, Henry Benson, and Sir Donald MacDougall. See *Report of the Committee on Turnover Taxation*, Her Majesty's Stationary Office. Cmnd. 2300, London, 1964, esp. pp. 75-77.

<sup>2</sup>The "U.S. price level" has three different meanings: (a) the price level of goods and services, domestic and imported, sold in the United States; (b) the price level of domestically produced goods and services sold in the United States; (c) the price level of U.S. output sold in the United States and abroad.

What happens to these three price levels in the conditions assumed here depends on the relative size of exports and of imports. Price level (a) will rise, because tax is shifted from sales abroad to sales at home. Price level (c) will decline because tax is shifted from exports to imports. Price level (b) will remain unchanged if exports and imports are equal. For the United States, with exports and imports and the difference between them small as percentages of GNP, the differences among the changes in these different "price levels" will be close to zero.

TABLE 1—COMPONENTS OF \$1.00 of GNP

	(1) Before Tax Change	(2) After Tax Change Total Output	(3) Exports
Wages, interest, and other costs, excluding corporate profits	\$ .824	\$ .824	\$ .824
Corporate profits after tax	.040	.040	.040
Corporate profits tax	.044	—	—
Existing indirect taxes	.092	.092	.092
New indirect tax	—	.044	—
	<hr/> \$1.000	<hr/> \$1.000	<hr/> \$ .956

It also is unlikely that "everything else" will remain the same if a new indirect tax is substituted for the corporate profits tax. The implications of these two points are considered in turn.

### *I. The Components of the Price of Exports and the Components of the Price of GNP*

The reason for doubting that corporate profits taxes are the same proportion of the value of exports as they are of the value of total GNP is that corporations probably do not produce the same proportion of U.S. exports as they do of total GNP. In 1961 the GNP attributed to production by incorporated business was \$250 billion. If the total value of exports were produced entirely by unincorporated business, the elimination of the corporate profits tax would not affect the prices of U.S. exports, on the assumptions made above. (The term "production of exports" refers to all stages, including extraction of raw materials, fabrication, transportation, and finance.) The imposition of the indirect tax would raise the domestic price of the kinds of goods that are exported, but the rebate of the tax on exports would leave export prices unchanged.

At the other extreme, if the total value of exports were produced exclusively by corporations, corporate profits taxes would be a much larger proportion of the value of exports than of total GNP. In 1960, corporate profits taxes were 8 per cent of the value of GNP *produced by corporations*. Thus, if all exports were produced by corporations, the amount of corporate profits taxes included in the value of the exports, and consequently the possible reduction in the price of exports as a result of eliminating the tax, might be 8 per cent.

It is unclear whether corporations produce a larger or smaller proportion of U.S. exports than they do of GNP. Unincorporated business accounts for most of agricultural production, and exports of agricultural products account for 20 per cent of U.S. exports. On the other

hand, government and some personal service industries that also are largely unincorporated account for a substantial portion of GNP and contribute little to the production of exports.

A helpful first approximation is that corporate profits taxes constitute about the same proportion of the value of exports as they do of GNP (4.4 per cent in 1961); the actual ratio of these taxes to exports is almost certain to fall within a range of from 3 per cent to 6 per cent. To facilitate the discussion, it is assumed that corporate profits taxes constitute 5 per cent of the value of exports.<sup>3</sup>

## *II. The Effects of the Change in the Tax Mix on the U.S. Price Level*

Thus far it has been assumed that "everything else" remains the same when the corporate profits tax is repealed and a new indirect tax of equal yield is imposed. The relevant issue is what happens to the U.S. price of GNP. The assumption that "everything else" remains unchanged is extreme; it lies at one end of a range of possibilities. If U.S. domestic prices do not rise as a result of the tax change, the price of U.S. exports will diminish by the amount of the rebate on exports that the change in the tax mix permits. The other possibilities generally have the effect of reducing the decline in U.S. export prices, and consequently the improvement in U.S. international competitiveness that might be expected from the change in the tax mix. At the other extreme, if U.S. domestic prices rise by the amount of the new indirect tax, the rebate of the tax will serve only to keep U.S. export prices constant and will not improve the competitiveness of U.S. exports.

The question of what happens to U.S. prices if the corporate profits tax is replaced by an indirect tax is usually considered as if it were identical with the question whether corporate taxes are "passed on in higher prices." It is argued that if corporate taxes are passed on, the elimination of this tax will reduce prices per unit of output by the amount of corporate profits taxes per unit of output. The imposition of an indirect tax of equal yield would then raise prices back to their initial level. On the other hand, if corporate taxes are not passed on in higher prices, the elimination of the corporate taxes will not reduce prices, but will raise corporate profits after tax. The imposition of the indirect tax then will cause a net increase in domestic prices.

This argument, however, does not answer the question of the effect of a change in the tax mix on export competitiveness. The general focus of most analyses of the incidence of the corporate tax—i.e.,

<sup>3</sup> This discussion assumes that, within the corporate sector, the rate of profit is the same on export sales as on domestic sales. To the extent that the rate of profit is lower on export sales than on domestic sales, the estimated reduction of export prices that would follow elimination of the corporate tax also is lower.

whether it is passed on in higher prices to others or borne by owners of corporate equities in lower incomes—is essentially on the effects on real profits or profit rates and of the *share* of profits in total income. Such analyses do not examine the impact of the corporate tax on the level of prices, the relevant factor for exports.<sup>4</sup> The “passing on” of corporate profits taxes is consistent with either changes in the price level or an unchanged price level, and so is the “absorption” of the corporate profits taxes.

Several possibilities are illustrated in Table 2. Column 1 presents a rounded-off version of the 1961 data as the initial situation.

TABLE 2—COMPONENTS OF PRICE PER \$1.00 OF OUTPUT

	Initial Situation	Pass-on Theory		Absorption Theory	
		Constant Prices	Higher Prices	Constant Prices	Higher Prices
Costs (mainly wages)	\$ .9000	\$ .9000	\$ .9474	\$ .8550	\$ .9000
Corporate profits after tax	.0500	.0500	.0526	.0950	.1000
Corporate profits tax	.0500	—	—	—	—
New indirect tax	—	.0500	.0526	.0500	.0526
U.S. Price	\$1.0000	\$1.0000	\$1.0526	\$1.0000	\$1.0526
Price of U.S. exports, after rebate of new indirect tax	\$1.0000	\$ .9500	\$1.0000	\$ .9500	\$1.0000

Column 2 reflects the initial assumption that a new tax is substituted for the corporate profits tax while everything else remains the same. U.S. prices remain constant, and export prices fall by 5 per cent. The impact of the elimination of the corporate profits tax has been passed on in the form of a reduction in prices, and the new indirect tax has been passed on as an addition to prices. (This also can be viewed as a case in which the corporate tax had been absorbed by profits and the indirect tax is now absorbed by profits.) Real corporate profits after tax remain unchanged.

Column 3 assumes that the initial effect of the shift in the tax structure is to raise prices. In some industries, for example, the amount of new indirect tax will exceed the amount of the reduction in corporate taxes, and their prices would tend to rise. While the reverse would be

<sup>4</sup>For example, the main test of the incidence of the corporate profits tax used by Kryzaniak and Musgrave in their recent study is the effect of the tax on the rate of return on corporate capital. See Marlan Kryzaniak and Richard A. Musgrave, *The Shifting of the Corporation Income Tax* (Baltimore 1963).

true in other industries, prices might not fall there, at least at once, because of downward rigidity, a rise in short-run marginal costs due to the indirect tax, and an effort to see whether the rise in after-tax profits can be made to stick. Thus there could be an initial increase of average prices and of corporate profits after tax if financial policy is permissive and permits an increase in the price level. Wages might then rise because of the rise in prices and in corporate profits after tax. The increase in wages would lead to higher prices in the noncorporate sector and absorb some of the initial increase in profits after tax in the corporate sector. Column 3 shows one possible point where this process might stop; at this point the elimination of the corporate profits tax has been completely passed on. *Real* corporate profits after tax are the same as in columns 1 and 2; the increase in corporate profits in money terms is proportional to the increase in the price level. So are "real taxes" and real "other costs." However, everything is at a higher price level, with the result that after the rebate of the new indirect tax, U.S. export prices are as high as before the change in the tax mix.

Column 4 assumes that corporate profits taxes have been absorbed in a reduction of corporate profits after tax and that the elimination of the tax will increase corporate profits after tax. If product prices are constant when the tax change is made and profits after tax rise, other costs, mainly wages, must fall. A shift from a corporate profits tax to a general indirect tax reduces the after-tax productivity of labor and raises the after-tax productivity of capital. Incorporated business will pay more for capital and less for labor than before the tax change.<sup>5</sup> If financial conditions are not permissive, so that the price level does not rise, the decline in the productivity of labor will result in higher unemployment until money-wage rates fall.

The result in column 5 also depends on a decline in the real productivity of labor as a result of the change in the tax mix. It differs from column 4 in that it is assumed that money-wage rates will not fall and that financial policy is permissive and permits an increase in the price level. In this case prices will rise enough to depress *real* wage rates to the level consistent with high employment. The price of U.S. exports will not decline.

It is conjectural which of these four results, or various intermediate possibilities, would follow the shift from the corporate profits tax to an indirect tax. The answer depends partly on the corporate response

<sup>5</sup> A roughly correct way of looking at this point is that before the tax change, if a business added \$1.00 to its labor cost, it added nothing to its taxes, while if it added \$1.00 to its equity capital cost, it added \$.50 to its taxes. After the tax change it adds approximately \$.05 to its taxes for each \$1.00 of additional cost of either kind.

to the change in the tax mix, partly on the response of labor to changes in prices, profits, and employment, and partly on the permissiveness of financial policy in response to changes in business costs.<sup>6</sup>

### III. *The Reaction in Foreign Countries to the Change in the U.S. Tax Mix*

That other countries have corporate taxes is relevant to this discussion for two reasons. First, it is usually said that the U.S. international competitive position is handicapped because direct taxes are

TABLE 3—CORPORATE TAXES AND DIRECT TAXES ON  
CORPORATIONS AS A PER CENT OF GNP—1961  
(general government)

	Corporate Taxes as a Per cent of GNP	Direct Taxes on Corporations as a Per cent of GNP <sup>a</sup>
United States	4.3%	4.4%
Belgium	NA	2.1 <sup>b</sup>
Canada	NA	4.4
France	2.3	2.4
Germany	3.2	3.2
United Kingdom	3.1	3.1
Japan	3.9	4.7
Netherlands	NA	3.4
Sweden	NA	2.5

*Sources:* Otto Eckstein and Vito Tanzi, "European Experience with Direct and Indirect Taxes and Their Significance for Economic Growth," prepared for National Bureau-Brookings Conference on the Role of Direct and Indirect Taxes in the Federal Revenue System, Oct. 1963; *Economic Survey of Japan*; United Nations, *Yearbook of National Account Statistics*; *Economic Statistics of Japan*.

<sup>a</sup> Includes taxes levied at regular intervals on profits, capital, or net worth, including corporate income and excess profits taxes, taxes on undistributed profits and on capital stock.

<sup>b</sup> 1960.

NA: Not available.

excessively high in the United States compared to the direct taxes in other countries. If this is the main point, it seems appropriate to "equalize" the U.S. corporate profits tax with those of other countries (whatever that means) rather than to eliminate the U.S. corporate tax. Second, even if the United States sought to obtain the maximum trade

<sup>\*</sup> It might also be noted that if foreigners buy \$16 billion of U.S. goods *produced by corporations*, they "pay" the corporate tax if the tax has been passed on. The tax burden on foreigners would decline perhaps by as much as \$300 million, if the corporate tax were repealed.

advantage from the elimination of the corporate tax, the possibility exists that other industrial countries would also eliminate or reduce their corporate tax.

Table 3 shows corporate tax revenues as a percentage of GNP in 1961 in the United States and in other large industrial countries.<sup>7</sup> While the U.S. effective corporate tax rate generally is higher than in most countries, the difference amounts to less than 2 percentage points of GNP.<sup>8</sup> If each country eliminated its corporate profits tax, and if the export prices of each country fell by the percentage that corporate taxes are of its GNP, then U.S. export prices would decline by no more than 2 per cent relative to export prices of major competitors of the United States.

Actually the ratio of corporate profits tax included in the value of exports to the value of exports is relevant rather than the ratio of corporate profits tax to GNP. These ratios may differ in other countries, as well as in the United States. The spread between the effective rate of corporate taxes on exports in the United States and in other countries may be smaller than is suggested by Table 3; it appears unlikely that it would be larger. Corporations probably contribute a larger proportion of the total exports of the Western European countries and Japan than they do of their GNP because agriculture provides a smaller percentage of their exports than of their GNP.

Moreover, if the tendency to "pass on" the elimination of the corporate profits tax is greater in other countries than in the United States, the decline in U.S. export prices relative to those abroad would be smaller, and conversely. If the elimination of the corporate tax were completely passed on in the United States, but absorbed in higher prices abroad, the U.S. export prices might decline by 5 per cent relative to those of other countries; in the reverse situation, U.S. export prices might rise relative to those of other countries by about the same amount.

<sup>7</sup> The 1961 data have been used because comparisons with other countries are not readily available for later years. It might be noted that U.S. corporate taxes declined from 4.4 per cent of GNP in 1961 to 4.2 per cent in 1963, and that they are expected to decline further—to less than 4 per cent—as a result of the Revenue Act of 1964.

<sup>8</sup> The small difference between corporate taxes as a per cent of GNP in the United States and other industrial countries may come as a surprise. Corporate tax rates in most industrial countries are in the range of 40 per cent to 52 per cent, so that, even as a per cent of corporate profits, U.S. corporate taxes in 1960 were not much higher than in other industrial countries. Thus a large part of the difference between the ratio of corporate taxes to GNP shown in Table 3 for the United States and for other countries is due to the larger proportion of GNP produced in the corporate sector in the United States. The basic point, however, is that the ratio of corporate profits to GNP is everywhere so small that even substantial differences in rates of corporate tax can result in only small differences in ratios of corporate taxes to GNP.

#### IV. *Conclusions*

The ratio of corporate profits to GNP is less than 2 percentage points higher in the United States than in other major industrial countries. If all countries abolished their corporate profits tax, or if the U.S. corporate tax rate were reduced so that corporate tax revenue relative to GNP were about the same in the United States as in these other countries, or if all countries were permitted to rebate corporate taxes paid on export sales (as they do for indirect taxes), U.S. prices would be unlikely to decline relative to export prices of other major industrial competitors by more than 2 per cent.<sup>9</sup> And because this tax shift may induce an increase in the domestic price level, the relative decline in U.S. export prices may be less than 2 per cent if the corporate tax is replaced by an indirect tax.

If the United States could act unilaterally and eliminate the corporate tax while other countries kept theirs, the improvement of the U.S. competitive position would be greater. The relative decline in U.S. export prices might be as much as 4 or 5 per cent if the U.S. price level remained unchanged. If, as seems more likely, the U.S. price level increased, the decline in U.S. export prices relative to price levels in other countries would be smaller.

\* The balance-of-payments impact of a shift from the personal income tax to an indirect tax can be analyzed with this same approach. There is a smaller presumption of a gain in U.S. export competitiveness, both because it is probable that a switch from the personal income tax to an indirect tax would raise domestic prices more than a switch from the corporate tax would, and because personal income taxes as a per cent of GNP are not much higher in the United States than in the major industrial competitors.

# PATTERN OF INCOME DISTRIBUTION IN AN UNDERDEVELOPED ECONOMY: A CASE STUDY OF INDIA

By P. D. OJHA AND V. V. BHATT\*

It is generally believed that the pattern of income distribution is more unequal in an underdeveloped economy than in a developed one.<sup>1</sup> Recently Simon Kuznets [3] collected the available evidence with regard to the income distribution pattern in some developed and underdeveloped countries, and this evidence seems to support the hypothesis about greater income inequality in the underdeveloped countries than in the developed countries. This evidence also includes the estimates on the pattern of income distribution in India.

The estimates on the income distribution pattern in the underdeveloped countries, however, are not very reliable, and the estimates about India, in particular, are based on highly inadequate data and involve some questionable assumptions [6] [7].<sup>2</sup> Recently, more adequate and reliable data and estimates relating to national income, consumers' expenditure, earnings of agricultural and industrial labor, population, saving, etc., have become available in the case of India; some of these data are not available even for some developed countries. Since India represents one of the largest underdeveloped economies, and since it is now possible to estimate fairly reliably the pattern of her income distribution, it would be interesting to find out whether the conclusion reached by Kuznets about the greater in-

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<sup>1</sup> See W. Arthur Lewis [4, pp. 226-28]. Lewis, of course, recognizes that it is difficult to generalize in this matter because of the differences among the underdeveloped countries as well as among the developed countries.

<sup>2</sup> The estimates by Mukherjee and Ghosh are based on very inadequate data. Lydall uses the income tax data as well as the consumers' expenditure data; however, he has not examined the reliability of consumers' expenditure data for any single year. Furthermore, his estimates are based on population projections that have turned out to be incorrect. Lydall combines the tax data and the consumers' expenditure data on the assumption that the Pareto "Law" of the distribution of incomes applies in India to the same extent as it does in most other countries. His data, incidentally, show far less concentration than the data of the Mukherjee-Ghosh study and thus do not fit Kuznets' general conclusions.

For a detailed reference to the available data and their limitations, see [8].

equality in income distribution in the underdeveloped countries than in the developed countries is borne out in the case of India.

It is the object of this paper to estimate the pattern of income distribution in India on the basis of all available data and to compare it with the pattern in some of the developed economies during recent years as well as during the earliest historical period for which estimates are available. This comparison shows, as will be seen in detail later, that the pattern of income distribution is less unequal in India than in some of the developed countries. Even the distribution of posttax income is only slightly more unequal in India than in the United Kingdom and the United States, and it is less unequal than in Denmark, Netherlands, and Sweden. For the earlier historical period for which estimates are available, the pattern in the developed countries was much more unequal than that which prevails at present in India.

In Section I, estimates relating to the pattern of income distribution in the rural and urban household sector as well as in the aggregate household sector in India are presented for the period 1953-54 to 1956-57.<sup>3</sup> The Indian pattern is compared, in Section II, with the patterns in some of the developed countries during recent years as well as for the earliest historical period for which data are available, and with some of the underdeveloped countries for a recent period. Some of the conclusions emerging from this study are presented in Section III.

### *I. Pattern of Income Distribution in India*

For the purpose of this study, the income-receiving unit is taken to be the household and not the individual.<sup>4</sup> This study attempts to present the pattern of income distribution in the household sector only, which comprises households, noncorporate businesses (including the agricultural sector), and private collectives like temples, educational institutions, and charitable foundations. The household sector thus includes all sectors other than the corporate and the government sector.

The household sector<sup>5</sup> is divided into three income groups: (a) households with an annual income equal to or below Rs. 3,000 (which is the income-tax-exemption limit), defined as belonging to the *low-income group*; (b) households with an annual income between Rs.

<sup>3</sup> These are fiscal years April to March.

<sup>4</sup> Pattern of income distribution based on individual income recipients would be misleading. See in this connection Titmuss [9, Chs. 2 and 4].

<sup>5</sup> The National Sample Survey (on the basis of which the estimates are made in this study) defines a household as "a group of persons usually living together and taking principal meals from a common kitchen." See [2, pp. 1-2].

3,001 and Rs. 25,000; and (c) households with an annual income above Rs. 25,000. The (b) and (c) households are described as belonging to the *high-income group*, while only the (c) households are referred to as the *top-income group*. The low-income-group households are subdivided into ten income groups corresponding to the consumers' expenditure brackets of the National Sample Survey. Thus in all there are twelve income groups.

The high-income-group households are divided into three functional groups: (a) salary-earner group; (b) nonsalary-earner group, including proprietors and partnership business concerns; and (c) farm household group. Incomes earned by private collectives are treated as belonging to the high-income nonsalary-earner group; this would introduce an upward bias in the estimate of the level of per-household income in this group. However, since the incomes of private collectives are not likely to form a significant proportion of the income of the nonsalary-earner group, no substantial error would be introduced in the estimate.

The household sector is also divided into (a) rural households and (b) urban households. The availability and the degree of reliability of the relevant data have largely determined the period—1953-54 to 1956-57—selected for this study. Again, in view of the quality of the available data, this period is divided into two parts; Period I relates to years 1953-54 to 1954-55, and Period II relates to years 1955-56 to 1956-57.

The method of estimation is described in detail in our article in the *Reserve Bank of India Bulletin* [8]. From that, it would be clear that a variety of sources has been used to obtain the required data. These different sets of data may not be mutually consistent, and they may have different degrees of reliability. Inevitably certain assumptions had to be made while combining these different sets of data. The results of this study, therefore, should not be taken as definitive; they indicate fairly reliably, however, the *structure* of income distribution among various income groups.

On the basis of the estimated pattern of income distribution, Lorenz curves were drawn. Table I is based on the Lorenz charts and gives income (both personal and disposable) accruing to the top 5 per cent of households and each decile group of households during 1953-54 to 1956-57.<sup>9</sup>

The top decile accounts for 28 per cent of personal income, while the bottom decile obtains only 3 per cent; the top 5 per cent of households accounts for 20 per cent of personal income. The distribution

<sup>9</sup> We are grateful to Mr. D. G. Vashi and Mr. T. R. Venkatachalam for drawing the Lorenz charts for us (not shown here) and for computing the concentration ratios.

TABLE 1—PATTERN OF INCOME DISTRIBUTION IN INDIA:  
SHARES OF ORDINAL GROUPS OF HOUSEHOLDS, 1953-54 TO 1956-57

Households	(Percentage)					
	Personal Income			Disposable Income		
	Rural sector	Urban sector	All India	Rural sector	Urban sector	All India
Top 5 per cent	17	26	20	17	24	19
Top tenth	25	37	28	25	36	28
Second tenth	14	12	14	14	11	14
Third tenth	13	10	13	12	11	12
Fourth tenth	9	8	9	10	8	9
Fifth tenth	9	8	8	9	8	9
Sixth tenth	8	6	8	9	7	8
Seventh tenth	7	6	6	7	7	6
Eighth tenth	6	6	6	5	4	6
Ninth tenth	5	4	5	5	4	5
Bottom tenth	4	3	3	4	4	3
Concentration ratio	0.310	0.400	0.340	0.306	0.382	0.335

pattern of disposable income is more or less similar to that of personal income. The concentration ratio<sup>7</sup> for disposable income is only slightly lower (0.335) than that for personal income (0.340); direct taxes seem to affect only the top 5 per cent.

Income distribution is more uneven in the urban sector than in the rural sector; the urban sector concentration ratio for personal income is 0.400, while this ratio for the rural sector is only 0.310.

<sup>7</sup> The ratio of the area between the Lorenz curve and the diagonal, representing equality, to the area under the diagonal; this ratio measures the departure of the Lorenz curve from complete equality, expressed as a ratio to complete inequality (which would be represented by the full right-angle triangle under the diagonal).

TABLE 2—PATTERN OF INCOME DISTRIBUTION IN INDIA:  
CHANGES IN CONCENTRATION RATIO BETWEEN PERIOD I<sup>a</sup> AND PERIOD II<sup>a</sup>

	Period I		Period II		Savings—Income Ratio <sup>b</sup> (Percentage)	
	Personal Income	Disposable Income	Personal Income	Disposable Income	Period I	Period II
Rural household sector	0.305	0.304	0.304	0.301	2.6	2.6
Urban household sector	0.378	0.368	0.421	0.405	12.5	19.5
All-India (household sector)	0.349	0.342	0.341	0.335	5.3	7.5

<sup>a</sup> Period I refers to fiscal years 1953-54 and 1954-55, and Period II refers to fiscal years 1955-56 and 1956-57.

<sup>b</sup> Source: [1]. This refers to the ratio of net saving to disposable income.

Though the concentration ratio for personal income declined slightly from Period I to Period II because of the change in the distribution pattern in the rural sector, the concentration ratio in the urban sector actually increased from 0.378 during Period I to 0.421 during Period II (Table 2). It was only in Period II that a substantial acceleration in capital formation under planned development took place [1], particularly in the urban sector, and this seems to have made the pattern of income distribution more uneven in the urban sector. Again, this increase in the concentration ratio in the urban sector is associated with a rise in the saving-income ratio in the urban sector and hence in the aggregate household sector; the saving-income ratio in the urban sector rose from 12.5 per cent during Period I to 19.5 per cent during Period II.

## II. *Income Distribution Pattern: A Comparison of India with Some Developed Countries*

The patterns of income distribution for recent years in five underdeveloped countries (including India), six developed countries, and Italy are shown in Table 3. The shares of the top 5 per cent and the

TABLE 3—PERCENTAGE SHARES OF ORDINAL GROUPS OF UNITS (HOUSEHOLDS OR TAX RETURNS) IN PERSONAL INCOME: SELECTED COUNTRIES

Countries and Year	Shares of Ordinal Groups				
	Bottom 20%	Bottom 60%	Top 20%	Top 10%	Top 5%
<b>Underdeveloped countries</b>					
India, 1953-54 to 1956-57	8.0	36.0	42.0	28.0	20.0
Ceylon, 1952-53	5.1	27.7	53.9	40.6	31.0
Mexico, 1957	4.4	21.2	61.4	46.7	37.0
Barbados, 1951-52	3.6	27.1	51.6	34.2	22.3
Puerto Rico, 1953	5.6	30.3	50.8	32.9	23.4
<b>Developed countries</b>					
United Kingdom, 1951-52	5.4	33.3	44.5	30.2	20.9
West Germany, 1950	4.0	29.0	48.0	34.0	23.6
The Netherlands, 1950	4.2	29.5	49.0	35.0	24.6
Denmark, 1952	3.4	29.5	47.0	30.7	20.1
Sweden, 1948	3.2	29.1	46.6	30.3	20.1
United States, 1950	4.8	32.0	45.7	30.3	20.4
Italy, 1948	6.1	31.2	48.5	34.1	24.1

### Sources:

1. United Nations [11, p. 29].
2. United Nations [10, Ch. 9, Table 3, p. 6].
3. Kuznets [3, Table 3, pp. 13-15].
4. U.S. Department of Commerce [12, Table 21, p. 85].

top 10 per cent in personal income seem to be similar in the underdeveloped countries (excepting Ceylon and Mexico) and in the developed countries; the share of the top 5 per cent in these countries ranges between 20 per cent to 25 per cent, while the share of the top 10 per cent is between 30 per cent and 35 per cent. Only in Mexico and Ceylon do their shares go beyond these ranges. It is, however, significant that in India the shares of the top 5 per cent and the top 10 per cent are the lowest among these 12 countries.

Similarly, though there are not very sharp differences between the shares of the bottom 20 per cent in the developed and the underdeveloped countries (excluding India), its share in India is the highest among the 12 countries.

The average concentration ratio in five underdeveloped countries (excluding India and counting Mexico twice) is higher (0.47) than that for six developed countries (0.42), indicating a more uneven distribution pattern in the underdeveloped countries (Table 4). However, the concentration ratio in India (0.34) is lower than in all the developed countries except the United Kingdom. Even with regard to the distribution of disposable (posttax) income, the concentration ratio in India is lower than those in the Netherlands, Denmark, and Sweden.

TABLE 4—PATTERN OF INCOME DISTRIBUTION IN SELECTED COUNTRIES:  
CONCENTRATION RATIOS

Country and Year	Personal Income	Disposable Income
Underdeveloped countries		
India 1953-54 to 1956-57	0.34	0.33
Ceylon 1952-53	0.42	0.41
Ceylon 1952-53	Above	—
Mexico 1950		
Mexico 1957		
Barbados 1951-52		
Puerto Rico 1953		
Developed countries		
United Kingdom 1951-52	0.33	0.29
West Germany 1950	0.45	—
The Netherlands 1950	0.45	0.41
Denmark 1952	0.44	0.40
Sweden 1948	0.44	0.41
United States 1950	0.35	0.32
Italy 1948	0.40	—

*Sources:*

1. United Nations [10, Ch. 9, Tables 3 and 12, pp. 6, 22].
2. Kuznets [3, Table 8, p. 28, p. 17; Appendix Table 2, p. 72].

TABLE 5—CHANGES IN THE PATTERN OF INCOME DISTRIBUTION IN SELECTED COUNTRIES:  
PERCENTAGE SHARE OF ORDINAL GROUPS, TAX UNITS OR CONSUMING UNITS,  
IN PERSONAL INCOME

Country and Year	Top 5%	Top 20%
1. United Kingdom 1880	48.0	58.0
1957	18.0	41.5
2. Germany 1913	31.0	50.0
West Germany 1959	18.0	43.0
3. Denmark 1870	36.5	—
1955	17.5	—
4. United States 1913-19	24.0	—
1944-48	17.0	—
5. Japan 1930	10.4 <sup>a</sup>	above 52.0
6. India 1953-54 to	20.0	42.0
1956-57	4.9 <sup>a</sup>	

<sup>a</sup> Share of top 0.2 per cent.

*Sources:*

1. Kuznets [3, Table 60, pp. 60-64].
2. Lockwood [5, Table 23, p. 272].
3. Ojha and Bhatt [8].

Of course, the disparity between the concentration ratio of the distribution of personal income and that with regard to disposable income is much wider in the developed countries than in the underdeveloped countries like India; this indicates the greater effectiveness and scope of the system of direct taxes in reducing the degree of inequality in income distribution in the developed countries.

The historical data on income distribution patterns in some of the developed countries are shown in Table 5 for the earliest period for which such data are available. These data indicate a much greater degree of inequality in income distribution in the developed countries at an earlier stage of their economic development than that which prevails at present in these countries as well as in India. Probably in the earlier stages of economic development, the pattern of income distribution becomes more uneven, and the trend towards greater equality establishes itself only after a certain stage in economic development is reached. If this is true, the distribution pattern in India may become more unequal as development proceeds; the rise in the concentration ratio in the urban sector in Period II may indicate this trend.

The picture with regard to the distribution pattern in India as compared to that in the developed countries is altered somewhat when a comparison is made for the rural/agricultural and urban/nonagricul-

tural sector separately. Table 6 gives these concentration ratios in the rural and the urban sectors in nine countries. The Indian concentration ratio in the urban sector is lower than that in Ceylon, Puerto Rico, and Poland, but is higher than that in Germany, Netherlands, Sweden, and the United States; this shows that the distribution pattern in the urban sector is more uneven in India than in the developed countries.

TABLE 6—PATTERN OF INCOME DISTRIBUTION IN THE AGRICULTURAL AND NON-AGRICULTURAL SECTORS IN SELECTED COUNTRIES: CONCENTRATION RATIOS

Countries and Year	Agricultural Sector	Nonagricultural Sector
1. India <sup>a</sup> 1953-54 to 1956-57	0.31	0.40
2. Ceylon <sup>b</sup> 1952-53	0.45	0.52
3. Puerto Rico <sup>c</sup> 1953	0.32	0.43
4. Poland 1929	0.35	0.50
5. Italy 1948	0.41	0.40
6. Germany 1928	0.19	0.35
7. The Netherlands 1954	0.22	0.29
8. Sweden 1959	0.36	0.39
9. United States 1950-53	0.41	0.34

<sup>a</sup> For India, agricultural sector refers to rural, and the nonagricultural sector to the urban sector.

<sup>b</sup> Agricultural sector includes hunting and fishing.

<sup>c</sup> Agricultural sector includes forestry and fishing.

Sources: Kuznets [3, Table 14, pp. 50-53].

In the rural sector, the concentration ratio in India is the lowest among the seven countries (except Germany and the Netherlands). Probably there is not as much difference in the distribution pattern in the rural sector as between the developed and the underdeveloped countries as there is between countries with large-scale and highly capital-intensive farm industry and those with small-scale and less capital-intensive farm industry.

In any case, the Indian data confirm the general trend in most of the countries towards a greater degree of inequality in income distribution in the urban sector than in the rural sector. Only the United States and Italy show a different picture. The estimates for the United States for earlier periods were in conformity with the general trend; the recent experience, as Kuznets suggests [3, pp. 53-54], may be due to a rise in the shares in total agriculture of the South and the West, both of which show wide inequality in the distribution of farm income, the former because of the cleavage between Negroes and whites, and the latter because of the cleavage between large-scale capital-intensive farms and smaller units.

### III. *Concluding Observations*

This comparative study of the income distribution pattern in India and some of the developed countries leads to the following tentative conclusions; these conclusions are inevitably tentative because of the possibility of a large degree of error in the estimates with regard to India as well as some of the developed countries.

1. The pattern of income distribution in India is more or less similar to the patterns in some of the developed countries and is more even than the patterns in some of the underdeveloped countries and the pre-1930 patterns in some of the developed countries.

2. However, the distribution pattern in the urban sector seems to be more uneven in India than in some of the developed countries, and the trend seems to be towards an increase in the degree of inequality in income distribution in the urban sector in India.

3. In the rural sector, there seems to be much less difference between the distribution pattern in the developed countries and the underdeveloped countries than that between countries with large-scale capital-intensive agriculture and those with less commercialized and less capital-intensive agriculture.

4. As in some of the developed as well as the underdeveloped countries, in India, too, the distribution pattern in the rural sector is more even than the pattern in the urban sector.

5. The generalization about greater unevenness in the distribution pattern in the underdeveloped countries than in the developed countries does not seem to be valid in the light of the Indian distribution pattern. It is true, however, that the system of direct taxes is more effective in the developed countries in reducing the degree of inequality in income distribution than it is in the underdeveloped countries; the disparity between the concentration ratios with regard to personal and disposable (posttax) income is much wider in the developed countries than in the underdeveloped countries.

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# CAPITAL THEORY AND THE RATE OF RETURN

## *A Review Article*

By EDWARD F. DENISON\*

The three lectures in the volume under review<sup>1</sup> consolidate and extend Robert Solow's previous examination of the relationship between capital and economic growth. What is crucial is to know the social rate of return on investment; this is the principal theme, and a sound one.

Readers of Solow's articles on capital and growth will surely wish to continue with this book. It is also a particularly good introduction to the discussion; prepared for oral delivery, the presentation seems easier to follow than his previous contributions. It would be an injustice, however, to imply that these lectures are merely one more contribution to the current debate over capital and growth; they are more than that. This review can scarcely suggest the breadth of the discussion or the elegance of the theoretical analysis and manipulation of simple models; suffice it to say that the whole volume is highly stimulating even where, as it seemed to the reviewer, it goes wrong.

The first lecture begins by asking why capital theory is in so unsettled a state. One cause is that capital has many forms and several characteristics, and different writers stress different aspects. There is no reason to suppose a "single object called 'capital' can be defined to sum up in one number" all the facts about time lags, gestation periods, and all the different kinds of capital goods. Solow's prescription for progress is to narrow the issues by concentrating on the causes and consequences of saving and investment. Because the consequences are so important, the "central concept in capital theory should be the rate of return on investment," which yields an unambiguous answer to "almost any important planning question . . . about the saving-investment process." That appraisal of the effect of investment on output requires some form of rate of return seems almost a truism. Yet much empirical analysis is based on capital-output ratios or incremental capital-output ratios. Solow's comment that the only way one can make any sense out of these ratios is to suppose they are meant as crude approximations to rates of return, and that, if that is indeed the intent, economists have an obligation to do better, is not unneeded.

Solow states that rates of return on investment can be calculated without measuring the capital stock. The point would be as important as Solow con-

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<sup>1</sup> *Capital Theory and the Rate of Return*. By Robert M. Solow. (Professor F. de Vries Lectures.) Amsterdam: North-Holland Publishing Co., 1963. Pp. 98. \$2.80.

siders it if it meant that the difficulties that haunt measurement of capital can also be avoided. This, unhappily, is possible neither in theory nor in statistical practice. Consider even the computation of a one-period rate of return in Solow's simplest models. Solow has a planning authority consider alteration of some initial plan by reducing consumption (and raising investment) in period 0 in order to raise consumption in period 1. Computation of the rate of return, he finds, requires only comparison of the additional consumption foregone in period 0 with the additional consumption made possible in period 1. But the latter is after depreciation; the capital stock must be left as well able to provide for future production as in the initial plan. The replacement of capital required to keep the stock at the end of period 1 at the level envisaged by the initial plan must therefore be known. Measurement of the additional consumption allowable in period 1 thus encounters every complexity faced in measuring the capital stock; they can be measured equally well—as in Solow's case of only one capital good which evaporates at a known constant rate—or equally badly in more complicated cases. When in Lecture 3 Solow computes a rate of return for the real world, he must also have a capital stock value to replace consumption foregone in period 0.

In Lecture 2 Solow introduces "technical progress," shows how to calculate rates of return in simple models in which progress is, and is not, embodied in structures and equipment, and states his preference for the embodied model. He also shows that if private and social rates of return are equal in the absence of technical progress, they will also be equal with technical progress, whether it is embodied or disembodied (unless one adopts the Arrow postulate "that technical progress arises out of experience and experience consists of gross investment"). In other words, "the mere fact of embodied technical progress does not itself drive a wedge between the private and social rates of return, if there were no wedge before." He argues, however, that in the embodied model the rate of return falls short of the conventionally calculated net marginal product of capital by obsolescence. Emphasis here must be on Solow's special conception of the term "conventionally calculated"; no modification of accepted theory or practice, which have always taken account of obsolescence, seems to the reviewer to be involved.

Lecture 3 calculates social rates of return at high employment in the United States and West Germany on the assumption of full embodiment of productivity increases in capital goods. Though results are presented with diffidence, caution, and alternatives, his preferred estimates for the United States are likely to be quoted and used. This reviewer considers (1) they are much too high on Solow's assumption that all technical progress is embodied, and (2) that the embodied model itself is unreasonable.

Solow's estimates for the United States refer, initially, to the private domestic business sector, excluding housing. He uses the Cobb-Douglas production function with all productivity change embodied in capital that he presented in his May 1962 *AER* paper. He calculates  $\alpha$ , the elasticity of GNP in this sector with respect to capital, for each of several assumed values of  $\lambda$ , the rate of quality improvement in capital. The former paper provided estimates for values of  $\lambda$  from .00 to .04; the present volume adds estimates of  $\alpha$  for  $\lambda$  equals .05. As  $\lambda$  goes up,  $\alpha$  goes down, and  $R^2$  goes up.

Solow says the proper  $\lambda$  in principle is that which provides the highest  $R^2$ , but all are so high that the only available criterion for choosing one value of  $\lambda$  rather than another is observed distributive shares. He views the  $\alpha$  of .36 obtained with  $\lambda$  at .05 as close to the property share in the relevant sector of GNP, and hence as satisfactory. The value of  $Q$ , potential GNP in this sector, is put at \$302 billion in 1954. At  $\lambda$  equals .05, the model throws off a 1954 value of \$309 billion for  $J$ , the value of structures and equipment when old capital is equated with new by its productivity. He computes a "gross" rate of return as  $\alpha Q/J$ , which equals .35. From this .04 is deducted for depreciation and .05 for obsolescence. Another .05 is deducted because  $\alpha$  includes the earnings of inventories, but  $J$  excludes their value. (No mention is made of the need for a similar, but larger, deduction for land.) This leaves .21 as the net rate of return on business capital at high employment in 1954. Inclusion of housing, he estimates, would lower it to .18. The housing reduction seems only half enough, but I shall examine only the 21 per cent rate of return excluding housing.

The actual share of capital obviously is a decisive determinant of the results. Solow, relying on the distribution of corporate gross national product, says "in reasonably prosperous years since 1929" employee compensation has amounted to 64 per cent of corporate gross product at market prices and 70 per cent at factor cost, and suggests that, the choice being uncertain, an  $\alpha$  between .30 and .36 would be appropriate. But surely market prices are irrelevant; there is no reason to count indirect taxes as capital income and little to think that they even fall disproportionately on property income. On the other hand, the corporate profits tax, treated in national accounts as a tax on profits, must in some degree (if only in regulated industries) be shifted. Hence the share of gross property income in corporate GNP at factor cost provides a maximum, not a minimum, estimate. This share in point of fact has not been as high as .30 since 1951. I think .29 is the highest one could take as representative of high employment in 1954.

If one relies on factor shares, there is no need to experiment with a range of  $\lambda$ s, as Solow does. The correct one is uniquely determined by the assumptions of full embodiment and a Cobb-Douglas production function. Full embodiment means that the increase in capital each year is, by definition, the amount that keeps output proportional to total factor input at high employment. The index of capital should be so measured every year, yielding identical indexes for output and input. Since Solow actually uses an average  $\lambda$  for all years, however, I illustrate the calculation by using average growth rates for the 1929-57 period, based on  $\alpha = .29$ .

Solow (p. 82) gives a growth rate of .5 per cent for man-hours. The increase in man-hours therefore contributed .5 times the .71 labor share, or .35 points to the growth rate of  $Q$ , which Solow gives as 3. This leaves 2.65 points as the contribution of capital. Working backwards, we can divide this by the capital share of .29 to obtain 9.1 per cent a year as the growth rate of quality-adjusted capital, Solow's  $J$ . In this model  $J$  *must* include land and inventories, since their earnings are included in  $\alpha$  and they are not counted as separate inputs. Solow gives the growth rate for conventionally measured structures and equipment as 2.1 per cent. We can obtain 1.8 as the weighted-average

growth rate for all conventionally measured capital if we take the growth rate of land as 0 and that of inventories as 1.8; allocate the gross capital share of .29 as .04 for land, .04 for inventories, and .21 for structures and equipment (my rough estimates); and use gross income shares as weights for the growth rates in computing the average. Since the average age of structures and equipment is almost the same in 1929 and 1957, we can subtract 1.8 from 9.1 and obtain 7.3 per cent (.073 as Solow would express it) for  $\lambda$ . This is the average rate of quality improvement for structures, equipment, land, and inventories combined. The rate of return implicit in the model is also readily approximated. Solow gives the dollar value of  $J$  only for values of  $\lambda$  up to .05, but by extrapolating his results we may estimate that, when  $\lambda$  is .073,  $J$  as he measures it, covering structures and equipment, is about \$245 billion. Goldsmith's estimates of nonresidential land and inventories in the relevant sectors are about \$125 billion and \$100 billion, respectively, so the total  $J$  is about \$470 billion.  $Q$  remains \$302 billion. Following Solow, depreciation is .04 and obsolescence, equal to  $\lambda$ , is .07, giving .11 of the value of structures and equipment for the two together. The high-employment net rate of return in 1954 from this model is therefore about  $[(.29 \times 302) - (.11 \times 245)] \div 470$  or .13, which compares with the .21 calculated by Solow. Over half the difference is due to Solow's ignoring land, and the rest to the net effect of his use of a higher  $\alpha$  and lower  $\lambda$ . The 13 per cent rate of return at *high employment* implied by full embodiment is not, perhaps, *prima facie* unreasonable for 1954, but I shall try to show that the full-embodiment assumption is.

Technical progress is supposed to be embodied only in structures and equipment. The  $\lambda$  of .073 computed from the model is lowered because it includes inventories and land; without them it would be at least .101. If the shares and growth rates cited in the previous paragraph are used, it can be readily computed that conventionally measured labor, land, and inventories together account for only .43 points in the growth rate, leaving 2.57 as the contribution of structures and equipment. With  $\alpha$ , referring to structures and equipment alone, at .21, division yields a growth rate for  $J$  referring to structures and equipment of 12.2 per cent. The growth rate of conventionally measured structures and equipment was 2.1, so  $\lambda$  for structures and equipment is 10.1 per cent (.101). But this in fact underestimates the  $\lambda$  implied by the assumption of full embodiment. The breakdown of property income given above distributed net earnings by asset values (separately within agriculture and other industries) with the net value of structures and equipment based on customary obsolescence rates. With  $\lambda$  at .101 or more, obsolescence would be greater, the current values of structures and equipment therefore much smaller,  $\alpha$  therefore less than .21, and  $\lambda$  therefore much greater than .101.

Both the fully embodied model and the usual calculations obtain the rate of return as the ratio of earnings to net stock. The fully embodied model implies a higher rate of obsolescence than presumably has been customary in business or national income accounting, hence lower values for both net earnings and net stock. Whether it yields a higher or lower rate of return depends on the composition of the stock; my calculations suggest that, not surpris-

ingly, any difference is not enormous. Nonetheless, the question of whether this is a useful approach to the rate of return rests on whether it is reasonable to suppose the rate of obsolescence is above 10 per cent a year on the value of all structures and equipment combined, including office buildings, hammers, and desks as well as computers. Solow originally carried his  $\lambda$  only up to 4 per cent and still has gone only to 5, probably not above the obsolescence rates implicit in some of the usual estimates. A rate above 10 must therefore not have seemed likely to him (the comparison seems legitimate since Solow discusses  $\lambda$  as referring only to structures and equipment though it actually includes other capital); the implied net value of structures and equipment, well under \$200 billion in 1954, seems implausibly low; and the underlying assumption that output per unit of conventionally measured input changes *only* from changes in the quality of capital goods is not really tenable. The knotty problems of measuring capital consumption and net stock cannot be solved by taking the extreme assumption.

Solow states he rejects private rates of return as usually calculated in favor of his production function because he is "after bigger game"—the social rate of return. But his production function, selected so as to make  $\alpha =$  the capital share, does not help him. All five sources of possible divergence between private and social rates of return that he lists refer either to the measurement of national product or to aspects of property income measurement other than obsolescence, and are handled identically in the usual income-stock ratio and in his production function.

Solow himself gives enough reasons why his German production functions, fitted to 1925-38 and 1950-57 data for the entire economy, cannot be satisfactorily compared with the American, and others can be inferred from what I have said. In addition, Rolf Krenzel's work shows that any production function using 1950-55 data that does not take account of the elimination of imbalances in the capital stock during this period could hardly be usable.

# COMMUNICATIONS

## A Graphical Proof of the Impossibility of a Positively Inclined Demand Curve for a Factor of Production

The mathematical impossibility of a positively inclined demand curve for a factor of production, under conditions of perfect competition in both the factor and product markets, has been irrefutably established.<sup>1</sup> However, to the mathematically unsophisticated, this theoretical conclusion remains somewhat mystical or at least unsubstantiated by a more graphic analysis. It is, indeed, difficult to reconcile this central theorem of economic theory with the salient (albeit anomalous) case of a positively sloped demand curve for a consumer's good.<sup>2</sup> And the layman's scepticism is not easily allayed by the common explanation that the firm's reaction to a factor price change involves nothing comparable to the consumer's income effect (which, when negative, makes the positive slope feasible); for the contention that the "only 'production effect' is something similar in character to the substitution effect" [1, p. 90] is not wholly accurate—the firm's "scale effect" (i.e., the effect exercised on the demand for a factor by a change in the level of output that is itself consequent to the factor price change) is, at least conceptually, analogous to the consumer's income effect. The crucial question that must be asked (and which has been answered mathematically) is not whether the theory of the firm involves something similar to the consumer's income effect (for it does), but whether this scale effect can be negative.

It is because of the incomplete understanding of the economic underpinnings and implications of this mathematical proof that the problem of positively sloped factor-demand curves is neglected in most nonmathematical presentations of microeconomic theory; and where such analyses have been attempted, they have been fallacious. A prime example of misconceived graphical expositions of the "law of factor demand" is that committed by Professor Hicks himself in *Value and Capital* [1]. In Figure 1 (similar to Hicks's Figure 20, p. 90) is drawn the production function for the case of one input,  $A$ , and one output,  $X$  (ignore, for the moment, the broken lines). The line  $K_1P$  has a slope equal to the ratio of the factor price to the product price; hence, equilibrium is given by the point of tangency of  $K_1P$  and the production function, point  $P$ . (At this point the price ratio equals the marginal product of  $A$ ; that is, the factor price equals the value of the marginal product of  $A$ .)

<sup>1</sup> Cf. J. R. Hicks [1, pp. 320-22] and P. A. Samuelson [2, pp. 76-78].

<sup>2</sup> Even in the case of consumer theory it is seldom realized that if the consumer has a convex preference-ordering, the demand curve for a commodity can only increase with a rising price over a limited price range. From its uppermost intersection with the price axis, the demand curve must initially have a negative slope and might then double back toward the price axis to accommodate a positive slope.

A decline in the price of  $A$  is represented by a diminution in the slope of  $K_1P$  to that of, e.g.,  $K_2P'$  and a new point of tangency at  $P'$ . Because of the convexity (from above) of the production function (which is a prerequisite for equilibrium in the first place), the quantity employed of the factor  $A$  must necessarily increase to accommodate the new equilibrium. This result is perfectly general (under the assumption of perfectly competitive markets) *for the case where only one factor is employed by the firm*. However, as a next

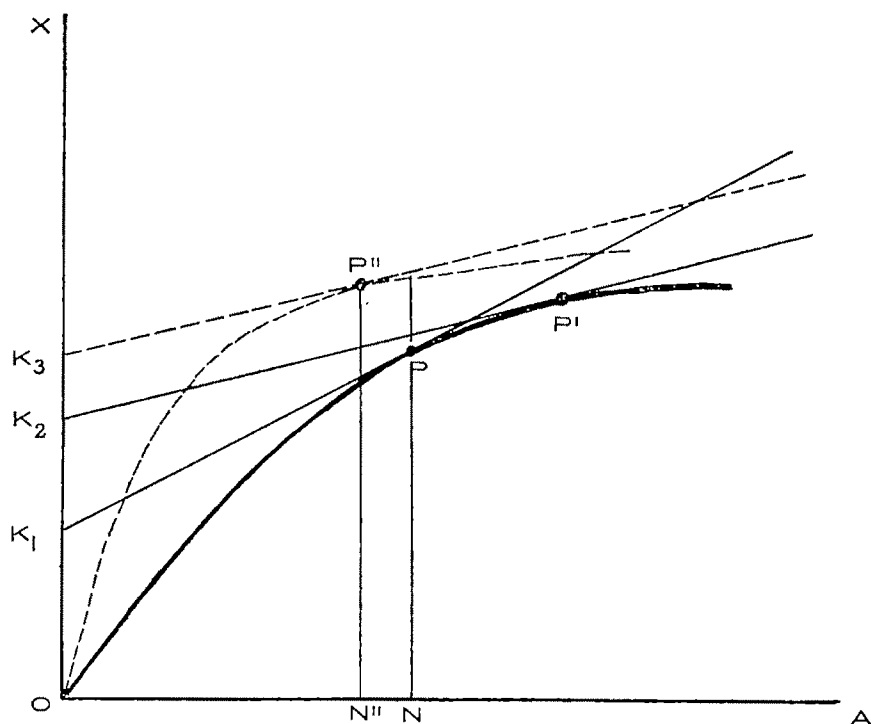


FIGURE 1

step in his analysis, Hicks [1, p. 93] supposes that "the firm produces one product  $X$ , and employs two factors,  $A$  and  $B$ . Then, since the relation connecting the amounts of factors and the amount of product still has the same sorts of properties as those to which we are accustomed, the demand for  $A$  must necessarily expand when its price falls." Abstracting from the mathematical appendix, this is a *non sequitur*. When there are two (or more) factors, there is a family of production functions for (any) one factor, each pertaining to a fixed amount of the other factor(s). Thus, the production function in Figure 1 is drawn on the assumption that the quantity of  $B$  is fixed. But it certainly does not make sense to assume that the quantity of  $B$  employed by a profit-maximizing firm will be unchanged when the quantity of both input  $A$  and output  $X$  are increased. Thus the new equilibrium will

probably be located on a different production function. And the textual analysis of Hicks does not negate the feasibility of a new equilibrium at  $\bar{P}'$  where  $K_s P''$  (parallel to  $K_s P'$ ) is tangent to the higher production function, and where the quantity of  $A$  has decreased (that this situation can be contrived on an isoquant map will be shown shortly). Obviously a more sophisticated graphical analysis is needed.

In the following analysis<sup>3</sup> we continue to assume perfect competition in both the factor and product markets. We also assume that the firm combines two factors of production,  $A$  and  $B$ , according to a production function which is representable by a convex set of isoquants, to produce one commodity (see Figure 2a). Labeled  $Y_1$ ,  $Y_2$ , and  $Y_3$  are iso-revenue curves (that is, isoquants valued at the given product price); the price line (or equal-cost line) labeled  $P_1$  has a slope equal to the negative of the ratio of the price of  $A$  to the price of  $B$  (the latter price also denoted  $P_1$ ). Equilibrium is, of course, given by the tangency point  $s$  where  $Oa_3$  and  $Ob_1$  of  $A$  and  $B$ , respectively, are employed. A decline in the price of  $A$  (with the product price and factor  $B$  price remaining unchanged) is represented by a diminution in the slope of  $P_1$  to, e.g.,  $P_2$ . If the firm were subject to an expenditure constraint, it would operate at point  $t$ , which clearly could be either to the left or right of point  $s$ . However, unlike the household, the profit-maximizing firm does not impose such a constraint upon itself; consequently, the lower marginal cost will induce the firm to expand its output<sup>4</sup> (and, under the assumption of perfect competition, the increase in revenue will be proportionate to the increase in output). Hence, the new equal-cost line will be higher than  $P_2$ , e.g.,  $P_2'$ .

From the knowledge contained in this diagram, we cannot say whether the quantity of  $A$  will increase or decrease; if the appropriate iso-revenue curve were  $Y_3'$ , then  $A$  would increase to  $Oa_4$ , but if the curve were  $Y_3$ , then  $A$  would decrease to  $Oa_2$ . However, the feasibility of this latter possibility is not established by this diagram because it does not represent the entire profit-maximizing process, but only depicts those conditions which must hold on the expansion path. The superstructure of the profit-maximizing process is represented in Figure 3 where the price of the factor is equated to the value of its marginal product,  $VMP_A$ . The horizontal lines represent the given prices of  $A$  corresponding to price lines  $P_1$  and  $P_2$  in Figure 2a. At the original price,  $P_1$ ,  $Oa_2'$  of  $A$  is purchased, and when the price is reduced to  $P_2$ , if the  $VMP_A$  curve remained stable, then  $A$  would increase to  $Oa_3'$ . However, the  $VMP_A$  curve is drawn on the assumption of a fixed quantity of input  $B$ , and it is doubtful that this latter quantity would be invariant with respect to changes in the quantity of  $A$  employed. Indeed, under the conditions for a positively inclined demand curve established in Figure 2a, it is obvious that the quantity of  $B$  purchased must increase; i.e., since point  $v$  must lie on the price line,  $P_2'$ , if it is to lie to the left of a vertical line through  $s$ , it must also lie above a horizontal line through  $s$ .

<sup>3</sup>Since the analysis is largely heuristic, perhaps the word "proof" should properly be eschewed in disclamation of mathematical rigor.

<sup>4</sup>This follows obviously from the assumption of a horizontal marginal-revenue curve and from the necessity (for an equilibrium) of an upward-sloping marginal-cost curve.

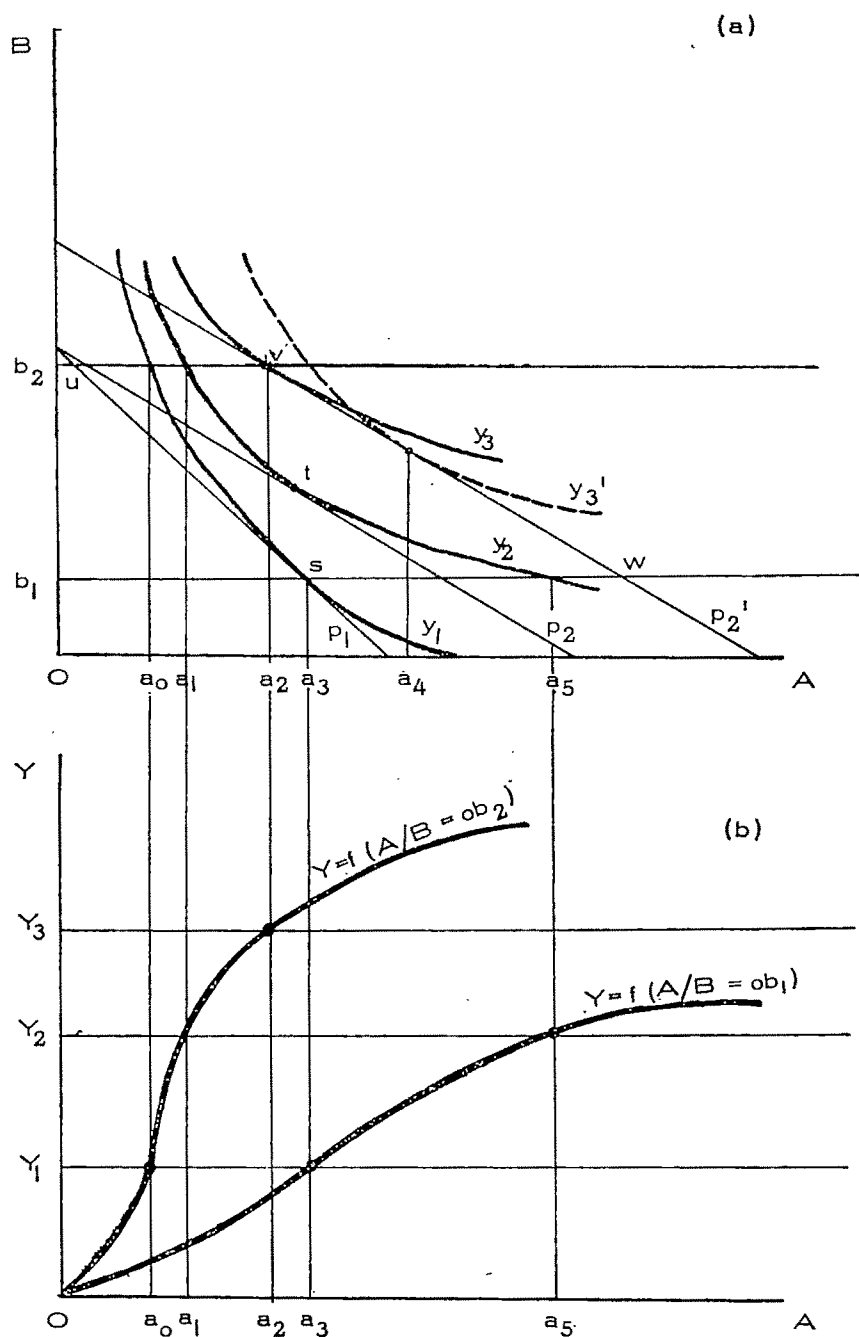


FIGURE 2

Most economists<sup>5</sup> assume that if the amount of  $B$  used in the production process is increased,  $VMP_A$  will increase. If this were universally true, then the new  $VMP_A$  curve would be represented by, e.g.,  $VMP_2$  in Figure 3, and the quantity of  $A$  purchased by the firm would invariably increase (to  $Oa_4'$  in Figure 3) as a result of a price decrease. However, in order to show that this assumption is not universally valid, it is only necessary to contrive an

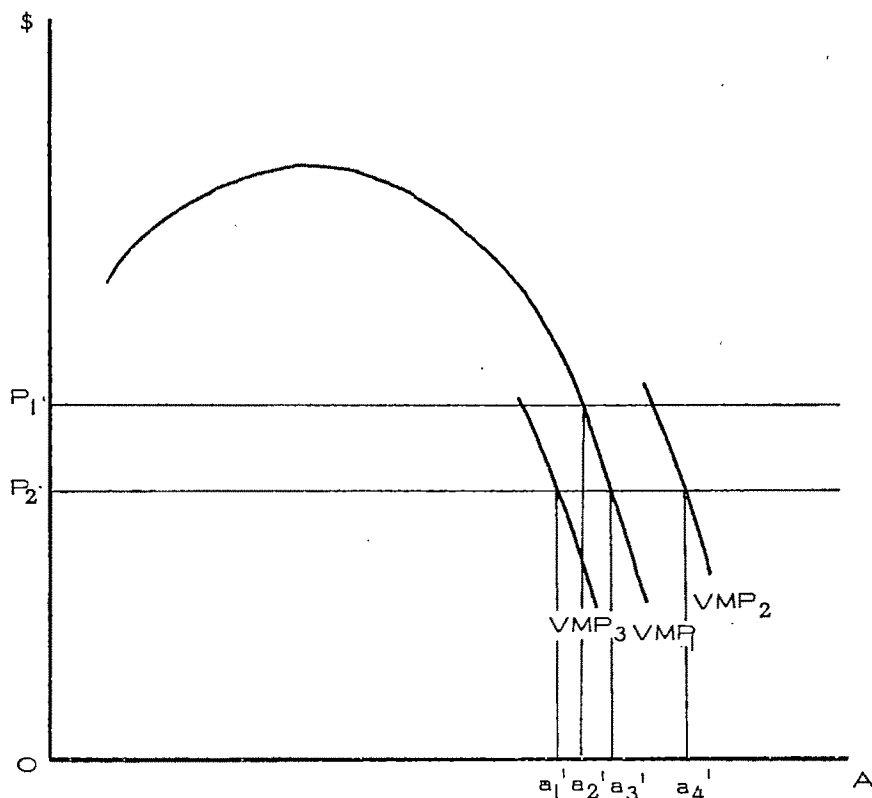


FIGURE 3

isoquant (or iso-revenue) map in which the level of the  $VMP$  curve of one factor is inversely related to the quantity employed of the other factor. In Figure 4a is constructed such a map; *over the relevant range of output*, the  $VMP_A$  curve corresponding to  $Ob_1$  of  $B$  is higher than the  $VMP_A$  curve corresponding to  $Ob_2$  of  $B$ . This point is clarified by the derivation, in Figure 4b, of two total-revenue curves respective to quantities  $Ob_1$  and  $Ob_2$  of  $B$ .<sup>6</sup> Since

<sup>5</sup> Cf. G. J. Stigler [3, p. 189].

<sup>6</sup> The iso-revenue map is drawn to yield total-revenue curves without diminishing returns to facilitate comparison of the slopes, but we could just as easily derive curves with diminishing returns.

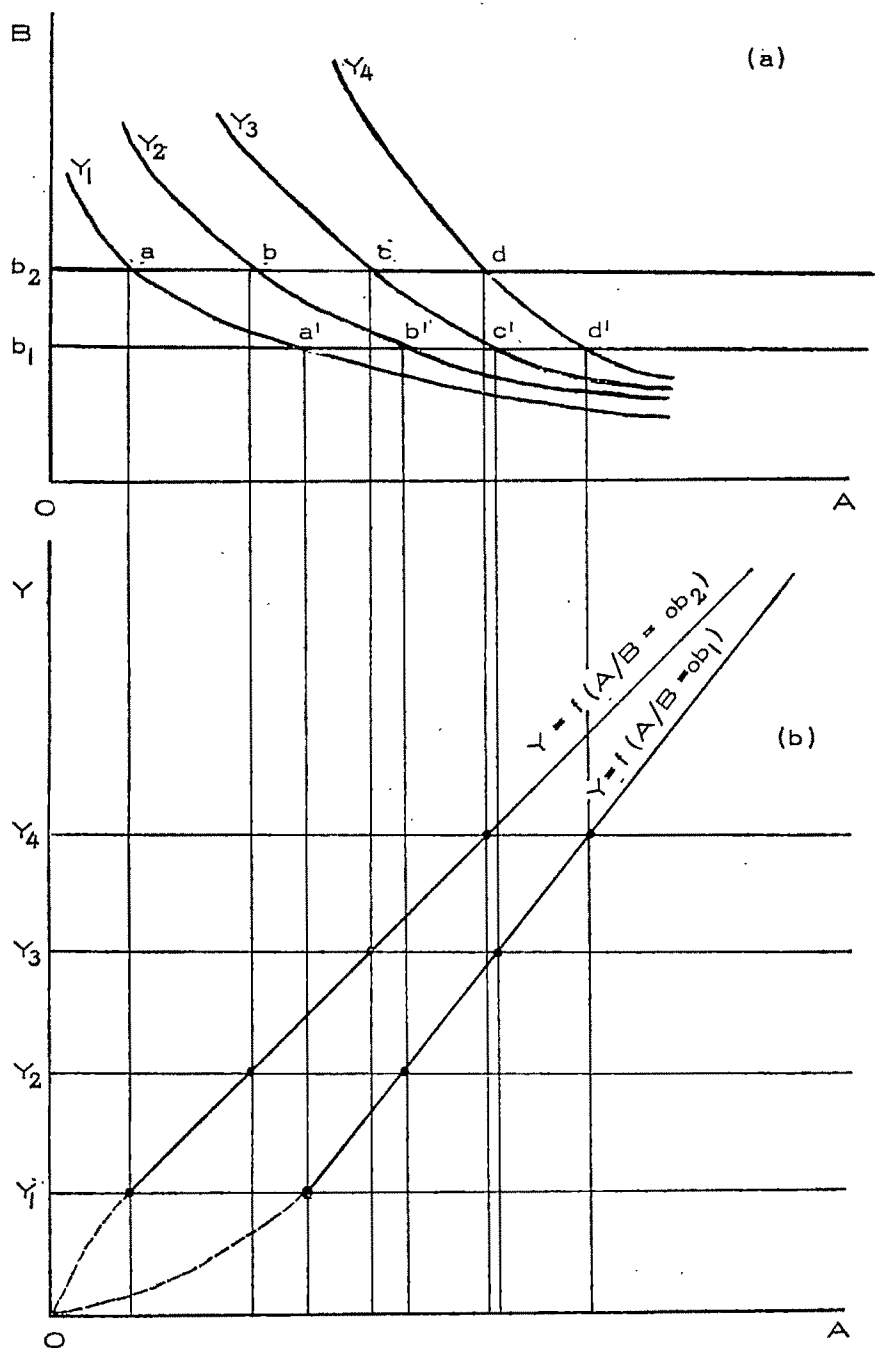


FIGURE 4

$ab > a'b'$ ,  $bc > b'c'$ , etc., the slope (i.e., the  $VMP_A$ ) of the total-revenue curve derived with  $B = Ob_2$  is smaller than the slope of the total-revenue curve drawn with  $B = Ob_1$ . Consequently, the profit-maximizing conditions in Figure 3 do not, in themselves, preclude the possibility that the lower price of  $A$  will lead to a shift in the  $VMP_A$  curve to, e.g.,  $VMP_3$  with a reduced quantity,  $Oa_1'$ , of  $A$  satisfying the new equilibrium.

Thus, independent examination of the expansion-path equilibria and the superficial profit-maximizing process has in each case failed to vitiate the feasibility of a positively sloped demand curve for a factor, but has in each case established conditions (manifested in the shape of the iso-revenue map) that must hold for such a phenomenon to occur. It is necessary now to ask whether these two conditions are compatible with each other. A cursory comparison will indicate that they are fundamentally contradictory and incompatible. For the total-revenue curves in Figure 2b, derived from the iso-revenue map in Figure 2a, reveal a steeper slope, or larger  $VMP_A$ , when  $B = Ob_2$  than when  $B = Ob_1$ ; and this, of course, is in sharp contradistinction to the case of Figure 4. We can easily show that any iso-revenue map which can accommodate a positively inclined demand curve necessarily implies that the level of the  $VMP$  curve of one factor is directly related to the quantity employed of the other factor by demonstrating that the horizontal distance between  $Y_1$  and  $Y_2$  in Figure 2a is greater when  $B = Ob_1$  than when  $B = Ob_2$ . When  $B = Ob_2$ , the *maximum* horizontal distance between  $Y_1$  and  $Y_2$  is  $uv$ . The *minimum* distance between these iso-revenue curves when  $B = Ob_1$  is  $sw$ , which must be greater than  $uv$  since the (absolute) slope of  $P_1$  is greater than the (absolute) slope of  $P_2$ . Hence, the minimum distance between the two curves when  $B = Ob_1$  is necessarily greater than the maximum possible distance when  $B = Ob_2$ . This conclusion, of course, requires our assumption of convex iso-revenue curves, and holds, *a fortiori*, under the case of strict convexity depicted in the diagram. Thus, the superficial profit-maximizing conditions require an isoquant map, or production function, that is fundamentally different from the production function required by the expansion path conditions—each has an unorthodox skew, but in opposite directions.<sup>7</sup> We conclude, then, that the demand curve for a factor of production, under conditions of perfectly competitive factor and product markets, must have a negative slope for all ranges of output.

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<sup>7</sup> These contradistinctive production functions might be interpreted as "*A-intensive*" (Figure 4) and "*B-intensive*" (Figure 2), respectively.

\* Although the author is currently assistant professor of economics at the University of California, Santa Barbara, this note was written while he was a graduate student at Harvard University.

### Professor Samuelson on Theory and Realism

In a discussion of "Problems of Methodology" at an AEA meeting [4], Paul Samuelson embarks on a critique of theories which employ unrealistic assumptions. He concludes with this strong indictment of "unrealistic, abstract models": "If the abstract models contain empirical falsities, we must jettison the models, not gloss over their inadequacies" [4, p. 236].

Let us first indicate how Samuelson reaches this judgment. He defines a theory "as a set of axioms, postulates, or hypotheses that stipulate something about observable reality [4, p. 233]. Denoting the theory as  $B$ , the "consequences" derived from it as  $C$ , and the "assumptions" antecedent to it as  $A$ , he argues that  $A$ ,  $B$ , and  $C$  must actually be identical in meaning, mutually implying one another. Thus, he holds, if the assumptions are empirically false, and the theory therefore unrealistic, the deduced consequences cannot possibly be empirically valid. In other words, an unrealistic theory cannot yield realistic consequences.

Samuelson also considers the possibility that the assumptions are *wider* than the theory, and that the theory is *wider* than the consequence, so that there is a subset  $C-$  of  $C$ , while  $A$  is a subset of  $A+$ . In this case  $A+$  may imply  $B$ , without  $B$  implying  $A+$ ; and  $C$  may imply  $C-$ , without  $C-$  implying  $C$ . If  $C-$  happens to be empirically valid, does this "validate" the wider theory  $B$ , or the even wider set of assumptions,  $A+$ ? Samuelson explicitly rejects this. He regards as "nonsense" the claim that the validity of  $C-$  justifies holding an unrealistic theory  $B$ , let alone the completely unwarranted set of assumptions  $A+$ .

What Samuelson does here is to reject *all theory*. A theory, by definition, is much wider than any of the consequences deduced. If the consequences were to imply the "theory" just as the theory implies the consequences, that theory would be nothing but another form of the empirical evidence (named "consequence") and could never "explain" the observed, empirical facts.

In addition, Samuelson errs in another way. We never deduce a consequence from a theory alone. We always combine the postulated relationships (which constitute the theory) with an assumption of some change or event and then we deduce the consequence of the *conjunction* of the theoretical relationships and the assumed occurrence. Thus, we do not infer  $C$  or  $C-$  from  $B$ , but rather from the conjunction of  $B$  and some occurrence  $O$ . If  $C-$  can be deduced from  $B$  *cum*  $O$ ; and if both  $O$  and  $C-$  are found to *correspond* to data of observation which can be regarded as the empirical counterparts (referents, proxies) of the theoretical  $O$  and  $C-$ ; then we rule that the theory  $B$  has sustained the test. This test does not prove that  $B$  is "true," but we have no reason to "jettison"  $B$ —unless we have a better theory  $B'$ .

Let us now leave aside the argument by which Samuelson reached his decision against "unrealistic, abstract models" and theories; let us, instead, confront Samuelson's judgment with Samuelson's pattern of theorizing when

he discusses, not methodology, but substantive propositions of economics. Let us choose the brilliant performance with which he demonstrated an important proposition in the theory of international trade.

In his ingenious papers on international factor-price equalization [2] [3], Samuelson shows "that free commodity trade will under certain specified conditions, *inevitably* lead to *complete* factor-price equalisation" [2, p. 181]. He admits that "it would be folly to come to any startling conclusions on the basis of so simplified a model and such abstract reasoning," but he submits—very rightly, in my opinion—that "strong simple cases often point the way to an element of truth present in a complex situation" [2, p. 181].

What are his assumptions, hypotheses, conditions? Here is the list:

1. There are but two countries, America and Europe.
2. They produce but two commodities, food and clothing.
3. Each commodity is produced with two factors of production, land and labour. The production functions of each commodity show "constant returns to scale". . . .
4. The law of diminishing marginal productivity holds. . . .
5. The commodities differ in their "labour and land intensities". . . .
6. Land and labour are assumed to be qualitatively identical inputs in the two countries and the technological production functions are assumed to be the same in the two countries.
7. All commodities move perfectly freely in international trade, without tariffs or transport costs, and with competition effectively equalizing the market price-ratio of food and clothing. No factors of production can move between the countries.
8. Something is being produced in both countries of both commodities with both factors of production. . . .

From this he concludes: "Under these conditions, real factor prices must be exactly the same in both countries (and indeed the proportion of inputs used in food production in America must equal that in Europe, and similarly for clothing production)" [2, p. 182].

In his "intuitive proof" he goes so far as to state this: "I have *established unequivocally* the following facts:

Within any country: (a) a high ratio of wages to rents will cause a definite decrease in the proportion of labour to land in both industries; (b) to each determinate state of factor proportion in the two industries there will correspond one, and only one, commodity price ratio and a unique configuration of wages and rent; and finally, (c) that the change in factor proportions incident to an increase in wages/rents must be followed by a one-directional increase in clothing prices relative to food prices" [clothing being the more labor-using commodity, food the more land-using commodity] [2, p. 187].

It may be fair to state that Samuelson had characterized the problem as "a purely logical one" [2, p. 182]. But he sometimes uses language of empirical operations, for example, when he speaks of "observing the behaviour of a representative firm." It should be clear, however, that what he "observes"

is merely the logical consequence of a set of assumptions; that the "behaviour" is purely fictitious; and that his representative firm is only an ideal type, a theoretical construct. Let me quote the sentence: "... if we observe the behaviour of a representative firm in one country it will be exactly the same in all essentials as a representative firm taken from some other country—regardless of the difference in total factor amounts and relative industrial concentration—provided only that factor-price ratios are really the same in the two markets" [2, pp. 187-88, emphasis supplied].

At the end of his discussion Samuelson evaluates some important qualifications which he finds help to "reconcile results of abstract analysis with the obvious facts of life concerning the extreme diversity of productivity and factor prices in different regions of the world" [2, p. 196]. These "qualifications" to the theorem furnish Samuelson with the "causes" of the factor-price diversities. In other words, he does not hesitate, quite rightly in my view, to explain the observed facts of life—factor-price differentials—by the divergences of real conditions from the ideal ones which form the basis of the factor-price equalization theorem.

Would the Samuelson of the *A-B-C* argument against unrealistic, abstract models approve of the Samuelson of the intuitive proof of the factor-price equalization theorem? Frankly, I do not know. Perhaps both Samuelsons make a distinction between a theorem and a theory, meaning by the former a proposition deduced from counterfactual assumptions and postulates, and by the latter a proposition stipulating something about observable reality. But the Samuelson of the *Foundations of Economic Analysis*, who preceded both other Samuelsons, did pledge allegiance to a program emphasizing "the derivation of operationally meaningful theorems" [1, p. 3].

Since, according to Samuelson, a theorem deduced from counterfactual hypotheses cannot yield empirically true consequences, and does not contain operationally defined terms, it is not immediately clear just what an "operationally meaningful theorem" is supposed to be. If it is supposed to be a "strong simple case" pointing the way to "an element of truth present in a complex situation" [2, p. 181], then we have no quarrel. For, I submit, this is what the bulk of economic theory does. It is based on counterfactual assumptions, contains only theoretical constructs and no operational concepts, and yields results which, we hope, point to elements of truth present in complex situations. To call such theorems "operationally meaningful" is to confer on them a designation which is slightly deceptive; but in any case it gives them the recognition which Samuelson, as critic of the Friedman position, or "the F-twist," wants to deny.

I conclude that Samuelson, one of the most brilliant theorists in present-day economics, produces his best work when he deduces from unrealistic assumptions general theoretical propositions which help us interpret some of the empirical observations of the complex situations with which economic life confronts us.

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## Theory and Realism: A Reply

1. The art of jujitsu is to direct your opponent's strength against himself. As I read Fritz Machlup's attempt to use my earlier writings on international factor-price equalization and on operationally meaningful theorems to annihilate my recent contention that the contrafactual content of a theory is its shame and not its glory, I am all admiration for his pretty footwork. Indeed I feel like the friend of Abraham Lincoln, who said he could have foregone the pleasure of being run out of town on a rail if it weren't for the honor of it.

But the issue is a serious one, and I must record the impression that, after examining Machlup's argument, I see no reason to change my view or to acknowledge any inconsistency between my precept and my practice.

2. Let the issue be clear. No one expects that anything be *perfect*, much less a simplified theory. All scientists settle for some degree of approximation; and, by gad, they had better! However, the whole force of my attack on the F-twist (which I believe is not well rendered by the first sentence that got quoted from my paper) is that the doughnut of empirical correctness in a theory constitutes its worth, while its hole of untruth constitutes its weakness. I regard it as a monstrous perversion of science to claim that a theory is *all the better for its shortcomings*; and I notice that in the luckier exact sciences, no one dreams of making such a claim.

In connection with slavery, Thomas Jefferson said that, when he considered that there is a just God in Heaven, he trembled for his country. Well, in connection with the exaggerated claims that used to be made in economics for the power of deduction and a priori reasoning—by classical writers, by Carl Menger, by the 1932 Lionel Robbins (first edition of *The Nature and Significance of Economic Science*), by disciples of Frank Knight, by Ludwig von Mises—I tremble for the reputation of my subject. Fortunately, we have left that behind us. Still there is no reason to encourage tolerance of falsification of empirical reality, much less glorify such falsification.

3. My position is so innocuous as to be platitudinous. Yet from it, Machlup believes, must follow a rejection by me of all theory. (In my copy the last two words are double-underlined!) He says: "A theory, by definition, is much wider than any of the consequences deduced." By whose definition? Machlup's? God's? Webster's? Well, not by mine—as those who read my earlier note will have seen. And I have to confess to an even sharper disagreement with Dr. Machlup on this issue.

Scientists never "explain" any behavior, by theory or by any other hook. Every description that is superseded by a "deeper explanation" turns out upon careful examination to have been replaced by still another description, albeit possibly a more useful description that covers and illuminates a wider area. I can illustrate by what everyone will agree is the single most successful "theory" of all time. I refer to Newton's theory of universal gravitation.

Kepler's ellipses gave better descriptions of the planets than did the epicycles of the Greeks. Newton showed that second-order differential equations relating the accelerations of bodies to the inverse square of their distances from neighboring bodies could still better *describe* astronomical observations. After Newton had described "how," he did not waste his time on the fruitless quest of "why?" but went on to run the Mint and write about religion. Nor has anyone since Newton provided "the explanation."

The second greatest achievement of scientific theory was Maxwell's formulation of electromagnetism. Heinrich Hertz, the great discoverer of radio waves, made my methodological point in the strongest terms when he said, "All of Maxwell's theory boils down to the simple question of whether the observable measurements on light and waves do or do not satisfy Maxwell's partial differential equations."

My third and final example illustrates how a stubborn fact can kill a pretty theory. Mercury's orbit never quite agreed with Newtonian gravitation. An F-twister would have said: "So much the worse for the fact. The theory is even the better for its inadequacy." That, of course, is rot. One would not jettison Newton's theory until a better one was found to replace it, for the very good reason that Newton did describe many facts *correctly*. But any specialist on the perihelion of Mercury would prefer to use mechanical extrapolations to reliance on the false theory. Then along came Albert Einstein. His special theory of relativity described well (but did not "explain") a host of facts, including the perihelion of Mercury. For velocities small compared to the speed of light, Newton's theory came close to duplicating Einstein's. But when the factual chips were down, the simpler Newtonian equations had to be replaced by the Einstein-Lorenz equations *because the facts called for this*.

4. About my 1948 factor-price equalization analysis I can be brief. From certain empirical hypotheses taken as postulates, by cogent logic I *deduced* as theorems certain other empirical properties. After the demonstration, the implications were obvious. When one looks at the complicated real world, one finds it obvious that the hypotheses of the syllogism are far from valid; and, also, the consequences are far from valid. This is indeed a matter for regret and full disclosure of inaccuracy should be made. Nevertheless, and here I imagine Dr. Machlup and I for once will agree, a strong polar case like this one can often shed useful light on factual reality.

Thus, writing before the Marshall Plan and at a time when many were despairing of Europe's future, I was led by my factor-price model to make the diffident suggestion that moving goods by trade might be able to do nearly as much for living standards as moving Europeans to Australia and elsewhere. Any validity to such a lucky insight would have to be attributed to the degree

of realism of the model, and I can only pray for my brain children that they be ever more realistic.

5. Since the emphasis of my *Foundations of Economic Analysis* on "operationally meaningful theorems" has been brought up, it gives me the opportunity to use my strength against my friendly critic. The doctrines of revealed preference provide the most literal example of a theory that has been stripped down to its bare implications for empirical realism: Occam's Razor has cut away every zipper, collar, shift, and fig leaf. In 1938 I had shown that the regular theory of utility maximization implied, for the two-good case, no more and no less than that "no two-observed points on the demand functions should ever reveal the following contradiction of the Weak Axiom":

$$\begin{aligned} P_1^a Q_1^a + P_2^a Q_2^a &> P_1^a Q_1^b + P_2^a Q_2^b \\ P_1^b Q_1^b + P_2^b Q_2^b &> P_1^b Q_1^a + P_2^b Q_2^a \end{aligned}$$

In *Foundations*, I showed that the complete theory of regular utility for any number of goods could be exhausted by the empirical hypothesis that the following Slutsky-Hicks matrix of compensated-substitution terms be singular, symmetric, and nonpositive definite of rank  $n-1$ . I.e., the demand functions  $Q_i = Q_i(P_1, \dots, P_n, I)$  satisfy

$$S = [S_{ij}] = \left[ \frac{\partial Q_i}{\partial P_j} + Q_j \frac{\partial Q_i}{\partial I} \right] = [S_{ji}]$$

$$\sum_{j=1}^n S_{ij} P_j = 0$$

$$\sum_1^n \sum_1^n S_{ij} P_i P_j x_i x_j < 0 \quad \text{for } x_i \neq \lambda P_i, \quad \lambda \neq 0$$

In 1950, Professor H. S. Houthakker, in a brilliant maiden work, strengthened the Weak Axiom to show that the whole conventional theory, for any number of goods, boils down (aside from obvious regularity conditions of continuity and smoothness) to the testable requirement that one never observe the following contradiction of the Strong Axiom:

$$\sum P^a Q^a > \sum P^a Q^b, \quad \sum P^b Q^b > \sum P^b Q^c, \dots, \sum P^s Q^s > \sum P^s Q^a.$$

I beg that Dr. Machlup take me literally and seriously when I assert that the conventional theory has *no* wider implications than these prosaic factual implications. Once the two formulations had been rigorously proved to imply each other mutually, that issue was settled.

6. The F-twist, in the crude form set up and massacred by me, represents an unlamented lost cause. Let me conclude by mentioning some genuinely puzzling methodological problems that Dr. Machlup and I might continue to argue about fruitfully.

a. Quine, Polanyi, Kuhn, Hanson,<sup>1</sup> and other students of science have

<sup>1</sup> W. V. O. Quine, *From a Logical Point of View* (New York, 1961), Ch. 2, "Two Dogmas of Empiricism"; M. Polanyi, "Tacit Knowing," *Reviews of Modern Physics*, Oct. 1962, 34, 601-16; T. Kuhn, *The Structure of Scientific Revolutions* (Chicago 1962); N. R. Hanson, *Patterns of Discovery* (Cambridge, U.K. 1961).

been propounding variants of the view that what is a fact (and what is a truism!) is a very subjective thing. While I should guess that much of this discussion involves a confusion between the psychological problem of forming scientific notions and the valid findings of science, undoubtedly there are deep problems here that need further study.

b. Scientists constantly utilize parables, paradigms, strong polar models to help understand more complicated reality. The degree to which these do more good than harm is always an open question, more like an art than a science.

c. Finally, in some areas at least, Nature seems to show an inexplicable simplicity. This is a brute fact, more or less of a bonus, which if it had not existed could not have been expected. As a result, the working scientist learns as a matter of routine experience that he should have faith that the more beautiful and more simple of two equally (inaccurate) theories will end up being a more accurate describer of wider experience.

This bit of luck vouchsafed the theorist should not be pushed too far, for the gods punish the greedy.

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### Diminishing Returns and Linear Homogeneity: Comment

In a recent issue of this journal, G. Warren Nutter [3] has provided a mathematical specification of a production function which seeks to justify the practice common in many price theory books of drawing the short-run product curve with a point of inflection *and* a maximum point, while assuming the long-run curve to involve constant returns to scale. He cites as the reason for his communication part of my footnote invitation to those who draw the curve in this way to *specify* their function [2, p. 126, n. 3] and a statement by Professors Stonier and Hague [5, p. 229] in which they flatly assert that the marginal product of the variable factor will always diminish when returns to scale are constant. (Both my footnote and its context make clear that a maximum point is also contemplated.)

Although Nutter implies that my view is identical to that of Stonier and Hague, he is wrong: My position is simply that "a thorough search of the literature has failed to reveal a single case in which a writer has *specified* a linearly homogeneous production function which produces such a total product curve." Nor has Nutter cited any examples. Indeed it can be done, but he, too, is unsuccessful in his *ex post* attempt to specify a function which will produce diagrams such as those contained in George Stigler's book [4, p. 115, Fig. 46] or in Richard Leftwich's book [1, p. 111, Fig. 34; p. 124, Fig. 36], which may presumably be taken as representative examples. Both depict the short-run curve as having a maximum point.

Nutter's entry, which he asserts is representative of a family which does exist and makes economic sense, is:

$$(1) \quad x = 22a^{1/4}b^{3/4} - 20a^{1/2}b^{2/3},$$

with the first and second partial derivatives:

$$(2) \quad \frac{\partial x}{\partial b} = \frac{66a^{1/4}}{4b^{1/4}} - \frac{40a^{1/3}}{3b^{1/3}}$$

$$(3) \quad \frac{\partial^2 x}{\partial b^2} = -\frac{66a^{1/4}}{16b^{5/4}} + \frac{40a^{1/3}}{9b^{4/3}}$$

In these equations,  $x$  is output, and  $a$  and  $b$  are inputs. [Shinsuke Horiuchi has pointed out that the exponent of  $b$  in the second term on the right in equation (3) should be  $4/3$  and not  $3/4$ .]

Nutter states that "if  $a=1$ , equations (1) and (2) are positive for all  $b \geq 0.998$ ." This must mean that, when  $a=1$ , total product continues to increase without limit as long as the variable input increases beyond  $b=0.998$  because  $\partial x/\partial b$  remains positive. Thus *his* short-run product curve has *no* maximum point.

Nutter's equation is of the general form:

$$(4) \quad x = Aa^tb^{1-t} - Ba^ub^{1-u},$$

where  $t$  and  $u$  are positive fractional exponents, each less than unity. In general, in the case of equation (4),  $x$  will have a zero value when

$$(5) \quad b = a \left[ \left( \frac{B}{A} \right)^{1/(u-t)} \right].$$

The general formula for the marginal product of  $b$  is

$$(6) \quad \frac{\partial x}{\partial b} = (1-t)Aa^tb^{-t} - (1-u)Ba^ub^{-u}.$$

This partial derivative will be equal to zero when

$$(7) \quad b = a \left[ \left( \left( \frac{1-u}{1-t} \right) \left( \frac{B}{A} \right) \right)^{1/(u-t)} \right].$$

Letting  $b_0$  represent the value of  $b$  at which  $x=0$ , and  $b_1$  represent the value of  $b$  at which  $\partial x/\partial b=0$  in equations (5) and (7) respectively, it follows that

$$(8) \quad b_1 = b_0 \left[ \left( \frac{1-u}{1-t} \right)^{1/(u-t)} \right].$$

In equation (8), if  $u > t$ , the term in parenthesis will be less than unity, but the exponent will be greater than unity; and if  $u < t$ , the term in parenthesis will be greater than unity, but the exponent will be negative (with an absolute value greater than unity). Thus, in equation (8), the base raised to the power will always be less than unity, unless  $u=t$ . Accordingly,  $b_1$  will be less than  $b_0$  in every case except that in which  $u=t$ . It follows, therefore, that the marginal product of  $b$ , or  $\partial x/\partial b$ , will always attain a

zero value at a lower level of input of the variable factor than that at which the total product, or  $f(x)$ , attains its zero value, except when  $u=t$ . In the latter case, both attain a zero value at the same level of input. Moreover,  $\partial x/\partial b$  will be negative for all values of  $b$  less than the complex term on the right in equation (7) and positive for all values of  $b$  greater than that term. It follows, therefore, that  $f(x)$  will generally have a minimum point at a negative level of output (whatever that may mean in economic terms) but that it has no maximum point.

Since Nutter's function has no maximum point, it can hardly serve as the mathematical basis of the curves depicted in books like those mentioned above. What Professor Nutter has done is to provide a linearly homogeneous production function with a point of inflection on the short-run product curve, but, in the process, he has eliminated the point of negative marginal returns to the variable factor (the Point of Absolutely Diminishing Returns). Thus, in the case of his family of functions, what is ordinarily described as Stage II, the Stage of Diminishing Average Returns to the Variable Factor, never ends.

Nor is this all. Although his statement "if  $a=1$ , equations (1) and (2) are positive for all  $b>0.998$ " is true enough and indicates that his function has no maximum, the statement is quite meaningless. Since, in Nutter's equations  $u>t$ , not only do these two equations become positive at *different* values of  $b$ , but also, because of the values of the constants specified by him, neither becomes positive when  $b=0.998$ . His statement can be made about any values of  $b>0.319$ . Equation (2) is equal to zero when  $b=0.07756$  and positive thereafter, while equation (1) is equal to zero when  $b=0.319$  and positive thereafter. These *two* values differ too greatly from each other and from Nutter's *single* value of 0.998 to allow explanation of the difference as a minor arithmetical error.

Moreover, Nutter also asserts [3, p. 1085]:

... equation (3) is positive for  $2.45>b>1$  and negative for  $b\geq 2.45$ . Of course, over the range at which  $b$  has increasing returns,  $a$  has a negative marginal product. That is,

$$\frac{\partial x}{\partial a} < 0 \quad \text{for } 1 < b/a < 2.45.$$

Not only is this statement wrong, but the upper numerical value ( $b/a=2.45$ ) is the value at which his *second partial derivative* is equal to zero. The incorporation into the statement of *this* value clearly shows that he has fallen into the elementary error of confusing the *direction* of movement of the *marginal* product of the variable input, on the one hand, with its *sign* and with its *magnitude* relative to that of the *average* product of the variable input on the other. The nature of his mistake can be made clear by reference to Euler's Theorem, which, of course, applies to linearly homogeneous functions.

The theorem states:

$$(9) \quad x = \frac{\partial x}{\partial a} a + \frac{\partial x}{\partial b} b,$$

and by dividing both sides by  $b$  and transposing the terms, we have

$$(10) \quad \frac{x}{b} - \frac{\partial x}{\partial b} = \frac{\partial x}{\partial a} \frac{a}{b}.$$

Since  $a$  and  $b$  are inputs and  $x$  is output, and all three are always positive in the relevant discussion, so are  $a/b$  and  $x/b$  always positive. The sign of  $\partial x/\partial a$  thus depends upon the sign (not on the direction of movement) of  $\partial x/\partial b$  and upon the relative magnitudes of  $x/b$  and  $\partial x/\partial b$ . The term  $x/b$  is, of course, the average product of the variable factor. Equation (10) discloses that whenever  $\partial x/\partial b$  is negative,  $\partial x/\partial a$  must be positive. Moreover,  $\partial x/\partial a$  will be positive when  $\partial x/\partial b$  is positive only if  $\partial x/\partial b$  is less than  $x/b$ ; and  $\partial x/\partial a$  will always be negative when  $\partial x/\partial b$  is both positive and greater than  $x/b$ .

It happens in the case of Nutter's function that, when  $\partial x/\partial b$  is positive and increasing, it is also greater than  $x/b$ . But, it is clearly true in the case of his function that  $\partial x/\partial b$  is also positive and greater than  $x/b$  for part of the range in which  $\partial x/\partial b$  decreases and that  $\partial x/\partial a$  is negative up to the point at which  $\partial x/\partial b = x/b$ .

Making use of Nutter's equations and constants, I have produced the results shown in Table 1. This table clearly demonstrates the nature of his errors. It shows that not only is Nutter's lower limit for  $\partial x/\partial a < 0$  incorrect, but also that his upper limit is wrong. Since the marginal and average products of the variable input are approximately equal when  $b=9.98$ , he should have written

$$\begin{aligned} x &< 0 && \text{for } 0.319 < b/a \\ \partial x/\partial a &< 0 && \text{for } 0.000 < b/a < 9.98. \end{aligned}$$

The values in Table 1 also disclose that Nutter's function produces a curve which is different from those contained in the books cited previously. When  $x$  is measured on the vertical axis and  $b$  is measured on the horizontal axis, his function produces a short-run product curve which begins at the origin, falls below the origin, with  $x$  becoming ever more negative until  $b=0.07756$ , and then begins to rise, with  $x$  becoming ever less negative until  $b=0.319$ , where the curve crosses the  $b$  axis. The value of  $x$  then increases at an increasing rate as  $b$  increases, until  $b=2.45$ , and, eventually, the value of  $x$  increases without limit at a decreasing rate. Admittedly, the table does not show the value of  $x$  beyond the point at which  $b=1,000,000,000,000$  when  $a=1$ .

Nutter completes his communication by stating this conclusion at the end of his comment [3, p. 1085]:

The essential point is that the condition of linear homogeneity in a production function does not, in and of itself, rule out areas of short-run increasing returns (or negative marginal products) to some inputs.

If this is *the* essential point, Nutter has said nothing new. In the sentence in my book [2, p. 126] immediately following the one to which I have at-

TABLE 1

$a$	$b$	$x$	$x/b$	$\partial x/\partial b$	$\partial x/\partial a$	$(\partial x/\partial b)(b)$	$x - (\partial x/\partial b)(b)$
1	0	0	0	0	0	0	0
1	0.01	-0.23	-23.25	-9.71	-0.13	-0.10	-0.13
1	0.08*	-0.40	-5.21	0	-0.40	0	-0.40
1	0.32**	0	0	2.44	-0.78	0.78	-0.78
1	0.99#	2.00	2.00	3.17	-1.16	3.16	-1.16
1	2.45	6.73	2.75	3.30	-1.35	8.08	-1.35
1	2.50	6.90	2.76	3.30	-1.35	8.25	-1.35
1	3.00	8.55	2.85	3.29	-1.33	9.88	-1.33
1	4.00	11.83	2.96	3.27	-1.24	13.07	-1.24
1	9.98	30.82	3.09##	3.09	-0.02	30.84	-0.02
1	10 <sup>a</sup>	264.81	2.65	2.34	30.31	234.50	30.31
1	10 <sup>1a</sup>	2(10 <sup>10</sup> )	2(10 <sup>-2</sup> )	15(10 <sup>-2</sup> )	4.83(10 <sup>9</sup> )	15(10 <sup>9</sup> )	4.83(10 <sup>9</sup> )

## Notes:

\* 0.07756

\*\* 0.319

# 0.998

## 3.08832

tached the footnote—which is partly quoted in Nutter's comment—I have said:

It follows from Euler's Theorem and the fact that the production function is assumed to be linearly homogeneous that, whenever the marginal product of  $a$  is greater than the average product of  $a$ , the marginal product of  $b$  *must* be negative [italics in the original].

(In my example  $a$  is the variable input.)

And, on page 133 of the book, I have repeated for the benefit of the careless or untrained reader:

... As we have already seen, if total product or output ( $X$ ) increases proportionately as all inputs are increased proportionately, the case involves constant returns to scale, and the production function is linearly homogeneous. As we have already also seen, this type of production function is not inconsistent with the principles of increasing or diminishing marginal or average returns to the variable factor, for even though output increases proportionately as inputs are increased proportionately, this fact alone does not require or result in a situation in which output changes proportionately *when one factor only is changed* and others are held constant [italics in the original].

The preceding quotation speaks for itself. Indeed, since the publication of Nutter's comment, I have received from Ryuzo Sato a copy of a brilliant paper concerning the stability of growth equilibrium (to be published in *Econometrica*) which he delivered at the December, 1963 meeting of the Econometric Society. This paper contains a new and quite general formulation of linearly homogeneous production functions. Some of the forms of the Sato function do, in fact, produce short-run total product curves having points of inflection as well as maximum points, while others do not. Nutter's equation is merely a special case of the Sato function. As far as I know,

Sato is the first to produce a general equation of this type, and, although he utilizes it in the context of growth problems, one by-product of his work is that he has also met a need long existing in the literature of price theory.<sup>1</sup>

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<sup>1</sup> I wish to thank Shinsuke Horiuchi for having assisted me in making the computations.

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#### Diminishing Returns and Linear Homogeneity: Comment\*

In his comment on G. Warren Nutter's note, H. H. Liebhafsky refers to my production function, the one exhibiting short-run total product curves which have points of inflection as well as maximum points. This function is

$$(1) \quad Y = F(K, L) = \frac{K^2 L^2}{b_0 L^3 + b_1 K^3}, \quad b_0 > 0, \quad b_1 > 0,$$

where  $Y$  = total product,  $K$  = capital, and  $L$  = labor. When

$$\frac{L}{K} = \sqrt[3]{\frac{2b_1}{b_0}},$$

the marginal product of labor becomes zero, and when

$$\frac{L}{K} = \sqrt[3]{\frac{b_1}{2b_0}},$$

the marginal product of capital becomes zero. Furthermore, since

$$F''\left(1, \frac{L}{K}\right) < 0 \quad \text{for} \quad \left[\frac{b_1(7-3\sqrt{5})}{2b_0}\right]^{1/3} < \frac{L}{K} < \left[\frac{b_1(7+3\sqrt{5})}{2b_0}\right]^{1/3},$$

short-run diminishing returns set in when  $L/K$  is equal to

$$\left[\frac{b_1(7-3\sqrt{5})}{2b_0}\right]^{1/3}.$$

As Liebhafsky mentions, I used the above function to illustrate the stability conditions of growth equilibrium ("On the Stability of Growth Equilibrium" presented at the 1963 meeting of the Econometric Society). In fact, equation (1) is a special case of a general linear homogeneous function,

$$(2) \quad Y = \left[ aL^{-\alpha} + \sum_{i=0}^n b_i K^{-\beta_i} L^{-(\alpha-\beta_i)} + cK^{-\alpha} \right]^{-1/\alpha}.$$

In the above equation when  $a=c=0$ ,  $b_i=0$  (except for  $i=0$  and  $i=1$ ),  $\alpha=1$ ,  $\beta_0=2$  and  $\beta_1=-1$ , then equation (2) turns out to be equation (1). And also in equation (2), when  $a=c=0$ ,  $b_0=22$ ,  $b_1=-20$  and  $b_i=0$  ( $i=2, 3, \dots, n$ ),  $\alpha=-1$ ,  $\beta_0=-\frac{1}{4}$ , and  $\beta_1=-\frac{1}{3}$ , then we obtain Nutter's equation. When all  $b_i=0$ , then we get the constant-elasticity-of-substitution (CES) function.

I used equation (2) to illustrate the production function represented by equation (1) as well as to illustrate a production function in which the elasticity of substitution is always positive (the positive-elasticity-of-substitution function).

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### Diminishing Returns and Linear Homogeneity: Comment

Professor Nutter shows that constant returns to scale and increasing marginal productivity are not mutually exclusive properties of a production function. But, while his production function displays these two properties, it differs in two important respects from the "traditional" production function<sup>1</sup> with which all students of economics are familiar.

The Nutter production function is:

$$(1) \quad x = 22a^{1/4}b^{3/4} - 20a^{1/3}b^{2/3}$$

where  $x$  is output and  $a$  and  $b$  are inputs of two productive services. Differentiating with respect to  $a$  and  $b$ , the marginal products of the productive services are found to be:

$$\begin{aligned} \frac{\partial x}{\partial a} &= \frac{22}{4} a^{-3/4} b^{3/4} - \frac{20}{3} a^{-2/3} b^{2/3} \\ \frac{\partial x}{\partial b} &= \frac{66}{4} a^{1/4} b^{-1/4} - \frac{40}{3} a^{1/3} b^{-1/3}. \end{aligned}$$

Holding  $a=1$  and varying  $b$ , we find the following deviations from the "traditional":

<sup>1</sup> See, for example, R. H. Leftwich, *The Price System and Resource Allocation* (New York 1960), p. 124.

1. Output is negative if  $0 < b < .3113$  even though the inputs of both productive services are positive.

2. If  $.01385 < b < 2.447$  the marginal product of  $b$  is positive and rising with increases in  $b$  and the marginal product of  $a$  is negative. This is the result sought. But the function is not symmetrical, for there is no area within which the marginal product of  $a$  is positive and falling with increases in  $b$  while the marginal product of  $b$  is negative.

A production function which conforms to the "traditional" is:

$$(3) \quad x = \frac{a^2 b^3}{.5a^3 + .5b^3}$$

where, again,  $x$  is output and  $a$  and  $b$  are inputs of two productive services.<sup>3</sup> Differentiating, we find,

$$(4) \quad \begin{aligned} \frac{\partial x}{\partial a} &= \frac{ab(b^4 - .5a^3b)}{(.5a^3 + .5b^3)^2}, \\ \frac{\partial x}{\partial b} &= \frac{ab(a^4 - .5ab^3)}{(.5a^3 + .5b^3)^2}. \end{aligned}$$

This function displays constant returns to scale, positive output for all positive combinations of inputs of the two productive services, and (holding  $a=1$  and varying  $b$ ) the familiar marginal product curves. Specifically:

1. If  $0 < b < .5265$  the marginal product of  $b$  is positive and increases with increases in  $b$  while the marginal product of  $a$  is negative.

2. If  $b > 1.898$  the marginal product of  $a$  is positive and decreases with increases in  $b$  while the marginal product of  $b$  is negative.

JOHN W. ROWE, JR.\*

<sup>3</sup> This is a representative of a family of production functions described by:

$$x = \frac{a^\alpha b^\alpha}{ga^{2\alpha-1} + (1-g)b^{2\alpha-1}}$$

which includes special cases of the Cobb-Douglas ( $\alpha=.5$ ) and CES ( $\alpha=1$ ) production functions. If  $\alpha > 1$ , areas of increasing marginal productivity result.

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### Diminishing Returns and Linear Homogeneity: Comment

In many respected textbooks<sup>1</sup> there will be found the opinion that increasing marginal productivity of a variable input and linear homogeneity of the production function are incompatible. This error may result from failure to write down algebraic functions that fulfill both criteria. G. Warren Nutter [8] has now given a linearly homogeneous function that has an initial region of increasing marginal returns to the variable input. Another such function with first increasing marginal returns was mentioned by Karl Menger [7, pp. 25-56; esp. p. 30]. But the two functions of Menger and Nutter are not representative of the "law of diminishing returns" that has dominated economic thought since Serra and Turgot, particularly in the well-known interpretation of Frank H. Knight.<sup>2</sup> This "law" is represented in general by a three-dimensional surface, a "hill of returns,"<sup>3</sup> symmetrical with respect to two inputs. If input  $a$  is held constant and small portions of input  $b$  are successively combined with it, marginal returns to the variable input will at first increase and then eventually decrease. The same result will follow if  $a$  is made the variable input and  $b$  the fixed one.

The simplest functions that fulfill the "Knightian case" are of the following type:

$$(1) \quad x = (-Aa^3 + Ba^2b + Cab^2 - Dd^3)^{1/3}$$

or

$$(2) \quad x = \frac{-Aa^3 + Ba^2b + Cab^2 - Dd^3}{Ea^3 + Fb^3}$$

where  $x$  is output;  $a$  and  $b$  are inputs of different productive services; and  $A, B, C, D, E$ , and  $F$  are parameters such that  $B+C > A+D$ .

In the special case of

$$(2a) \quad x = \frac{-a^3 + 6a^2b + 6ab^2 - b^3}{a^3 + b^3}$$

we have

$$(2a') \quad \frac{\partial x}{\partial a} = \frac{-a^4 - 9a^2b^2 + 14ab^3 + 6b^4}{(a^3 + b^3)^2}$$

$$(2a'') \quad \frac{\partial^2 x}{\partial a^2} = \frac{14(a^3b^2 - 3a^2b^3 - 3ab^4 + b^5)}{(a^3 + b^3)^3}.$$

If  $b=1$ ,  $(2a'')$  is positive for  $a < 0.25$ . The partial derivatives for  $b$  are analogous.

<sup>1</sup> See, for instance, Erich Schneider [9, p. 197, n. 2].

<sup>2</sup> Compare Knight [6, pp. 99-100]; Cassels [2, pp. 223-36; reprint pp. 103-18, esp. p. 109].

<sup>3</sup> Compare Edgeworth [3, p. 75], Fisher [4, pp. 304-11], and Carlson [1, p. 18].

There are now two crucial points in these functions. For  $b$  a constant,  $x$  will be negative for  $a=0$  and will become positive for  $a>q>0$ , where  $q$  is some positive value of  $a$ . That is, these symmetrical functions all contradict the usual graphical representation in textbooks, where the total product curve (as a function of one variable input) begins in the origin of coordinates. The same contradiction holds for functions like

$$(3) \quad x = \frac{(-Aa^3b + Ba^2b^2 - Cab^3)}{Ea^3 + Fb^3}$$

because the economically relevant region with positive marginal returns begins where the variable input is greater than zero.

It is perhaps even more interesting to compare the Knightian case of the law of diminishing returns with the original agricultural examples, especially with those of Turgot [10, p. 420] and Wicksell [11, pp. 354-55]. These examples imply a positive output even when no variable input (for instance, manure) is used. For this condition to hold, production function (2) must (for instance) be rewritten as

$$(4) \quad x = \frac{-Aa^3 + Ba^2b + Cab^2 + Db^3}{Ea^2 + Fb^2}.$$

In such a case, the isoquants trace out a rather odd pattern. In the normal Knightian case neither one of the two inputs can be entirely dispensed with if output is to be positive, even in the long run when both inputs are variable. In the economic region of substitution all isoquants obey the principle of diminishing marginal rate of substitution.<sup>4</sup> This is not so in the Turgot-Wicksell case. Here one input [ $a$  in (4)] can be entirely eliminated without eliminating output, and for small quantities of this input  $a$  there is an increasing marginal rate of substitution of  $b$  for  $a$ . The isoquants in the Turgot-Wicksell case are concave toward the origin for small values of  $a$  and convex only for larger values of  $a$ . This can be seen when we define the marginal rate of substitution of  $b$  for  $a$  as

$$r_b = \frac{-\partial x}{\partial a} \bigg/ \frac{\partial x}{\partial b}.$$

In the Knightian case (2a)

$$(5) \quad r_b = - \left( \frac{-a^4 - 9a^2b^2 + 14ab^3 + 6b^4}{6a^4 + 14a^2b - 9a^2b^2 - b^4} \right).$$

In the Turgot-Wicksell case [(4) with the same parameters as (2a)]

$$(6) \quad r_b = - \left( \frac{-a^4 - 9a^2b^2 + 10ab^3 + 6b^4}{6a^4 + 14a^2b - 3a^2b^2 + b^4} \right).$$

Both (5) and (6) may be reduced to functions of the ratio  $a/b$ , and the described properties may then be easily verified.

As is well known, the fact that neither input is dispensable—that is,

<sup>4</sup> See Hicks [5, p. 20].

the fact that isoquants are of the normal Cassels-type [2, p. 109]—does not imply marginal returns that first increase and then decrease. We can see this from the production function

$$(7) \quad x = (-Aa^2 + 2Bab - Cb^2)^{1/2}, \quad \text{for } B^2 > AC,$$

which implies isoquants of the Cassels-type but which displays diminishing returns from the beginning. It follows that the traditional law of diminishing returns is not implied by the dual conditions of (1) indispensability of both inputs and (2) a fixed ("indivisible") amount of one of them. Moreover, these dual conditions are not met by the famous historical examples of Turgot and Wicksell, both of which imply dispensability of one input. These historical examples contradict the general principle of a diminishing marginal rate of substitution. The properties of inputs are different in the two cases, those of Knight and Turgot-Wicksell.

As far as I can see, this dichotomy has never been mentioned in our textbooks. I think the failure to do so has resulted from the loose practice of not basing productivity curves and isoquants on specified algebraic production functions.

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### Diminshing Returns and Linear Homogeneity: Comment

Professor Nutter should be commended for showing, figures in hand, that the same production function can exhibit constant returns to scale *and* increasing (diminishing) returns to one factor of production. This is a point which economics students often fail to grasp. In the process, however, he may have raised fresh doubts in the minds of our charges on the relationship between the marginal products of the variable and fixed factors.

Using the linear and homogeneous function

$$x = 22a^{1/4}b^{3/4} - 20a^{1/3}b^{2/3}$$

where  $x$  is output and  $a$  and  $b$  are the two inputs, Nutter shows that the marginal product of  $b$  increases over the range  $2.45 > b > 1$ , and concludes:

Of course, over the range at which  $b$  has increasing returns,  $a$  has a negative marginal product. That is,

$$\frac{\partial x}{\partial a} < 0 \quad \text{for } 1 < b/a < 2.45.$$

The essential point is that the condition of linear homogeneity in a production function does not, in and of itself, rule out areas of short-run increasing returns (negative marginal products) to some inputs.

This may easily mislead the reader into thinking that increasing returns (in the conventional sense of "increasing *marginal* returns") to one input imply a negative marginal product for the other input. Now this is not necessarily so. The correct statement is that increasing *average* returns to the variable factor imply a negative marginal product for the fixed factor, and conversely (for a proof, see George Stigler, *The Theory of Price*, New York 1952, rev. ed., p. 302, mathematical note 9).

Using Nutter's production function, it can be seen that  $\partial x/\partial a$  is negative over a wider range than the one indicated, i.e.,

$$\frac{\partial x}{\partial a} < 0 \quad \text{for } 1 < b/a < 10.06$$

as the average product of  $b$ , with  $a=1$ , reaches its maximum for  $b=10.06$ .

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### Diminishing Returns and Linear Homogeneity: Reply

My one-page note on increasing returns and linear homogeneity has borne unexpected returns of its own, some positive and some negative. In stimulating the communications of Dr. Dieter Schneider, Mr. de Fontenay, Professor John Rowe, and Professor Ryazo Sato, it has justified itself and hence needs no further defense. Were it not for the nature of Professor Liebhafsky's response, I would remain silent, but under the circumstances I feel I must make a few remarks.

My note deals simply with a simple issue unquestionably raised in Liebhafsky's book (*The Nature of Price Theory*, Homewood, Ill. 1963, p. 126) as well as in other texts. That issue has to do with whether it is possible to specify a linear and homogeneous production function in which marginal returns to a variable input first increase and then decrease as the variable input increases, other inputs fixed. The note was designed to meet that straightforward issue alone and not to be a rigorous discourse on the properties of prototype production functions, already well known to practicing economists, or on anything else. Reacting with puzzling vehemence, Liebhafsky now denies that he raised the issue in the first place, criticizes me for not discussing something else, and finds fault with my arithmetic. The last point is valid.

What he did or did not say is no doubt more important to Liebhafsky than to others, and there is therefore no point in dragging out a fruitless exegetical controversy. It is sufficient for me to say that he has not persuaded me that my original interpretation of his position was wrong. The footnote (*ibid.*) that I quoted speaks for itself, and it is not modified in any significant way by the surrounding text (*ibid.*, pp. 123-26). Moreover, the later statement (*ibid.*, p. 133) now cited by Liebhafsky does not alter the central issue. It merely says that marginal returns can *either* increase *or* decrease as the variable input increases, not that they can do both. Otherwise, what is the point of the earlier footnote? Finally, Liebhafsky's present insistence on linking the question of a maximum point in the short-run total product curve with the question of an inflection point is clearly an afterthought, since the two are not linked together in the cited footnote or the immediately surrounding discussion in his book. It is relevant to observe that he does not refer to diagrams of Stigler, Leftwich, or any other particular economist in his book even though he now does in his comment on my note.

But enough of such extraneous matters. Liebhafsky does offer two more relevant criticisms. First, he points out an arithmetic error on my part. (I ignore the typographical slip that set  $4/3$  as  $3/4$ .) My statement that "if  $\alpha=1$ , equations (1) and (2) are positive for all  $b>0.998$ " should read "are both positive for all  $b>0.319$ ." This mistake, an unfortunate and inexcusable copying error on my part, does not affect the general argument.

Liebhafsky, along with Mr. de Fontenay, also takes me to task for a careless statement about the relation between increasing marginal returns to one input and negative returns to the other. My statement, inserted as an afterthought to satisfy a referee, happens to be correct as it stands (except for the arithmetical mistake noted above), but it is misleading and incomplete. Because of some interesting points made by Dr. Schneider, it will therefore be useful to set forth more rigorously some of the relations that can exist in a linear and homogeneous production function among the various average and marginal product curves, and what those relations imply about the shape of isoquants.

The following relevant conditions hold for a linear and homogeneous production function  $\phi(a, b)$ :

- (1)  $\phi_a = x/a - (b/a)\phi_b$
- (2)  $\phi_{aa} = (b/a)^2\phi_{bb} = - (b/a)\phi_{ab}$
- (3)  $\partial b/\partial a = - \phi_a/\phi_b$
- (4)  $\partial^2 b/\partial a^2 = - (x/b)^2\phi_{aa}/\phi_b^3$

For convenience in later discussion of Schneider's contribution, let us refer to  $a$  as the variable input in the short run and to  $b$  as the fixed input.

As Liebhafsky and De Fontenay point out, we see from (1) that the marginal product of  $b$  will be negative if, and only if, the marginal product of  $a$  is larger than the average product of  $a$ —that is, if, and only if, the average product of  $a$  increases with  $a/b$ . Of course, an increasing average product of  $a$  does not imply an increasing marginal product of  $a$ . Similarly, an increasing marginal product of  $a$  does not imply a marginal product of  $a$  larger than the average product of  $a$ .

The marginal products of  $a$  and  $b$  cannot both be negative at any given  $a/b$  within the relevant region of the production function, which is to say that the average product can be increasing for only one input. It therefore follows from (3) that isoquants will be downward sloping at a given  $a/b$  if, and only if, the marginal products of both inputs are positive or, put another way, the average products of both inputs are diminishing. Isoquants will be upward-sloping if, and only if, the marginal product of one input is negative, which is to say that the average product of the other input is increasing.

Finally, we see from (4) and the preceding argument that isoquants will be convex toward the  $a$ -axis at a given  $a/b$  if, and only if, the marginal and average products of  $a$  are moving in the same direction. The isoquants will be concave if, and only if, the average and marginal products of  $a$  are moving in opposite directions.

In the case of a symmetrical production function with Knightian properties as represented by, for example, Schneider's function (2), isoquants will look like hairpins and will pass through five stages in their shapes as  $a/b$  increases through its relevant range. In stage one, isoquants will be upward-sloping and convex toward the  $a$ -axis; in stage two, upward-

sloping and concave; in stage three, downward-sloping and convex; in stage four, upward-sloping and convex; and in stage five, upward-sloping and concave. These correspond, of course, to the five stages in the Knightian law of variable proportions.

In the Turgot-Wicksell case as represented by Schneider's asymmetrical function (4), the first three stages of the Knightian case are replaced by two stages in which isoquants are downward-sloping, first concave and then convex to the  $a$ -axis. This comes about because the average product of  $a$  diminishes from the start—a finite output is possible without use of any  $a$ —even though the marginal product of  $a$  first increases and then diminishes. Thus the increasing returns to  $a$  in the Turgot-Wicksell case are strictly marginal and not average.

I will conclude with a comment on Schneider's statement that textbooks usually show the total product curve as starting at the origin, while his symmetrical function makes it start at some positive value for the variable input. Whatever may be the case for usual textbooks, Schneider's symmetrical function corresponds entirely with Knight's original formulation (*Risk, Uncertainty, and Profit*, London School Reprints of Scarce Works, 1957, pp. 99–100), which sets the total product at zero when the variable input is below a critically small level.

For this reason, Schneider's equation (1) or (2) is to be preferred to Rowe's function (3) or Sato's equation (1), since they imply a short-run total product curve that begins in the origin and becomes asymptotic to the horizontal axis. A generalized expression for a linear and homogeneous production function with all the Knightian properties would apparently be

$$(5) \quad x = \left( -\beta_0 a^{m/n} + \sum_{i=1}^{m-1} \beta_i a^{(m-i)/n} b^{i/n} - \beta_m b^{m/n} \right)^{n/m}$$

or

$$(6) \quad x = \left( -\beta_0 a^{m/n} + \sum_{i=1}^{m-1} \beta_i a^{(m-i)/n} b^{i/n} - \beta_m b^{m/n} \right) \cdot (\alpha_0 a^{(m-n)/n} + \alpha_1 b^{(m-n)/n})^{-1}$$

where  $x$  is output,  $a$  and  $b$  are inputs,  $\alpha$ 's and  $\beta$ 's are positive constants such that

$$\sum_{i=1}^{m-1} \beta_i > \beta_0 + \beta_m,$$

and  $m$  and  $n$  are any two different odd integers.

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### Multilateral Balancing in International Trade: Comment

In a recent article in this *Review* [2] Michael Michaely has introduced a new index of multilateral balancing of trade. At the same time he has formulated another such index based on an early report published by the League of Nations [1, p. 60].<sup>1</sup> He has then proceeded to use his own index rather than the League's (or more accurately, the one based on the League's report) for calculating the indices of multilateral balancing for various countries. Surprisingly, Michaely nowhere discusses why he has used his own index rather than the League's or any other index. He has done so presumably because he considers his index in some way superior to others. This can be the only justification one can give for introducing a new index when an old one is known to exist. The purpose of this note is to examine the validity of this implicit claim on behalf of Michaely's index.

Before we do that, however, we shall have to remove a slip in Michaely's algebraic formulation of the League's index. The League's report divides total trade into three parts: bilaterally balanced ( $BB$ ), multilaterally balanced ( $MB$ ), and unbalanced ( $U$ ). While in his footnote 7 [2, p. 693] Michaely interprets the League's index to mean  $MB/(BB+MB)$ , in his footnote 5 [2, pp. 688-89], where he gives the algebraic formulation of the index, he declares it wrongly to be  $(MB+U)/(BB+MB)$ .

Using a simplified version of Michaely's notations (which will be employed in the rest of this note), if we let  $X$  and  $M$ , respectively, stand for the total export and import of the country concerned, and  $X_i$  and  $M_i$  for its export to and import from country  $i$ , then the League's index (following Michaely's interpretation) would be

$$T_L = \frac{\sum_{i=1}^n |X_i - M_i| - |X - M|}{X + M - |X - M|}$$

where  $n$  is the number of the trading partners of the country concerned, and not

$$T_L' = \frac{\sum_{i=1}^n |X_i - M_i|}{X + M - |X - M|}$$

as in Michaely's algebraic formulation [2, pp. 688-89].

One obvious difference between the two formulae is that while the former ( $T_L$ ) has a range of values between zero and one, the latter ( $T_L'$ ) has no maximum limit. Thus only  $T_L$  can be expressed in percentage terms.

Michaely's own index (without multiplying it by 100), expressed in the above symbols,

$$T_M = \frac{\sum_{i=1}^n \left| \frac{X_i}{X} - \frac{M_i}{M} \right|}{2}$$

<sup>1</sup> As cited by Michaely in [2].

also possesses the characteristic of varying between zero and one. This no doubt is a desirable characteristic, but in this respect Michaely's index is not superior to the correctly formulated League's index.

But this is not all. There is at least one practical and one theoretical reason for preferring the reformulated League's index (henceforth called simply "League's index"). The practical reason is simply that it is very much easier to calculate. When calculating the League's index for a particular country, only one operation of division is required, while two such operations are needed for each trading partner of the country when Michaely's index is calculated. Thus if a country has 70 trading partners, Michaely's index would require 140 division operations, some of them showing quotients only in the sixth or seventh decimal place, against only one such operation for the League's index.

However, the much more serious deficiency of Michaely's index is analytical. There is nothing in his formula to remove or even reduce the influence of unbalanced trade. Michaely is himself aware of this and has therefore excluded a number of countries from consideration because they had a large trade imbalance. He even suggests that the higher average of the indices for various countries in 1948 as compared with 1938 may be the result of a greater unbalanced trade in 1948, rather than of greater multilateral trading in that year [2, pp. 689-91]. A simple illustration in Table I below shows clearly how Michaely's neglect of unbalanced trade may lead to absurd results.

Suppose that there are only three countries, A, B, and C, and A's trade with the other two is as follows:

TABLE 1  
(Value in million dollars)

	Export to	Import from
Country B	50	50
Country C	50	100

Here country A has no trade surplus with either of the two countries. Therefore, confining our attention to the above situation, there is no possibility of any multilateral settlement. But by using Michaely's formula ( $T_M$ ) we should get 0.167 as the index of multilateral balancing. The conclusion is obvious that a nonzero value of Michaely's index is no guarantee at all that the situation calls for a multilateral balancing of trade.

If we now turn to the League's index ( $T_L$ ), we can immediately see the attempt to remove the influence of unbalanced trade in the last term of both the numerator and the denominator. One result of this procedure is that we can be sure of obtaining zero value of the index where no multilateral settlement of trade is possible—it is in fact the result of taking away the unbalanced trade from the numerator only. Thus if we apply the League's index to the situation illustrated in Table I, we shall get a zero value for the index, which is as it should be.

The case for the League's index would appear even stronger when we realize that Michaely's and the League's indices become identical when the

trade is balanced, as in this case  $X$  equals  $M$  and both the formulae reduce to:

$$\frac{\sum_{i=1}^n |X_i - M_i|}{2X}.$$

The difference between the two, therefore, exists only when trade is unbalanced, in which case the League's index is demonstrably a superior one. Incidentally, in all cases of unbalanced trade the League's index gives a lower value than Michaely's.

Finally we may mention another index which is similar to that of the League, but which is slightly simpler in concept as well as in calculation. This index has been used, among many others, by Erik Thorbecke [3, p. 82].<sup>2</sup> This is the multilaterally balanced trade expressed as a proportion of total trade. In the above symbols it can be expressed as:

$$T_T = \frac{\sum_{i=1}^n |X_i - M_i| - |X - M|}{X + M}.$$

This also gives the same value of the index as the League's and Michaely's when trade is balanced. In all other cases it gives a lower value than the League's, which, as we have noted, gives a lower value than Michaely's.  $T_T$  also lies between zero and one, and, like the League's index, would give a zero value for the illustration in Table I. The last characteristic of  $T_T$  results from taking away the unbalanced trade from the numerator. But since the unbalanced trade has not been subtracted from the denominator (as was done in the case of  $T_L$ ), an increase in unbalanced trade would lower the value of the index.

Comparing the three indices we find that both the League's index and Thorbecke's index give the correct answer to the problem set in Table I, but that Michaely's index does not do so. Hence both  $T_L$  and  $T_T$  appear superior to  $T_M$ . Between  $T_L$  and  $T_T$ , my own preference is for  $T_L$ , mainly because of its neutral behavior in the face of a change in unbalanced trade. While an increase in unbalanced trade raises the value of Michaely's index, it lowers the value of Thorbecke's index. Only the League's index remains unaffected. In the case where there is no unbalanced trade (which of course would probably never be found in practice), we have already seen that there is no difference among the three.

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<sup>2</sup> See also [4, Ch. 1, p. 36] and [2, p. 693, n. 7].

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### Multilateral Balancing in International Trade: Reply

In his comment on my measurement of multilateral balancing in international trade [1], Professor Syed Ahmad (1) removes a slip in my algebraic formulation of the League of Nations' index of multilateral balancing—a task for which I am grateful; and (2) argues that the League of Nations' index should be preferred to the one used by me—an argument which I should like to dispute.<sup>1</sup>

Although Ahmad claims a general superiority of the League's index, his discussion is limited to a particular case, which seems to him to be clear-cut. In making explicit the underlying difference between the two indexes, it may indeed be most convenient to refer to this case. I shall also follow, for illustration, the numerical example provided by Ahmad. Country A's trade looks, then, as follows:

	<i>A's Exports to:</i>	<i>A's Imports from:</i>
Country B	50	50
Country C	50	100
	<hr/>	<hr/>
World	100	150

This is a situation in which country A has an over-all import surplus and no export surplus to any of its trading partners. Under these circumstances, Ahmad argues, "there is no possibility of any multilateral settlement." And any index, such as my own, which would provide here a positive coefficient of multilateral balancing, would therefore "lead to absurd results." But is this really so?

Country A, in this example, has an over-all import surplus of 50 units. It finances 50 units of imports of goods (and services) through capital imports; that is, it exchanges 50 units of financial assets for an equal value of goods. This exchange may be bilateral, or it may be multilateral. A broad definition of an index of multilateral balancing would have to take this exchange into account. I have chosen, on the other hand, to define the meas-

<sup>1</sup> I shall confine myself to a discussion of the conceptual meaning of the indexes. The practical reason which Ahmad suggests for preferring the League's index to mine, namely, that my index requires more operations of division than the League's, must be of minor importance—as Ahmad seems to agree. Incidentally, for the interwar years it would work the other way: the trade figures released by the League itself, in its annuals of *International Trade Statistics*, appeared as percentages of total exports or imports, respectively.

urement of multilateral balancing in a more narrow sense, so that it excludes the exchange of goods for financial assets. This has been done for several reasons,<sup>2</sup> whose acceptance is implied, it seems to me, in the League's index, and which are not disputed by Ahmad.

There is thus need to eliminate somehow that part of imports which is financed by capital imports, and treat the trade of the country in question as if it were balanced. There may be various ways of doing it, and each of them rests on arbitrary assumptions.

The question may be stated as follows: what would have happened to country A's trade had capital imports disappeared and the country's trade been balanced? The use of the League's index assumes, in the situation on hand, that the elimination of 50 units of capital imports would have reduced imports *from country C*—the one with whom country A maintains an import surplus—by the same amount. This, however, is only one of many possibilities. Even had the capital imports been provided in fact by country C, there is no need that their elimination would have expressed itself necessarily in the reduction of imports from C. Moreover, without access to further data, there is no reason to presume that country C is indeed the source of capital imports—it could be country B just as well.

In other words, the use of the League's index assumes, implicitly, that all exchanges of goods for financial assets are necessarily bilateral. This, obviously, is merely one of various possible assumptions. One might, for instance, go to the other extreme and adopt the assumption that all such exchanges are multilateral. I cannot see any a priori reason for selecting either of these two extremes. The use of my index amounts to the "neutral" assumption that, in the absence of capital imports, A's imports from each country would decline *in the same proportion*.<sup>3</sup> A's trade would then be as follows:<sup>4</sup>

	<i>A's Exports to:</i>	<i>A's Imports from:</i>
Country B	50	33
Country C	50	67
	<hr/>	<hr/>
World	100	100

<sup>2</sup> See the discussion in [1, pp. 686-88].

<sup>3</sup> The index of multilateral balancing is, then, (using Ahmad's notation)

$$\frac{\sum_i \left| Xi - Mi \frac{X}{M} \right|}{X + M - (M - X)}$$

This may easily be shown to be equal to the index used by me, which is

$$\frac{\sum_i \left| \frac{Xi}{X} - \frac{Mi}{M} \right|}{2}$$

<sup>4</sup> One might assume a proportionate increase of exports instead of a reduction of imports. The result, for the purpose on hand, would obviously be the same. Likewise, the argument would be similar—with proper qualifications—for a country running an export surplus rather than an import surplus.

Here, of course, part of country A's trade is multilaterally balanced—hence the positive value of my index of multilateral balancing.

Stated alternatively, the use of my index assumes that the exchanges of goods for financial assets are multilateral to the same extent as are the exchanges of goods for other goods. While, to repeat, this is nothing but an arbitrary assumption, it seems to me to be preferable, in the absence of any contradicting information or indication, to the one implied in the League's index.<sup>5</sup>

To sum up: It is obviously incorrect—as, I hope, I have shown here—that “there is nothing in [Michael's] formula to remove or even reduce the influence of unbalanced trade.” Both the League's formula and my own do try to remove this influence; they do it by asking, implicitly, what would the country's trade look like in the absence of capital imports? In answering this question the two indexes differ in resorting to different assumptions. I believe I have selected the better assumption; but this may be a reflection of one's feeling that whenever arbitrariness is concerned, his is preferable to anybody else's.<sup>6</sup>

MICHAEL MICHAELY\*

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<sup>5</sup> I cannot quite see the rationale of the index which Ahmad designates as Thorbecke's (Thorbecke himself, as would appear from [2, p. 81], probably has in mind something like the League's index, rather than this one). This index does not, incidentally, have the property of ranging from zero to one. Its lower limit is indeed zero; but its upper limit, as may easily be verified, is below one in a country with unbalanced trade.

<sup>6</sup> It may be possible to devise tests which would provide a more objective yardstick for the selection of the proper assumption. It may be rewarding, for instance—although I have not carried it out—to run a multiple correlation in which the degree of imbalance in the country's trade would be added to the factors which are presumed to genuinely affect the degree of multilateral balancing in the country's trade. This may be repeated for each of the alternative concepts of the index of multilateral balancing. The lower the correlation found between unbalanced trade and multilateral balancing, the stronger should be our belief that the index of multilateral balancing is free of bias introduced by the existence of capital imports or exports.

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#### Note on Lag in Effect of Monetary Policy

Because of the long lag in the publication of the research studies prepared for the Commission on Money and Credit, many readers are likely to be misled about the present state of some issues discussed. This note is to call their attention to one such issue, namely, a criticism of my work to which I replied in print some two years before the publication of the criticism.

In their article on “Lags in Fiscal and Monetary Policy,” which appears

in the CMC volume entitled *Stabilization Policies*, Albert Ando, E. Cary Brown, Robert M. Solow, and John Kareken state [2, p. 3]:

We have come to the conclusion . . . that this proposition [Milton Friedman's proposition that the effects of monetary policy actions on aggregate output are powerful, but occur with a very long and highly variable lag] simply will not hold water. We believe that the method underlying the proposition is based on a *non sequitur*, and further we have convincing evidence that all or most of the Friedman lag is a statistical artifact.<sup>1</sup>

Though this article first appeared in published form in December, 1963, a mimeographed version was available in 1960. And near-identical criticisms were made in an article by John Culbertson. I replied to both in an article [1] on "The Lag in the Effect of Monetary Policy," published in October, 1961.

It is amusing that, while the authors assert that "when correction is made for the bias" which is said to be inherent in what is termed the "Friedman approach," "no systematic lag appears," they themselves conclude, using other techniques, "that some substantial stabilizing power results after a lapse of time of the order of six to nine months"—a conclusion that, to put it mildly, is in no way inconsistent with my own. I find it even more amusing that, with appropriate changes, we also agree on another point: we each believe the other has come to approximately correct conclusions by faulty methods!

The main tool Kareken and Solow use is multiple regression in which prior values of the dependent variable enter as independent variables along with the current value of the variable suspected of having a lagged effect (call this the operational variable). The coefficients of the prior values of the dependent variable are used to estimate the distributed lag effect of the operational variable. This procedure will, under appropriate conditions, yield a valid estimate of the distributed lag, *if such a lag exists*. But, by itself, it does not provide any evidence on the existence of the lag, since it cannot discriminate between serial correlation in the dependent variable which arises from other sources (e.g., errors of measurement) and that which arises from the distributed lag effect of the operational variable. When other sources do make for serial correlation, the Kareken-Solow method is likely to produce poor and biased estimates of whatever distributed lag does exist—at least, that has been my experience in some calculations of this kind I have made. Perhaps this difficulty does not affect the Solow-Kareken calculations, but they give no evidence whatsoever on this point, and as yet, I believe, we have too little experience with the use of the technique to be able to accept its results as reliable without other confirming evidence.

MILTON FRIEDMAN\*

<sup>1</sup> The quotation is from the summary of Part I, which appears under the name of all four authors, although Part I itself is signed only by Kareken and Solow.

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## The Paternity of an Index

In the March issue of this *Review*, Benton F. Massell [5, pp. 52 ff.] uses an index of trade concentration of the form  $\sqrt{\sum (x_i/x)^2}$  where  $x_i$  is the value of a country's trade in commodity  $i$  (or with trading partner  $i$ ) in some period, while  $x$  is the country's total trade. This index appears to have come into wide use recently and, to my rather chagrined surprise, is referred to, by Massell as well as by Kindleberger [4, p. 143], Michaely [6], and Tinbergen [9, pp. 268 ff.], as the "Gini index" or "Gini coefficient."<sup>1</sup> Given the sudden popularity of the measure, I feel that I should stand up for my rights as its originator. It was first introduced and computed for a large number of countries in my book *National Power and the Structure of Foreign Trade* [3, Ch. 7 and pp. 157-62]. As explained there, the use of the index is indicated when concentration is a function of both unequal distribution and fewness. The traditional measures of concentration, generally devised in connection with income distribution and the Lorenz curve, are sensitive only to inequality of distribution, and we do owe several such measures to Gini.

The confusion on this score is the stranger as I referred at length in my book to the important work of the Italian statisticians on measurement of concentration, and particularly to Gini [3, pp. 157-58]. Upon devising the index I went carefully through the relevant literature because I strongly suspected that so simple a measure might already have occurred to someone. But no prior inventor was to be found.

To complicate the story, I must add that there was a posterior inventor, O. C. Herfindahl [2], who in 1950 proposed the same index, except for the square root. While obviously unaware of my earlier work when writing, Herfindahl did acknowledge it in a footnote [2, Ch. 1 and p. 21, n.]. Nevertheless, when the index is used for measuring industrial concentration, the second principal area of its application, it is now usually referred to as the "Herfindahl index," owing to a well-known paper by Rosenbluth [7] who has, however, recently made a valiant, but probably vain, attempt to straighten the matter out [8, pp. 391-92].

The net result is that my index is named either after Gini who did not invent it at all or after Herfindahl who reinvented it. Well, it's a cruel world.

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<sup>1</sup> An honorable exception must be made for Coppock [1, pp. 97 ff.].

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### The Opportunity Locus in a Hedging Decision: A Correction

In my 1961 *AER* article on spot and futures prices,<sup>1</sup> I drew the opportunity locus between expected return ( $E$ ) and risk ( $R$ ) as a straight line. This is incorrect; it should be a concave function (negative second derivative). All the other results of that paper are unchanged.

The risk on a linear combination of hedged and unhedged stock is equation (1), where  $x$  refers to the fraction unhedged.

$$(1) \quad R = \text{var} [xu + (1 - x)h].$$

Variables  $u$  and  $h$  are the returns derived from unhedged and hedged stock, respectively.

(a)  $u = p^* - p - m$  where  $p^*$  is the expected price,  $p$  is the current price, and  $m$  is the marginal net carrying costs.

(b)  $h = (p^* - p) - (q^* - q) - m$  where  $q$  is the futures price, and  $q^*$  is the expected futures price at the time the contract will be liquidated.

Solving for  $R$ , we obtain equation (2).  $\sigma_p^2$  is the variance of  $p^*$ ;  $\sigma_q^2$  is the variance of  $q^*$ , and  $r$  is the correlation coefficient between  $p^*$  and  $q^*$ .

$$R(x) = \sigma_p^2 + (1 - x)^2 \sigma_q^2 - 2r(1 - x)\sigma_p\sigma_q.$$

$$(2) \quad \begin{aligned} (a) \quad R(0) &= \sigma_p^2 + \sigma_q^2 - 2r\sigma_p\sigma_q \\ (b) \quad R(1) &= \sigma_p^2 \end{aligned}$$

<sup>1</sup> J. L. Stein, "The Simultaneous Determination of Spot and Futures Prices," *Am. Econ. Rev.*, Dec. 1961, 51, 1012-25.

The slope of the opportunity locus is  $dE/dR$ , where  $E = xu + (1-x)h$ , and is equation (3).

$$\begin{aligned} \frac{dE}{dR} &= \frac{dE/dx}{dR/dx} = \frac{(u-h)/2}{(r\sigma_p\sigma_q - \sigma_q^2) + x\sigma_q^2} = f(x). \\ (3) \quad (a) \quad f(0) &= \frac{(u-h)/2}{r\sigma_p\sigma_q - \sigma_q^2}, \\ (b) \quad f(1) &= \frac{(u-h)/2}{r\sigma_p\sigma_q}. \end{aligned}$$

The slope of the opportunity locus is positive at  $x=1$  if the expected return on unhedged stock exceeds that on hedged stock. When  $x=0$ , the slope will be negative if the variance of  $q^*$  exceeds that of  $p^*$ . There would exist an  $x$  between zero and unity where a vertical tangent occurs.

Since  $f'(x)$  is negative, the general concavity is proved.

No change is required in the rest of the paper.

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## BOOK REVIEWS

### General Economics; Methodology

*Bargaining Behavior.* By LAWRENCE E. FOURAKER AND SIDNEY SIEGEL.  
New York: McGraw-Hill Book Co., 1963. Pp. ix, 309. \$8.95.

The purpose of this book is to examine the decision-making behavior of human beings when confronted with a conflict situation in which there are both competitive and cooperative elements. From the social sciences, economics was chosen as a suitable vehicle for this study because of its quantitative nature and its large body of theory. The study is divided into three parts: bilateral monopoly with equal strength, bilateral monopoly with unequal strength, and oligopoly. The results of the equal-strength case were reported earlier in "Bargaining and Group Decision Making" by the same authors (1960), and this book is concerned with the latter two categories.

The unequal-strength bilateral monopoly case consists of a single buyer and seller attempting to decide on the price and quantity of goods to be sold. This situation is simulated when the subjects in the experiment attempt, by examining tables relating the profit to be made with the prices and quantities sold, to maximize their profits (which they are subsequently allowed to keep). In this model the seller fixes the quantities to be sold and the buyer names the price. According to the hypothesis one of two types of resultant profit-splitting should occur. Either the payoff should be Pareto optimal, i.e., it should be impossible for the two participants simultaneously to improve their profits; or a Bowley solution should occur, i.e., the seller should set that price which will maximize his profits, assuming the buyer subsequently sets the quantities so as to maximize his own profits. In general, the Bowley solution will not be Pareto optimal.

There were three independent variables considered by the experimenters. The first was the amount of information available to the subjects. The subjects either had complete information: knowledge about the profits to himself and his partner as the result of any contract; or incomplete information: knowledge of only his own profit. The second variable was the method of bargaining. Bargaining was either a single transaction whereby the seller offered a price, the buyer a quantity, and the bargaining was over, or there was a sequence of price and quantity proposals culminating in an agreement. Finally, because it was felt that there might be a tendency toward the culturally favored point where the profits are split evenly, the position of this point (whether in the Pareto or Bowley solution) was also considered to be an independent variable.

It was predicted that complete information, repeated bidding, and the equal-profit point lying on the Pareto optimality-point set, would all tend to make the outcome Pareto optimal. The fact that the Bowley point has strong equilibrium properties and is essentially unilaterally enforceable while the

Pareto optimal result requires cooperation, communication, and information makes this hypothesis reasonable.

The set of experiments was undertaken at the University of Pennsylvania with 53 pairs of undergraduates as subjects. The subjects were never confronted with their counterparts, and all communications were made by written sheets of paper so that there was no visual or aural contact. It was recognized that there might be some effect of social interaction, and this was deliberately avoided. With three independent variables, where each may be in either of two states, there are eight possible combinations. It was felt that for the incomplete-information state only the case with repeated bidding where the equal-profit point was Pareto optimal was of interest so that only five cases were used.

Briefly, the results were as predicted. The most significant factor tending to put the outcome toward Pareto optimality was the form of bidding: repeated bargaining became a means of communication between the two players and, subsequently, a means of cooperation as well. Second in importance was the amount of information available; a knowledge of one's partner's profits, as well as one's own seemed to be necessary in reaching a Pareto optimal point. Finally the position of the equal-split point was least significant. The Bowley point, because of its strong equilibrium properties, predominated, however, except when the three favorable states of the three independent variables appeared in concert. The reasons for variations from the general tendencies mentioned above were also considered.

The last part of the study concerned itself with oligopoly: competition between a few competing sellers. Two types of situations were examined: in the first situation the sellers controlled only the single-variable quantity, in the second, only price. As a theoretical model for the first case the Cournot solution was considered. In this case it is assumed that only quantity may be adjusted and that the production of other firms is not influenced by the decision-maker's choice of production level. In the price-adjustment case the Bertrand solution was considered. It is thus assumed that the seller, in trying to maximize his profits, does not influence the already quoted price of his competitors.

There were three independent variables considered: the number of sellers (two and three were used), the amount of information given to the participants, and the "level of aspiration" of the participants. There were three categories for the latter variable—*rivalistic*, in which the participant attempts to maximize the difference between his own and his competitor's profits; *simple maximizer*, where he attempts to maximize his own profit; and *cooperative*, in which he places a positive value on his competitors' profits as well as his own. Although no attempt was made to categorize the players in advance, it was shown that players could recognize the Pareto optimal points and the competitive points as being the results of cooperative and rivalistic strategies when bonuses were given for cooperative and competitive behavior respectively.

As was predicted, incomplete information favors the Cournot and Bertrand solutions, the tendency toward the Bertrand solution becoming stronger, the

greater the dispersion, while the tendency in the Cournot case becomes greater as the number of players increases. Under complete information there is more dispersion in both cases, and the sources of this dispersion are discussed.

The results of this study as well as of the earlier work by the same authors, and any future work along the lines they suggest, will be of interest to psychologists, economists, game theorists, and behavioral scientists, in general, for they lay an empirical foundation upon which any meaningful theory of human behavior must lie.

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*Economic Policy and War Potential.* By MAX E. FIESER. Washington: Public Affairs Press, 1964. Pp. vi, 136. \$4.50.

Dr. Fieser criticizes U.S. protective measures for defense industry. Still important, these measures have had large impacts upon the world economy during and since the Korean War. Yet he finds either little explicit economic analysis to provide a rationale for them or analysis based upon outmoded mobilization concepts. He briefly reappraises World War II concepts, rightly finding them inappropriate for unlimited nuclear war or for possible limited wars. Then, however, his subsequent discussion fits within the old mobilization categories: matériel requirements, industrial capacity, criteria of essentiality, and the role of import restrictions.

No more is claimed than (p. 58) a "general framework within which the analysis of industrial defense potential and essentiality can proceed," i.e., sensibly ordered economic criteria to guide policy studies. The vital empirical content must be supplied elsewhere via industry studies that measure the excess burden from particular protective measures in terms of specific alternatives. This book will contribute to their orientation; while, in questioning old defense rationale and noting less clumsy instruments than import restrictions, it should help to counter naïve policies. But the trained interindustry analyst who has also absorbed such older defense references as Klaus Knorr's *War Potential of Nations* and Charles Hitch and Roland McKean's *The Economics of Defense in the Nuclear Age* will find little that is novel.

Beyond Fieser's chosen area lies economic planning for recuperation after general war, industrial reconversion if there should be drastic shifts in peacetime arms spending, and enhanced efficiency in acquiring arms systems. An author's scope is his privilege, especially when nobody has dealt adequately with such formidable topics. But can his proper cautions against excessive damage to international trade for outmoded defense reasons not be extended further? Is deliberately heightened interdependence now reasonable in some cases? If the United States neither needs nor can rely upon a huge mobilization after any hostilities start, we might look to greater dependence upon for-

eign sources, either because they are cheaper or less likely to be damaged. Fieser's analysis might be extended more positively, if more riskily.

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*The Mythmakers—An Essay on Power and Wealth.* By B. D. NOSSITER. Boston: Houghton Mifflin, 1964. Pp. ix, 244. \$4.00.

This is an important book which should be read by all economists. B. D. Nossiter is a perceptive observer of economics as well as a penetrating critic of economists. He demonstrates great insight into economic problems, especially the problem of economic power. He is critical of the Kennedy administration—indeed, all recent administrations—for failures in the area of economic concentration. In making what is actually, if not avowedly, an assessment of the late President's economic policies, Nossiter describes the anatomy and operation of the American political economy and prescribes some remedies for what he considers clear and present ills. The immediate questions for which Nossiter seeks answers are:

Could the business-oriented policies of the New Frontier master the problems confronting the economy? Will the corporate vision bring about full employment of men and resources, quicken the sluggish pace of economic growth and bring material abundance to the surprisingly large group outside the affluent society? Is the existing economic structure capable of solving these problems, as the President implied, or are some institutional changes necessary?

Throughout much of his analysis, it is clear enough that economists are included among the mythmakers in the book. He demonstrates that much of contemporary economic theory is irrelevant or useless in terms of solving real problems. Nossiter also is critical of the artificial division drawn between politics and economics, describing expertly how this conventional division obscures analysis of the real world.

The book consists essentially of eight provocative essays ranging from "The Corporate Vision and the New Frontier" to "The Myth of Countervailing Power" and "The Two Half-Truths of Disarmament." The concluding essay tackles the engaging issue of "Planning and Freedom," in which Nossiter advances a modest proposal for consideration.

The first two essays are devoted to the economic doctrines of the Kennedy administration because the problems it faced and the choice of solutions illuminate themes that recur throughout the book. Nossiter observes that the Business Advisory Council's triumph over Secretary of Commerce Hodges in the spring of 1961 provided an important clue to the character of the new administration. He argues that, of all the myths in the business literature, one of the most imaginative and furthest removed from reality is that portraying the late President as anti-business. The New Frontier's main thrust is evaluated as clearly in the conservative direction, copied in the main from Eisenhower's

earlier program. As an illustration, Nossiter uses the administration's concern with the balance-of-payments deficit, which he notes "ran in the grooves marked out by the most orthodox banking thought." Similarly, he observes that the celebrated trade bill of 1962 was a "masterful invocation of nineteenth century liberal economic thought," very little of which would "give sleepless nights to Adam Smith, David Ricardo, John Stuart Mill, or corporations and banks with interests abroad."

On the dramatic 1962 clash over steel prices, Nossiter, with careful documentation, shows that the real issue was not Blough's deliberate personal challenge to the President, but the threat steel's action posed for the administration's wage-price policy, an essentially pro-business, conservative position. The President's acquiescence in the 1963 round of "selective" increases Nossiter regards as symptomatic of the administration's unwillingness or inability to confirm the 1962 victory with some new institutional arrangements to deal with corporate pricing power.

In brief, in every important area of economic policy—wage policy, tax policy, international trade and finance, federal spending—Nossiter reasons, it is doubtful that a Republican President could have done as much for "Wall Street" in his stereotype role as a "business tool."

His essay "The Visible Hands" is among the best. In contemporary versions of the fable of the invisible hand, the real and the spurious are "buried under layers of conventional belief and self-serving myth." Whereas the untutored observer takes for granted the overriding fact of economic concentration, says Nossiter, the distressing fact is that it is often ignored by economists and taken into account by government policy-makers only with reluctance and when it intrudes on their vision with a prominence that can't be denied. It is difficult to rebut his judgment that:

Then, too, economists have invested considerable intellectual capital in exploring a model world of small, competing firms. If adequate account was taken of the fact the industries are generally organized by a few competitors, much of this capital stock would become obsolete.

Nossiter recognizes that there are no panaceas or simple solutions to the problem of concentrated economic power, but he is disturbed that concentration and its consequences rank so near the bottom of the public and scholarly agenda. He is willing to accept the social conscience of the new breed of professional managers, but if indeed a world of abundance is close at hand in a world of concentration under neo-paternalism, he calls upon these socially responsible managers to lead us into it and cease the delay. Nossiter asks, how exactly do corporate managers determine when a decision is socially responsible? What are their standards and how are they derived? How are competing claims measured and weighed in the boardrooms? If backsliding occurs, what recourse does society have?

Countervailing power cannot act as a social and political check on the power of business—or that of labor and government. Nossiter cites the record of regulatory commissions in evaluating the balance theory or redressed power through government countervaleances. He finds symbiotic partnerships extend-

ing beyond the usual commissions to the cabinet departments as well. He concludes that in the present state of affairs the government is more likely to combine with than to offset business power.

Nossiter argues that our economy is hobbled by its economic structure. Despite the spread of affluence and near-affluence in our economy, there is an impressive amount of poverty and deprivation, compounded by a disturbing level of chronic unemployment. Our political-economic structure, built around corporate concentration, threatens to transform cures into placebos. Nossiter views this structure's potential for damage as acutely linked to a disarming economy. In one of the best of his essays, "The Two Half-Truths of Disarmament," he examines current illusions about the lurking fear that American prosperity depends on arms spending and that cutbacks would trigger a recession or depression. The heavy interlacing of military spending in the economy is viewed as a possible threat to economic stability. He argues that reliance on the post-World War II experience for a disarmament and demobilization parallel is deceptive. Following Emile Benoit, Nossiter sees a more subtle difficulty for our lagging growth rate arising from the fact that so much of our search for new knowledge is financed by arms spending. In net, however, Nossiter recognizes that arms reduction would pose new dilemmas which could be eased by programs of public expenditures and reduction in taxes.

Thus, instead of creating problems, Nossiter sees disarmament as offering enormous opportunities for real advances in material well-being. But if disarmament is to prove a blessing and not an economic curse, there must be advance planning for the transition. Plans for tax cuts, expansion of specific government undertakings, and other economic policies would serve two purposes: help calm those who fear disarmament and insure that appropriate measures to hurdle the transition itself will be taken.

The kind of planning envisaged by Nossiter is noncoercive, "indicative" planning of the European variety, a "systematic confrontation of ends with means." The new style of planning accepts the concentration of markets, dominated by a handful of firms, and strong labor unions, as conferring a host of possible advantages—technological and political—that flow from this order. Expanding upon Gardiner Means' recent proposals to control corporate action (*Pricing Power and the Public Interest*), Nossiter suggests we harness the energies of these great institutions through a diet more heavily laced with carrots than sticks. It would be planning by assent, whereby government economists would tentatively select a plausible rate of growth for the economy, from which a four-to-five-year model would be developed, spelling out the likely spending by the household, government, and industry sectors of our national income accounts. Each major industry, leading businessmen, union executives, and public representatives would calculate the model's implications for their specific area of interest. "The Plan" would emerge, following reconciliation of the projections of individual industries and elimination of inconsistencies. Nossiter cites approvingly the French version of this variety of planning, although he recognizes that France's spectacular growth in the 1950's may have been due as much to other, more basic factors.

Nossiter seems less interested in planning per se, than in the establishment

of precise quantitative "economic" targets, against which performance can be measured and costs compared with benefits of alternative programs. Debates over targets would focus attention on society's larger goals and help provide a solution to the problem of legitimacy for economic decision-making in an economy of concentrated economic power. At the very least, Nossiter argues, indicative planning holds out the hope that the quality of both public and private policies will be improved, and that the dominant firms will set target rates of return in the expectation of *expanding* rather than *fixed* levels of output and employment.

One would certainly be hard pressed to quarrel with Nossiter's desire to have us face more squarely and systematically society's larger goals and the principal means for implementing them. The case for planning, however, should not be unduly influenced by the peculiar growth rates of France, Japan, or Norway. Also, basic to Nossiter's proposal is the assumption of self-fulfilling projections, given the proper-sized carrots (subsidies, investment funds, tax benefits, etc.) and the government's power to induce cooperation through sanctions. Nossiter believes the latter would be largely unnecessary to encourage adherence to the plans, since self-interest would encourage business, consumers, unions, farmers, and other groups to follow paths laid down in the model. Active partnership between government and business undoubtedly would produce a greater degree of certainty, thus reducing the risk businessmen seek to minimize or avoid completely. The recognized danger that the planning mechanism will be captured by corporate executives—that business considerations will dominate in setting output and investment targets such that existing market shares and inefficient firms are protected—is suggested by the Japanese and French experience. Nossiter correctly observes that the unplanned American society is subject to the same dangers. The difference, perhaps, is whether the lack of sanction for private, nonresponsible power could be corrected by a technique in which general and particular publics participate in economic policy-making, thus satisfying a hunger which current meaningless fictions only frustrate.

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### **Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought**

*Aggregate Supply and Demand Analysis.* By PAUL DAVIDSON AND EUGENE SMOLENSKY WITH C. L. LEVEN. New York: Harper & Rowe, 1964. Pp. xiv, 274.

Modern income-employment theory consists essentially in the analysis of the whys and hows of  $Y$ ,  $\Delta Y$ , and  $\Delta Y/Y$ , where  $Y$  stands for the *level* of national income,  $\Delta Y$  for income *fluctuation*, and  $\Delta Y/Y$  for the *rate* of growth of income. This volume is long on the determination of the former two, but short on that of the latter. The book as a whole would appeal to micro-oriented students of static macrotheory with a considerable amount of training in the neoclassical theory of value and distribution. Its main pedagogic contribu-

tion lies in a novel attempt to add to Keynes' "Principle of Effective Demand" a Ricardian concern with the profit-maximizing, least-cost behavior of individual firms. Thus Part III, which embodies that addition, is devoted to a "post-Keynesian" treatment of the familiar "Ricardo effect," the rest being allocated to "a careful exposition" of the received Keynesian doctrine.

Unlike Joan Robinson and N. Kaldor (the former in a genuine post-Keynesian manner incorporating value and distribution theory into growth theory, and the latter in a like manner introducing income distribution into a non-linear saving function for cycle analysis and policy), Davidson and Smolensky give a rather less post-Keynesian and rather more neoclassical emphasis to the supply side of a market economy. Accordingly, one would be amazed (or disappointed) to discover that the Davidson-Smolensky "aggregate supply function" is devoid of such long-run shift parameters as capital stock and technology, thereby simply and summarily letting those dynamic independent variables be summed in the catchall " $M/A$ -ratio" affecting the general shape (rather than its position) of that supply function. (N.B. p. 133, where the authors nonchalantly state: "we may simply note that we would expect these changes [capital and technology] to primarily affect the aggregate supply function through their effects on the  $M/A$ -ratio.") That star coefficient is, we are told (p. 125), the ratio whose numerator is the wage rate times labor-input and whose denominator is the price index times output (in their symbolism,  $M/A = wN/PQ$ ). This catchall income-distribution ratio is said to affect both the supply and demand functions via the microeffect of wages on marginal cost and that of wage-profit relations on consumption-demand. The possible interdependent effect of the dual cost-demand character of wages upon the stability condition of macroequilibrium is not discussed at all. The result is a familiar short-run supply curve which is upward-sloping, due to the operation of diminishing returns, and which represents that segment of the marginal cost curve which cuts the average variable cost curve from below and through the latter's minimum point—for well-known reasons, albeit at the macrolevel. A passing mention of the linear homogeneous production function of the Cobb-Douglas variety is made by way of expressing an upward-sloping, yet straight, long-run supply curve based on the special assumption of the unitary elasticity of substitution of factors (or constant returns to scale).

For a book that purports to stress aggregate *supply*, it is a great pity that the authors have seen fit to ignore both Leontief's input-output analysis with all its interesting implications for micro-macrolinear programming and Harrod's dynamic theory with all its exciting implications for growth planning. This book, while addressing itself mainly to the problem of employment, nevertheless fails to deal with the Hicks-Patinkin-Kurihara controversy over the relation of the liquidity trap and full employment in static and dynamic conditions and with the Robinsonian model of structural underemployment (due to slow capital growth relative to rapid population growth or to labor-saving automation policies). Joan Robinson's concept of a "golden-age equilibrium" is misapplied to a short-run static equilibrium solution. Some students might find the section on "Social Accounts" by Charles L. Leven and the last

chapter on "Econometric Applications" stimulating enough to pursue further inquiry.

This textbook is technically as rigorous as Hans Brems' *Output, Employment, Capital, and Growth*, though less ambitious in scope. It is substantively less didactic, though formally more esoteric, than Dudley Dillard's *The Economics of John Maynard Keynes* and A. Murad's *What Keynes Means*, not to mention L. Klein's perceptive and provocative *The Keynesian Revolution*.

KENNETH K. KURIHARA

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*Optimales Wachstum und Optimale Standortverteilung.* RUDOLF HENN, GOTTFRIED BOMBACH, AND EDWIN VON BÖVENTER. Schriften des Vereins für Sozialpolitik, New Series Vol. 27. Berlin: Duncker & Humblot, 1962. Pp. 133. DM 19.80.

As the reviewer did, the reader may expect this to be a book on the interaction of growth and location. Such is not the case; the book is simply a collection of three separate and entirely unrelated essays, two on optimal growth and one on optimal location. For this disappointment the publisher rather than the writers must be responsible.

The first essay, by Rudolf Henn, is a straightforward exposition of the von Neumann and of von Neumann-like growth models.

The second essay, by Gottfried Bombach with an appendix by von Weizsäcker, is devoted to defining an optimum growth path in growth models using a Cobb-Douglas production function. In such models, as we know, the equilibrium proportionate rate of growth of output will ultimately depend on three things only: (1) the elasticity of output with respect to labor force, (2) the rate of growth of labor force, and (3) the rate of technological progress. The propensity to consume does not affect the proportionate rate of growth of output but it does affect the level of the growth path of output. Plot output on a logarithmic scale as a function of time on a nonlogarithmic scale. A lower propensity to consume will then shift the economy from one straight-line growth path to a higher but *parallel* one characterized by a higher capital coefficient. Consumption equals output times the propensity to consume, hence has a growth path parallel to that of output. Now the elasticity of output with respect to capital stock is less than one; hence, successive equal reductions of the propensity to consume produce diminishing upward shifts of the growth path of output, so the growth path of *consumption* is first shifted upward to a highest position, but will then shift downward again. That highest position is the optimum defined by Bombach-von Weizsäcker and shown to possess a certain similarity to the von Neumann model in which, as we know, the rate of growth equals the rate of interest. The exposition would have been perfectly clear, had it not referred to the maximization of the long-run *level* of consumption ("langfristiges Konsumniveau"). This reference is meaningless, for there is, of course, no convergence to a stationary level of consumption. Had the authors used the semilogarithmic dia-

gram just referred to, they could have defined their optimum simply and meaningfully as the maximization of the long-run growth *path* of consumption ("langfristiger Konsumwachstumspfad").

The third essay, by Edwin von Böventer, is an attempt to develop further the great German tradition in the theory of location. First a summary and evaluation is given of the contributions by von Thünen (1826), Walter Christaller (1933), and August Lösch (1944). It was Christaller who carried into economics the idea first utilized by the geographers (1916) that the regular hexagon is the polygon of the highest order with which a surface may be filled out completely, hence the beehive-like pattern of his markets around cities.

Neither von Thünen, nor Christaller, nor Lösch bothered themselves with such very real phenomena as migration or regional balance-of-payment equalization, and Böventer takes the first steps to include these in his analysis. More than first steps he cannot take within the space available, but at least he succeeds in showing the reader the majestic order of magnitude of the difficulties involved in the construction of a complete general-equilibrium theory of location. Precisely because of this order of magnitude, it is somewhat surprising that Böventer, unlike Henn, Bombach, and von Weizsäcker, uses no mathematics.

HANS BREMS

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### **Economic History; Economic Development; National Economies**

*The Rise and Decline of the Medici Bank, 1397-1494.* By RAYMOND DE ROOVER. Cambridge: Harvard University Press, 1963. Pp. 500. \$10.00.

Although most scholars would consider it one of the most important studies of the Medici bank, Professor de Roover, in his author's preface, discounts his earlier work as "little more than a sketch." Since that time (1948), he has discovered the *libri segreti* or secret account books of the bank, spanning the period 1397-1450, and has fully utilized them in the current work to provide balance sheets and income statements for the bank as a whole and for each of its branches, as well as for demonstrating that the major concern of the Medici was banking and not trade. The new documentation alone would make this an indispensable work, both for the Medici bank and for the period, but De Roover has brought to his study an almost unique combination of skills for interpreting the new material. He is at home in the countinghouse, drawing analogies to modern financial instruments where they illuminate the Medici procedures, but his finely developed sense of the period's history renders him sound and cautious about generalizations supported only by analogy.

De Roover provides an extremely concise survey of European economic history in the fourteenth and fifteenth centuries before proceeding to a more intensive consideration of factors directly relevant to Medici banking history such as usury doctrine, taxation, and coinage variations and types. The early history of the bank is traced from the acquisition by Giovanni di Bicci de Medici

of the Rome branch of the Vieri's bank until its transfer to Florence on October 1, 1397, De Roover's date for the founding of the Medici bank proper. The author shows statistically that as late as 1420 the bank was "a dwarf in comparison" with the Bardi or the Peruzzi of the early fourteenth century and suggests that real growth does not begin until 1423. In 1438 the Medici acquired a silk shop and at varying times several wool shops which could be incorporated into the organization without difficulty because the Medici structure until 1441 was that of a series of partnerships subsumed "in a larger unit similar to a modern holding company." An extremely interesting chart of the organization in 1455 is given (p. 83), showing the silk shop, the cloth shops, and the branches at Geneva, Bruges, London, Avignon, Milan, Rome, Pisa, and Venice, and incidentally incorporating a considerable knowledge of the technology of cloth and silk production.

Each bank balanced its books on March 24 and sent copies of its report to Florence for the benefit of the senior partners. De Roover argues that this was actually done and further that control was sufficient for the partners to specify the amount of assets to be reported for tax purposes, though not to prevent internal deception. The existence of fraudulent books complicates the sources, but the author rightly suggests that, while the balance sheets attached to the tax reports may be suspicious, it is unlikely that the subsidiary reports, involving thousands of transactions, would be faked. The author provides not only records of the whole organization but also those of each branch, studying separately their rise and fall by means of 71 statistical tables.

In his chapters on trade and payments, De Roover sustains the thesis of economic decline during the Renaissance, echoing his earlier work concerning balance-of-payments difficulties between the North and Florence. He makes explicit the labor-saving character of the *torcitoio* or silk-thrower, stating that it "replaced several hundred throwsters doing the work by hand." The importance of the Rome branch, producing more than 50 per cent of aggregate earnings until 1435, and the involvement of the Milan branch with ultimately ruinous loans to the Sforza court further suggest a certain noneconomic undertone in the establishment even during the period of its prosperity. In the sad period of decline following the death of Cosimo in 1464, this undertone becomes the dominant theme, reiterated in each branch. The diversion by Lorenzo of 53,643 florins from his minor cousins and of 74,948 florins of public funds for his personal use emphasizes the noneconomic character of the bank in its later years. The magnificence of Lorenzo is in contrast to the state of the bank.

The book is an invaluable contribution to the economic history of the period. Minor difficulties of style and occasional excessively complex passages should surely be excused as they originate in the difficulties of the material itself. De Roover has rendered a service to the profession by making the Medici records available. He has written a splendid book.

HARRY A. MISKIMIN

*Yale University*

*The Cradle of Colonialism.* By GEORGE MASSELMAN. New Haven and London: Yale University Press, 1963. Pp. viii, 534. \$10.00.

To produce a thoroughly interesting book on early modern colonialism, take a base of competent research in political and economic history, add some biography, and flavor with good writing. This is George Masselman's recipe. And a good one it is. In result, this study will be read with both profit and enjoyment by scholars as well as those ubiquitous "intelligent laymen."

Contending that modern colonialism owes its origins primarily to the Dutch, who "were the first to apply the principle of capital investment to overseas ventures, creating in time the Dutch Empire," Masselman concentrates his attention upon the activities of the Dutch in the East Indies during the early seventeenth century. Fortunately, his narrative begins with a brief but very informative and entertaining account of the salient features of the history of the Netherlands before then. He traces the evolution of economic and political institutions and the rise of the merchant class to a level of prominence and importance it did not achieve until considerably later in other countries. These developments, coupled with advancements in shipbuilding, navigation, and geographic knowledge, enabled the Dutch to push into the East Indies.

Piracy, warfare, intrigue, and other forms of high adventure marked the earlier years of Dutch penetration into the East Indies as they drove out the Portuguese and thwarted British attempts to gain a foothold there. Masselman describes, in considerable detail, skirmishes, battles, and confrontations of the rival merchant adventurers during this romantic period.

Of particular interest to economic historians is his account of the formation of the United East India Company in 1603. This monopoly was conceived mainly as a desirable solution to problems created by virtually unfettered competition which forced up the prices paid for spices in the Indies and depressed them in Europe. It was to have a long, colorful history ending late in the eighteenth century.

One of the Company's most outstanding employees was Jan Pieterzoon Coen, considered by many to have been the greatest Governor General of the Dutch East Indies. Obviously impressed by the unusual qualities of this man, Masselman deftly intertwines a biography of Coen with a history of concurrent related business and economic developments. His engrossing account of Coen's career clearly supports his thesis that Coen saw the Indies as a potential colony, while the directors of the United East India Company were concerned almost exclusively with the Company's affairs, very narrowly conceived. They failed to see that, in the long run, the survival of their firm was dependent upon the broader economic and political developments envisioned by Coen.

Coen foresaw the desirability of permitting other Dutch firms to engage extensively in business in the Indies, but the directors and the Dutch government were opposed. In fact, Masselman notes, the directors clung stubbornly to this position until the Company failed near the end of the eighteenth century.

In an epilogue, Masselman very briefly describes some significant events which occurred between Coen's death in 1629 and the acquisition, by the Dutch government, of all the Indonesian possessions of the United East India Company in 1798. He concludes that the Company's unswerving insistence upon maintaining its monopoly position helped to bring about its ignominious demise. Other developments, perhaps, played more immediate roles. For example, on the one hand, the Company incurred what ultimately proved to be unbearably heavy debts, as it assumed military and other governmental obligations to protect its interests, while, on the other hand, its revenues were reduced by employees who smuggled company merchandise for their own private gain.

The restrictive, monopolistic practices of the Company prevented the rise of a Dutch middle class in Indonesia. They limited migration to the Indies and severely proscribed the business activities in which noncompany, fellow Hollanders could engage. Meanwhile, according to Masselman, the Chinese "gradually began to weld the bond between the Dutch and the Indonesians" and "were thoroughly entrenched" in the many enterprises essential to a viable economy when, in the nineteenth century, Dutch policy changed, opening Indonesia to exploitation by trading and plantation companies.

To conclude, as Masselman does, that at that time "it was too late to create a Dutch middle class," may be correct, but the reader might well ask for the evidence which Masselman does not provide. This illustrates a kind of criticism that an economic historian would make, but, fortunately, at only a few points in Masselman's rather lengthy account. However, this in no way modifies the views expressed in my opening paragraph.

WYTZE GORTER

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*The Economic History of Modern Italy.* By SHEPARD B. CLOUGH. New York and London: Columbia University Press, 1964. Pp. xv, 458. \$10.00.

Books dealing with economic history can to different degrees meet the needs of readers with varied interests. The value of such books depends, therefore, not only on the amount of information they contain, but also on the manner in which it is presented.

Professor Clough's approach is descriptive, and in the reviewer's opinion, excessively so. Although the wealth of facts that he has collected is as a rule put into organic relationships, the book does not abound with insights, analytical interpretations, and incisive judgments. Hardly any unanswered question is asked. Perhaps it was the author's intention not to draw all of the many synthetic conclusions that as comprehensive a study as this would normally invite. Nevertheless, the detailed data given in the book will prove useful in international comparative studies.

Clough's story begins with the first decades of the nineteenth century and the "age of mobility" that Italy reached through intensive construction of railroads financed by both foreign banks and government budgetary appropriations. Whatever impetus new transportation facilities imparted to the coun-

try's economic growth, the latter's momentum was lost in the middle of that century, largely due to political and military activity preceding Italy's unification in 1861. (Clough refers to it as "the first Italian miracle," which it probably was in view of Italian internal political turmoil.)

In the following decades economic development was resumed and came to depend, as it must in low-income countries, on forced saving (*corso forzoso*). This time it was the textile industry along with shipbuilding and, later, metallurgical production that received primary attention and expanded under governmental tutelage and with the assistance of foreign investment. The author exposes fully the conditions under which "both industrialists and agriculturalists became bedfellows in demanding succor from that agency most readily available to both—the state" (p. 115). Thus one can conclude that nothing has basically changed over the span of decades. In the past as now, there have been two principal reasons for the protection that Italian producers demand: the nation's meager economic resources and the pressure of foreign competition (to which, of course, structural unemployment must be added). By implication, Clough makes a convincing case for the necessity of national governments to assume responsibility for the economic development of new countries and their infant industries.

Among the author's noteworthy items is the fact that the South, the development of which has, since World War II, attracted much international—as well as domestic Italian—attention, has been subsidized governmentally ever since the country's unification. The *Mezzogiorno* "never got less than its percentage contribution to state tax receipts" (p. 169) and was the object of laws passed as long ago as 1904 and 1906. Even during the decade 1871-80 the South received 45.6 per cent of total state expenditures for public works while contributing only 30.4 per cent to tax revenue. Although the Fascist regime never considered adopting a consistent policy to accelerate Italy's economic growth, it was generous in appropriating funds to the development of the southern region.

Clough's account of the aftermath of World War II makes interesting reading, even if it does not seem to do justice to the book's over-all balance. Here, as in other places throughout the book, one fails to see the forest for the trees because of the flood of details. In the chapter on fascism, too much space is devoted to political factors and relatively little to its economic policies ("a tragedy of statesmanship"), although it is these that the chapter's title purports to take up. Description of Mussolini's personality and tactics puts in doubt not only the political wisdom of the king and the entire nation but also the prudence and motives of J. P. Morgan & Co., which in 1925 lent to Italy \$100 million and extended to her revolving credit. Did this and British financial assistance constitute the basis for Mussolini's foolish policy of maintaining an overvalued lira—for prestige reasons—for almost a decade? And was this latter an aspect of the dictator's political quixotism?

"The Second World War and Its Immediate Consequences" (Ch. 8) must be read to appreciate the heavy (and unnecessary) cost to Italy of her partnership with Hitler. She was prepared to wage the extravagant war against the comparatively weak Ethiopia but was overwhelmed by the war on three fronts

to which Germany forced her. Equally hopeless was her position with regard to German economic exploitation during the country's wartime occupation.

Clough's discussion of Italy's postwar reconstruction and welfare state sets forth many of the facts that have become widely known and have earned the country's economic progress the attribute of "a miracle." The story is presented without any argument and without extensive comment, which at least the Vanoni Plan and a few other attempts aimed at an organized solution of Italy's structural economic defects deserve. To the extent that the book does not generally leave room for intensive treatment of technically economic issues, it disposes, altogether too briefly, of the system of Italian capital budgeting, the role of taxation in relation to economic growth, and the country's participation in the European Economic Community, to name a few topics.

Resourceful as this historical book will be found by all students of the Italian economy, it may induce some readers to consider the question of what to expect of similar studies now that both economic thought and policies are committed to the ideas of economic growth and international integration. It seems that after the whole field of economic development has become conditioned for Rostow's stages of growth (if this method has not already become preferred), mere description is outdated and there is an unprecedented need for analytical inquiries capable of shedding new light on both past and future factors which have either facilitated or obstructed economic development of individual nations.

KAREL HOLBIK

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*Economic Trends in the Soviet Union.* Edited by ABRAM BERGSON AND SIMON KUZNETS. Cambridge, Mass.: Harvard University Press, 1963. Pp. xiv, 392. \$9.75.

This book of eight major essays on Soviet economic growth represents the last story of a monumental structure that took over a decade to build. With the exception of Simon Kuznets, whose wisdom and long experience in the measurement of growth were the most fitting qualifications for comparing Soviet and U.S. performance, all the contributors are distinguished students of Soviet economics; most of them have been active in the field since Abram Bergson laid the foundations of his national income project in the late 1940's.

In addition to the main contributors (Bergson, Warren Eason, Norman Kaplan, Raymond Powell, Gale Johnson, Janet Chapman, and Franklyn Holzman), a number of economists who worked on the lower stories of the structure were present as discussants at the Princeton Conference of May 6-8, 1961, at which these essays were delivered. Since the index at the end of the book is signally incomplete, it may not be amiss to list the scholars whose work either contributed directly to the finished products in the volume or formed a link in the elaboration of the underlying data. Among those who also served in this grand enterprise may be mentioned Roman Bernaut, James Blackman, Robert Campbell, Alexander Erlich, Walter Galenson, Raymond Goldsmith, Alexander Gerschenkron, Gregory Grossman, Donald Hodgman, Oleg Hoeff-

ding, Hans Heymann, Jr., Holland Hunter, Naum Jasny, Arcadius Kahan, Jerzy Karcz, Adam Kaufman, Joseph Kershaw, Richard Moorsteen, Warren Nutter, Nancy Nimitz, Dimitri Shimkin, and Lynn Turgeon.

Needless to say, Bergson was the chief architect: on the one hand, his own contribution to the volume, together with his major study of national income published by Harvard University Press in 1961, synthesize a good deal of the material in the other essays devoted to specific sectors and components of the Soviet economy (labor force, capital stock, industry, agriculture, consumption, and foreign trade); on the other, his concepts of measurement and his theoretical notions permeate the work of his collaborators. However, as Bergson is the first to acknowledge, there has been theoretical feedback from some of his talented disciples, including notably Richard Moorsteen, whose work on index numbers influenced Bergson's thinking.

The volume under review marks the substantial progress accomplished since the Arden House Conference of 1952, the proceedings of which were published in *Soviet Economic Growth*, also edited by Bergson. The advance is due in part to more intensive processing and analysis of existing data but also to the vastly increased flow of Soviet statistics, especially for the postwar period. (In 1952, Grossman had no more to go on for his tentative estimates of postwar growth in Soviet national income than a prewar base and changes in the consumption of mineral raw materials and fuels, freight traffic, and nonagricultural employment.) The astonishing thing is that some of the estimates on prewar growth came as close as they did to the more refined efforts of a later day. Grossman, on the basis of Jasny's raw data, had found that national income at factor prices of 1937 rose by 57 per cent between 1928 and 1937; Bergson arrives at an increase of 49 per cent in net national product during the period, also at 1937 factor cost (p. 4).

The essays by Bergson (national income) and Powell (industrial production) confirm the paramount importance for the measurement of growth of the "Gerschenkron effect": series aggregated by means of early-year (i.e., preindustrialization) weights tend to grow appreciably faster than series aggregated by means of later-day weights (because capital goods that were relatively scarce in the Soviet Union in 1928 exhibited faster-than-average growth during the five-year plans along with decreasing relative costs). *Par contre*, Johnson, in his essay on Soviet agriculture in the present volume, concludes that weights of alternative years had little effect on the measurement of farm output.

One of many novel aspects of the present study is the attempt to calculate "total factor productivity" by comparing the growth of conventionally aggregated inputs with actually recorded outputs. This has been done by Bergson for net national product (inputs: employment, reproducible fixed capital, farm land, and livestock), by Powell for his index of "final industrial product" (inputs: labor, fixed capital, inventories, purchases from agriculture, and transportation), and by Johnson for agricultural output (inputs: labor, farm land, purchases from other sectors, capital and livestock). As we should expect, the growth of total factor productivity is greatly influenced by the choice of price-weights for the outputs. Thus, for the period 1928-58, Bergson finds that the

ratio of net national product to aggregated inputs rose by 1.2 to 1.7 per cent per year (depending on the method of aggregating inputs) when national product is measured at 1937 prices and by 3 to 3.5 per cent per year when it is measured as a "composite" (in comparing each year with the base year 1937, output is computed at given-year values). Powell arrives at growth rates of residuals for industry during the same period equal to 1.3 to 1.7 per cent (depending on the rate of return on capital selected) for an output index in 1937 prices and 3.4 to 4.0 per cent for an index using 1928 prices. In agriculture, according to Johnson, total factor productivity, however measured, grew by less than one per cent a year between 1928 and 1959 (the exact results depend crucially on the measurement of labor inputs). For want of data on total factor productivity in the construction and transportation sectors, neither of which is given separate treatment in the present study, we cannot reconcile the calculations for individual sectors with the results obtained by Bergson for net national product as a whole. If, nevertheless, Bergson's results appear rather high, as compared to a weighted average of total factor productivities in industry and agriculture, the reason probably lies in the treatment of labor inputs in the various calculations. Bergson treats the entire labor force as a homogeneous input into national product, whereas Powell and Johnson in effect assign different weights to industrial and agricultural manpower. If a composite input index for the industrial and agricultural sectors were calculated, the aggregated labor inputs for this index would grow faster than Bergson's labor series, inasmuch as the industrial labor force, which grew much faster than the agricultural labor force, would receive an appreciably larger weight than the latter. Bergson recognizes the problem but argues convincingly that "measures of total factor productivity are still of interest if . . . they reflect qualitative changes in labor" (p. 17). Another complicating factor in such a reconciliation is that intersectoral flows, which appear as inputs in the Powell and Johnson indexes, cancel out for the economy as a whole. A reduction in material-input coefficients in agriculture will of course increase the share of industrial gross output available for end uses (i.e., will raise national product), but delicate weighting problems would have to be solved to predict the effect of such a sectoral improvement on total factor productivity for national product as a whole.

There are other reasons as well why the computations of Bergson, Powell, and Johnson are not altogether comparable. Powell and Johnson build up their capital index inputs by adding depreciation to interest charges on net assets to convert the capital stock to a flow. (Powell experiments with alternative interest rates equal to 8 and 20 per cent.) Bergson's index, if I understand his description correctly, is an index of the fixed capital stock, net of depreciation, in keeping with his measurement of net national product on the output side. The Powell-Johnson method is tantamount to weighting the physical stocks of different capital goods by their marginal value products gross of depreciation. (The value of an asset in the capital stock reflects the discounted stream of present and future marginal value products, which are determined in part by the length of its service life, whereas the desired input weight is the marginal value product of the asset during the year where it appears as an input

in the production function. At equilibrium, the gross marginal value product of an asset should equal interest charges on its total value plus depreciation.) To the extent that Bergson's procedure for measuring capital inputs approximates weighting physical assets by their marginal value products *net of depreciation*, it assigns a heavier weight to long-life assets than the Powell-Johnson method.

Another element of disparity is that Bergson and Johnson, for the most part, combine their inputs arithmetically, while Powell combines his geometrically. (Both Bergson and Powell, however, present some of their results with geometric and arithmetic weights respectively.) Since arithmetic weighting implies an infinite elasticity of substitution between inputs and geometric weighting an elasticity equal to unity, one would expect that an index of aggregated inputs with base-year weights would always come out larger when inputs are weighted arithmetically than when they are weighted geometrically. Hence total factor productivity should always come out larger when inputs are weighted geometrically than when they are weighted arithmetically. (These expectations are confirmed by Bergson's calculations for 1950-58 on a 1937 base on page 22.) Johnson's residual must, to some extent, be underestimated, since capital, labor, and land used in agriculture must have less than an infinite elasticity of substitution.

While the calculations of total factor productivity in the volume are valuable as they stand, there is room for further refinements of concepts and methods. Theoretical analysis is called for to gauge the effect of aggregating heterogeneous outputs on the calculations of residuals; there ought to be more study in the same connection of increasing and decreasing returns to scale as well as of substitution elasticities smaller than one. Powell suggests the likelihood of small elasticities "in the short run and for such extreme changes in factor proportions as occurred" (p. 173). It should also be possible to isolate other variable inputs in the aggregate production functions and thereby explain a larger proportion of the growth in observed outputs. (This is being done for human-capital inputs by Nicholas DeWitt.) A more comprehensive coverage of inputs would lend more credence to the widely held belief that the unexplained residuals represent the contribution of technological progress and increased efficiency to growth.

Franklyn Holzman's essay on Soviet foreign trade is imaginative in its treatment and offers the reader stimulating theoretical insights. The remaining papers—by Warren Eason on the labor force, by Norman Kaplan on the capital stock, and by Janet Chapman on consumption—while they present less theoretical interest, are all, as far as I can judge, impeccable in the quality of their scholarship and highly instructive both with respect to the Soviet economy per se and for the comparisons they draw with U.S. experience.

All those readers who, like myself, are consumers of statistics on Soviet economic growth rather than fellow workers in the field should be grateful to the eight expert contributors to the present volume for this work of intelligence, love, and patience.

JOHN M. MONTIAS

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*Education, Manpower and Economic Growth: Strategies of Human Resource Development.* By FREDERICK HARBISON AND CHARLES A. MYERS. New York: McGraw-Hill Book Co., 1964. Pp. xiii, 229. \$7.50.

The heavy emphasis of a decade ago on capital as the prime mover in economic development has gradually been supplanted by recognition of education, public administration, political leadership, economic organization, and technology as other major elements in economic growth. The present volume, then, is in the mainstream of the new orthodoxy.

While the main concern of the book is with formal education, the authors note that the concept of human-resource development is a good deal broader than this. Considerable attention is given to on-the-job training of people already employed, and to the structure of incentives which motivate people to acquire skills and allocate them among alternative occupations. There is interesting documentation of the fact that in many of the less-developed countries the wage-salary structure has a perverse effect on manpower allocation.

The countries of the world are first sorted out into four educational levels. The criterion used is enrollment in secondary and higher education, calculated as a percentage of the relevant age groups. The stock of educated persons would doubtless be a superior criterion, but stock figures are unavailable for many countries, and current flows are substituted. Educational level, as one would expect, turns out to be strongly related to national income per capita. Level I (underdeveloped) consists almost entirely of African countries. Level II (partially developed) includes mainly countries in Asia and Latin America. Level III (semiadvanced) is a mixture of Asia, Latin America, and the poorer European countries. Level IV (advanced) is the rest of Europe, North America, Australasia, Israel, Argentina, and Japan.

The heart of the book is a series of four chapters, one for each level, which first examine the manpower-resource situation in the countries concerned and then develop a set of recommendations for manpower strategy. The main emphasis in both analysis and prescription is on the formal educational system. To this reviewer, at least, the authors seem to have substantiated their thesis that the problems of educational strategy are qualitatively different at each level, and that in consequence the points at which effort needs to be applied and the desirable allocation of resources among educational levels will also be different. At the same time certain themes recur in successive chapters—notably the dilemma of quantity versus quality, and the key role of secondary education as both the bottleneck through which students must pass to reach higher education and the main source of teachers for feedback into the primary school system. In this connection it would have been interesting to see some use of formal programming methods, with values attached to the human inputs and outputs concerned, and with strategy deduced from some optimizing principle. While these methods have their own limitations, they can provide a useful check on informal intuitions and judgments.

The book concludes with a sophisticated commentary on the problem of setting human-resource targets and of relating manpower planning to over-all development planning. The authors correctly emphasize the inaccuracies inherent in any mechanical projection of occupational demands, the looseness of

formal educational requirements for most occupations, the fact that demand can adjust to supply as well as vice versa, and the fact that prospective short-falls can usually be met in a variety of ways.

This is a major effort in a major field. Its limitations derive mainly from the very large canvas on which the authors were trying to paint, and from the scarcity of quantitative information as well as the difficulty of quantifying policy objectives. Its policy proposals rest on a great deal of observation, discussion, and reading of unpublished government surveys and reports. No one can safely set himself up as either a student or adviser on educational policy without having digested the contents of this book.

LLOYD G. REYNOLDS

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*The British Economy.* By SIR ROY HARROD. New York: McGraw-Hill Book Co., 1963. Pp. 240. \$5.95.

All those who know Sir Roy Harrod or who have read his contributions to economic theory or his biographies of Keynes and Cherwell know that his is a mind distinguished but idiosyncratic, speculative but unstatistical, creative not commonsensical, candid but not judicious. A mind justly acclaimed, but singularly ill-fitted, one would have thought, to provide a short balanced account of the British economy for the student and intelligent layman, especially those outside Britain. And so it has proved to be.

Popularization, though usually written with a light heart and the left hand, actually carries heavier responsibilities than writing for one's colleagues. There is little danger of pulling the wool over *their* eyes, as most of us only too frequently discover; but the intelligent layman is almost defenseless, especially when softened up by an array of credentials such as Sir Roy trails behind him. If a review is to provide the nonexpert reader with any kind of defense, hard words must be used wherever hard words are called for; carping and faultfinding should be in direct, not inverse, proportion to the fame and distinction of the author of the book. Such at any rate is the belief in which this review is written.

Perhaps the first thing that strikes one on reading the book is the curious allocation of space among topics. Out of 240 pages, the industrial structure of the economy, trends in expenditure, output, productivity and employment, regional and industrial concentration, and government policies towards monopolies and restrictive practices occupy 18 pages. On the other hand, there are 67 pages on sterling and the monetary system. These provide technical and administrative detail in a profusion that most lay readers would find boring, or incomprehensible, or both. Worse, they are positively misleading because the weapons of fiscal policy are, in comparison, hardly discussed at all. Thus the impression is given that in controlling the economy it is monetary not fiscal policy which has been dominant. Not only is this, of course, quite false, but it will mislead the U.S. reader in a particularly unfortunate way. Perhaps the single most interesting point for an American about the British economy and its management is the possibility of flexible, ingenious, and ef-

fective fiscal policies that British parliamentary and governmental institutions allow.

Easily the best thing in the book is the 30 pages on socialism. Here Sir Roy does what he so conspicuously fails to do elsewhere: he bears in mind the *kind* of ignorance and preconception which an uninformed foreigner is likely to have about the British economy, and he outlines the actual position both justly and persuasively. The way in which he puts the nationalizations of 1945-50 into perspective could scarcely be bettered.

The rest of the book is devoted to the advocacy of a number of theses close to Sir Roy's heart. It might be argued that a book of this kind is not the place for controversy; but the controversy could be made acceptable and stimulating if the evidence were carefully marshalled and if the reader were given some guide as to what was generally accepted and what hotly disputed. Such guidance is most emphatically not provided. There is no bibliography of any kind. More striking, there is only one (very passing) reference anywhere in the text or footnotes to the work of *any* living economist apart from Sir Roy himself. No sources are given for any of the tables in the book: there is simply an unhelpful omnibus footnote at the beginning, listing various official publications used in the work. Thus it is impossible to check and assess properly all the arguments, but one's confidence may be shaken by the fact that in Tables 4 and 5 an elementary howler in comparing U.K. and U.S. statistics has produced serious errors in the figures—serious in the sense that the true figures support exactly opposite conclusions to those which are drawn, from the wrong figures, in the text.

This absence of documentation means that Sir Roy can produce the most bizarre obiter dicta. What can he mean, for example, by the remark on page 182 that "the task of ironing out the business cycle . . . has been pursued more faithfully in the United States than it has in the United Kingdom"? It also enables him to ride his hobbyhorses roughshod over his readers. He believes and argues at great length that the British economy has still not fully recovered from the war because exports have not yet "caught up" with imports. This theory, used to criticize the government for having abolished import restrictions by 1960, he bases on an uncritical acceptance of the prewar position as a norm; an even more uncritical faith in the ability of volume and price series for foreign trade over 25 years to bear the weight of precise numerical comparisons; and a bland disregard for index-number problems. He believes that many of the U.K.'s economic troubles were due to the devaluation of sterling in 1949. This is of course an arguable position; but he does not discuss the main difficulty in arguing for or against it: the Korean War broke out only nine months later, and the effects of this immediately swamped those of the devaluation. This fact is relevant, too, to his discussion on inflation. He outlines a theory to explain the course of inflation in the United Kingdom since the war, claiming rather disarmingly to provide not "an exhaustive analysis, but rather a challenge to research in the United States no less than in the United Kingdom." Not only does he not mention by name any of the many people who have done a great deal of research on this subject, he refers to none of their results and in fact contrives to give the impression that he is un-

aware of what has been done. Since his own approach is ham-handed and amateurish and yields, as he himself admits, some odd results for certain years, the whole business is rather disturbing.

C. W. McMAHON

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*The Economics of Take-off into Sustained Growth. Proceedings of a Conference Held by the International Economic Association.* Edited by W. W. Rostow. London: Macmillan; New York: St. Martin's Press, 1963. Pp. xxvi, 482. 63s.; \$14.95.

Near the end of the 1960 International Economic Association Conference at Lake Constance on "The Economics of Take-off into Sustained Growth," almost everyone spoke the lingo. Discussants of Kenneth Berrill's paper on "Foreign Capital and Take-off" held that international capital had set up preconditions; developed leading sectors; established progressive, self-financing institutions; and had bridged the gap from a 5 to a 10 per cent savings ratio (pp. 460-64). Yet not one of the authors of the 15 papers other than W. W. Rostow's own accepted his theory of the take-off. Rostow wondered if the rejection occurred because "the introduction of a new concept—especially a new term—is an act of aggression against respected colleagues and friends" (p. xiii). But the terms were not rejected as merely impudent robes for manifest principles. On the contrary, the terms have spread with a success comparable to "multiplier" and "liquidity preference" a generation earlier. At stake is the validity of the theory under the robe of terms and ornamental illustrations.

This long-awaited report of the conference has four parts. First come two general papers, by Rostow and Kuznets, on the merits and demerits of take-off theory. These are followed by attempts to identify take-offs for six countries: the United States (Douglass North); Britain (H. J. Habakkuk and Phyllis Deane); Germany (Wolfram Fischer and Walther Hoffmann); France (Jean Marczewski); Japan (Shigeto Tsuru); and Russia (Alexander Gerschenkron). In the third part, seven papers explore functional aspects of economic growth as related to the take-off hypothesis. Harvey Leibenstein contributes two, one on population growth and another on technical progress. Mogens Boserup and Octavio G. de Bulhões each discuss agriculture. Treatment of capital formation as a whole by A. K. Cairncross is followed by Paul Cootner's paper on social overhead capital and Kenneth Berrill's on foreign capital.

Last comes a 175-page summary record of the debate, prepared by Douglas C. Hague. Illuminating are the introductory comments to some papers, the reactions of Rostow, the final statement by each paper's author, the interim summing up by David Landes, and the reflections at the final session by Robert Solow. But the style of compressed indirect quotation makes at least half the debate unworthy of preservation: "Professor A thought B's estimates of  $x$  too low and  $y$  too high, but in Professor C's view both  $x$  and  $y$  were too high, while Professor D wondered if  $x$  and  $y$  were really as important in underdeveloped countries as in Europe." One gets the feeling of watching tropical fish through frosted glass.

## I

Few of us doubt that modern industrialized economies move in patterns different from preindustrial, traditional economies and that they have therefore gone through some process of transition. But did this process show an unusually marked acceleration in one phase; and, if so, did the acceleration have Rostovian take-off characteristics? At the conference none agreed altogether. The authors on Britain and France thought growth had been a long, steady climb with wars, international trade, and general economic conditions more important determinants of progress than novelties in leading manufacturing sectors. For the United States, Germany, Japan, and Russia, however, some accelerations could be distinguished in scale and significance from other cyclical recoveries. But only Hoffmann writing on Germany sees definite stages of preconditions, take-offs, and sustained growth while nevertheless rejecting the concept of leading sectors. Emphasis on general conditions, especially domestic demand, make North's explanation for the United States similar to those for Britain and France. Gerschenkron identifies a "great spurt" of Dahmen development block growth in Russia after 1885, a view that has much in common with Rostow's sectoral scale and spread effects. Otherwise his theory of the "strains of backwardness" differs sharply from Rostow's, especially in rejecting the preconditions stage. Finally, Tsuru provides a rather Marxist Japanese take-off centering around new social mechanisms for raising invested surplus from 5 to 10 per cent of national income. All writers except Tsuru insist that if they absolutely had to pick a take-off period, it would be earlier than Rostow's, by 5 years for Russia, 10 for the United States, 20 for Germany, 40 for Britain, and 80 for France—at least an orderly progression!

Setting back the initial dates removes a bit of the sting from Kuznets' failure to find much acceleration in aggregate income and investment growth during the periods chosen by Rostow. The data get hazier as one goes backwards in time, disabling alike the friends and foes of take-off. But the gradualists, Kuznets, Habakkuk, Deane, and Marczewski, undo themselves by working both sides of the faint trail. Leading manufacturing sectors, they say, on the one hand, could not cause a discontinuity in growth without affecting aggregate trends. Yet on the other they show that the slow national acceleration mainly reflects different things anyway: trends in international trade, wars, agriculture, and general domestic conditions. But to the very extent that these were important, key sectoral developments in manufacturing had to have repercussions less noticeable in the aggregate. A bold passenger comes to the cockpit in a thunderstorm and takes over the stick without immediate, visible effects on the speed and direction of flight. The real trouble with Rostow's theory is that it cannot predict which passenger will come forward and when. Leading sectors are identified *ex post facto*. To do better one must, as Solow put it, satisfy the great "appetite of a disaggregated economic model for parameters" (p. 471). After all, ten sectors take a hundred numbers, all changing, some very rapidly. So far, therefore, talk of spread and scale effects has not really given us anything more operational than the familiar literary rivalry of the champions of cotton textiles with the champions of rails and steel mills.

When pressed for quantifiable initial conditions, parameters, and rules of behavior connecting key variables, Rostow usually brings up the institutional complexity of growth, especially the role of entrepreneurs and the state. "Growth analysts cannot and should not cut their labors down to the level of simplification, aggregation, and abstraction which the tools of economic theory require, any more than they should restrict themselves to the consideration of those variables only for which reputable statistical measures exist" (p. xxiv). Since much historical evidence is irreducibly complex and unmeasurable but nevertheless trustworthy, one must agree. But by the same token one abandons hope for finding the preconditions in one chronological layer, and in others the emergence and spread of growth-reinforcing institutions. Since one cannot measure and predict the factors that must precede industrial growth, the notion of a preconditions stage suffers, in Gerschenkron's view, "from the double curse of non-operational conceptualization and useless tautology" (p. 166). Several participants concur, but Landes points out that, after all, "growth had to be preceded by a conditioning process of some kind, and this was susceptible to scientific analysis" (p. 394). But if part of this process was the birth of the "Newtonian outlook" in Europe (seeing the physical world as manageable according to a few fixed laws—Rostow's definition, pp. 10, 21, 312), what were the preconditions for that? Can one avoid going back to the peculiar medieval dissonance of half-assimilated Christian, Roman, and Teutonic institutions supplied by an already unusually dynamic technology in plowing, milling, textiles, metallurgy, and other crafts? At least Marczewski wants to go back to "the causes named by general historians a long time ago": wars depressing a potential for growth dating back to the Middle Ages, a potential reinforced by overseas discoveries, the advent of merchant and financial capitalism, etc. (pp. 137-38). This makes good sense and allows more realistic comparison of European growth with Asian and African turbulence and stagnation today. One must depart further from Rostow's stages than either going back to the familiar Industrial Revolution idea or on to Kuznets' substitutes, "the late pre-modern phase" and the "early growth phase" (pp. 42-43).

So little is left of final self-sustaining growth that "self-" drops out of the book's title. Kuznets asks the right questions: In what sense was growth *not* self-sustaining during the take-off? Does not all growth have self-limiting effects? Does it mean that these are finally outweighed by self-sustaining impacts? Does generating those not always mean a struggle? (pp. 39-40). Rostow had already agreed, "There is, then, nothing automatic and easy about the inner mechanics—the logistics, as it were—of sustained growth. [But] on present historical evidence, it appears fair to say that the larger psychological, social, technological, and institutional changes required for a take-off are such as to make it unlikely that we shall see a true lapsing back" (pp. 10-11).

## II

Among the functional papers, Cootner's on social overhead capital is brilliant, an example of what growth analysis should be, combining history and

theory with high relevance for policy. Transportation, power, and communications are denied a special prerequisite role but are made an integral part of economic expansion, necessary but themselves induced. Cootner shows that previous conclusions depend less on assumptions about immobility, scale, gestation, and durability than on objective uncertainty and imperfect foresight. Both uncertainty and scale, according to him, affect social overhead expansion less in industrialized than in primary-producing and -exporting nations. Development of material-exporting countries depends on diminishing returns in resource exploitation in advanced nations. Hence, the analysis can explain the out-of-phase alternation of overhead investment in the two types of area, presumably still evident in Labrador and Venezuela. At the same time, for countries developing via import substitution, it puts Hirschman's case for lagging social overhead spending there in a broader framework.

Another good paper is Berrill's demonstration that foreign capital played virtually no part in the take-offs of Britain, France, Germany, Finland, and Japan. Timing suggests that foreign capital was not an initiating factor in the United States, Canada, New Zealand, Australia, Sweden, Norway, Denmark, and Russia. In any case, it was only a "small" fraction of total saving, about one-fifth (p. 286), and not nearly as important as the coming of foreign technicians and machinery (pp. 291, 466).

The papers on capital, agriculture, population, and technical progress explore whether there is something about their behavior that is consistent with a discontinuous acceleration at the beginning of industrialization, and if so, whether for all or only under a few circumstances. For capital formation Cairncross finds that differences in urbanization and prevalence of capitalist institutions limit the relevance of European records for Africa and much of Asia. Some increase, however, though not necessarily a rapid doubling, of the savings ratio must form part of the process of growth. But if this process is under way, with investment needed and remunerative, why should a physical shortage of capital persist? Cairncross believes the mechanism by which a financial bottleneck makes itself felt has been inadequately discussed because it is too much taken for granted (p. 247). He also finds that almost any relation between capital and income growth is feasible, with causation possibly running in either direction. Once he goes too far with this. His argument that "There is, of course, no necessary inconsistency between a rising incremental ratio and a constant average ratio" (p. 253) depends implicitly on measuring the two by different price indices or on using a gross definition for one and a net definition for the other.

Mogens Boserup's analysis of "Agrarian Structure and Take-off" may be, as in Hans Singer's view, "a landmark in the development of growth economics" (p. 428). The first part amounts to the position that agriculture will not lead in economic development, except perhaps for short periods, but that if ignored, it can drag down the whole process. The second part identifies four basic structural changes (British, Eastern, French, and Mediterranean) that stress the fate of the tiller and the kind of person who "takes care of the entrepreneurial function." Changing the traditional cultivator's outlook, conquering agriculture from within, is not as easy as British tiller eviction or East-European serf "emancipation." Since development in South Asia is likely to be no more

than a shift from "Mediterranean" share-croppers to "French" peasant owners, industrialization is likely to take longer than in Northern Europe.

But if Boserup shows how little speed agriculture will allow for industrialization, Leibenstein believes that population growth now demands higher speed. In Europe population was primarily induced and "raced" against capital formation and aggregate income growth. Advances in public health, etc., have made population growth more of an exogenously determined obstacle in underdeveloped countries, thus raising the initial hurdle that must be overcome and demanding a sharper take-off as the escape from stagnation. Unlike Cairncross, Leibenstein seems to link capital formation, income, and population too rigidly to one another, making any unfavorable position of this constellation augur "sustained stationariness." If the modernization and institutional changes that make "a true lapsing back unlikely" occur in one region of a country, among one group or more, or in a few sectors, then aggregate population growth and its effects can be misleading about lack of progress.

### III

On one important point Rostow had much support: his emphasis on technological progress. The regular flow of new production functions is the main criterion of take-off. As Landes put it, "The heart of the system was the analysis of technological diffusion via leading sectors and linkages" (p. 391). Other participants had their own version. Cairncross speaks of "the process of innovation that lies at the root of economic progress" (p. 241). Leibenstein begins his paper on "Technical Progress, the Production Function, and Development" by noting that very few would quarrel with the statement that "the technological inventions of the last few centuries are the prime factors that make it possible for a country to shift from low levels of income *per capita* to high levels of income per head" (p. 186). But what follows is an ingenious contrivance about a minor aspect of technical change in a setting of dubious realism but consistent with the take-off hypothesis.

Leibenstein seeks to prove that capital formation, factor substitution, technical progress, and the inducement to invest reinforce one another where capital is abundant but not where it is scarce. At the capital-scarce level of development practically no production alternatives are said to exist, and chances for developing new ones would be low even if producers had the knowledge to search or innovate. The reverse of all this fits where capital is abundant. With isoquant geometry one can then show that a lot, but not a little, more capital would be worth saving and investing: a take-off.

Leibenstein's view of technical progress is that of Adam Smith, a process of simplification. The division of labor makes for more specialized, simpler processes; and the simpler they become, the more easily can one see the next potential improvement. Finally when the capital-labor ratio is high, one can substitute nonhuman mechanical energy for human or animal energy (pp. 193, 425). But doesn't the addition of power tools and the like happen in workshops that in most industries are still at the labor-intensive end of the spectrum? Long before the latest capital-intensive levels are reached, will purely manual work have been reduced to a few auxiliary operations? Meanwhile, the other phases of production will have become more interrelated, rather than simplified, and

the specialized worker at his post will have less and less knowledge of the unfavorable repercussions, which have nevertheless grown more likely, if he introduces a change. Where volume is large enough and capital cheap enough, not subdivided but complex, balanced and integrated, continuous-flow processes will predominate.

Questions of scale are curiously left out of the paper, and in the discussion Leibenstein insists that, if he were to rewrite it, he would not bring in scale (p. 425). Yet volume is a much more important determinant of technique than relative factor prices—in the sense of comparing the plausibility of changes with the magnitude of effect. Beyond critical levels, the most advanced capital-intensive methods used anywhere are consistent with the lowest known wage-interest ratios. Entrepreneurs in practice face the question of capital-labor substitution almost exclusively with the simplest theory-of-the-firm question: "How much volume do I need to amortize the equipment?" Volume shares this role only in that comparatively small range of a few processes where mechanization is possible but adopted or avoided mainly because of effects on quality. Where extreme weights, temperatures, pressures, voltages, poisonous fumes, and similar circumstances are qualities essential to production, human participation is drastically limited. Since science is more likely to add than to remove these, capital-labor substitution tends to lose significance in the course of economic development. Moreover, scientific research mostly concerns that element which often looms largest in a firm's books, though cancelling in the aggregate: materials. Research on novel ways of creating, substituting, or treating materials is mostly justified by the hope of gains larger than those to be had from merely changing capital-labor ratios. Capital-labor substitution is therefore neither within the intent nor the direct capacity of a large portion of scientific progress, though even here it may occur as a by-product. Discussion of production functions diffused during early industrialization cannot realistically abstract from the effects of science and economies of scale.

Like other good conferences, this one discussed three kinds of papers: cogent recapitulations of years of scholarship, method-centered reports of work-in-progress, and new questions boldly posed. Like the first Zeppelins that cumbersomely took off at Lake Constance, Rostow's theory may soon be a museum piece. But the problem of the transition remains, as did once the problem of flight, and the contributions stimulated by Rostow are important markers on the runway toward solution.

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*Resources and People in East Kentucky: Problems and Potentials of a Lagging Economy.* By MARY JEAN BOWMAN AND W. WARREN HAYNES. Baltimore: Johns Hopkins Press for Resources for the Future, 1963. Pp. xxiv, 448. \$10.00.

The East Kentucky region studied in this volume is defined as the 36 counties east of the Cumberland escarpment. By contrast with the rest of Kentucky, it is poor. Compared with national per capita income and unemployment rates, it is depressed. East Kentucky's economic development was based largely on the

exploitation of its coal resources, wood processing, and subsistence agriculture. Declining employment opportunities in coal mining, depletion of the forest resources, and stagnation of the agricultural sector have resulted in a high level of unemployment, low per capita incomes, and heavy outmigration.

East Kentucky's plight is provocative of many questions. The region, containing 737,000 people, rather clearly has a comparative and absolute disadvantage in almost everything. The development of efficient, large-scale farms and/or of manufacturing on any significant scale would be economically inefficient owing to the topography of the region and to the high cost of transportation to outside areas. The educational structure is appallingly bad. Outmigration during the 1950-60 decade was at an annual rate of 3.68 per cent. Despite the high natural rate of population increase (1.97 per cent), population declined from 874,000 in 1950 to 737,000 in 1960.

This raises interesting questions with regard to the internal economic structure. What impact does declining population have on local industry? What happens to local asset values and the tax base? What policies would be most efficient in ameliorating the situation?

The facts of East Kentucky's distress are well known. Professors Bowman and Haynes present a study that "may best be described as an historical-ecological approach to the diagnostic analysis of underdevelopment." The volume is divided into two parts. Part I—the major part of the book—consists of a detailed examination of the economy of the region and is written by Mrs. Bowman; Part II contains a study of the East Kentucky coal industry, written by Mrs. Bowman and Mr. Haynes. The first three chapters are intended to provide an overview of the region's resources, economic structure, and historical development. Chapters 4 through 7 focus on coal, manufacturing, over-all postwar employment, and wood processing. Chapters 8 and 9 assess the likely future growth of employment. Chapters 10 and 11 are concerned with population, migration, and education. The final chapters of Part I evaluate policy alternatives. Part II traces the history of employment, output, and profitability of the East Kentucky fields. For those interested in selective reading, there are summaries at the end of each chapter.

Unfortunately, the book has several major shortcomings that render it less useful than enumeration of chapter headings would indicate. In the first place, the use of data leaves a great deal to be desired. Related to this, and even more serious, the framework of analysis is somewhat deficient.

In the 436 pages of text, there are 73 maps, 95 tables, and 68 figures. Not only does the reader become lost in a plethora of data, but frequently the figures presented are not as meaningful as fewer and alternative numbers might have been. For example, nowhere is the employment structure of the entire labor force given. Instead, insured labor force data for 1956 are presented. Even then, only the mining and manufacturing breakdown is given. These employees account for less than half of insured employment and less than one-third of the (smaller) 1960 labor force. In this case, as in many others, the entire state of Kentucky is used as a basis of comparison. Unless one is conversant with the national figures or with Kentucky's structure contrasted with the national, use of this basis leaves the reader with an inadequate frame of reference.

Even more fundamental, the data presented are not brought together in any meaningful over-all framework. There are no regional flows data, and no examinations of trade, service, or agricultural sectors.

Part of the reason for this inadequacy may be the awkwardness of a 36-county unit and the fact that the authors conducted much of their research prior to the publication of the 1960 *Census*. They did, however, have and utilize some data from the 1960 *Census*. Greater reliance even on the 1950 *Census*, as opposed to piecemeal sources, would have greatly improved the manuscript. *Census of Business* data could have enabled some analysis of the service and trade sectors.

Data problems and the more substantive limitations of the volume are closely interrelated. By this reviewer's calculations, 30 per cent of the 1950 and 20 per cent of the region's 1960 labor force were engaged in agriculture. There is, however, no chapter, or even major section, devoted to analysis of this troublesome, low-income sector. Indeed, the authors are finished with the subject at the end of Chapter 1 when they concluded that, "East Kentucky agriculture . . . offers no promise for the future." Failure to distinguish coal-associated (unemployment) poverty from agricultural (low-income) poverty leads to some disappointing gaps in the analysis.

Likewise, the treatment of outmigration leaves a great deal to be desired. In the first place, it is hardly mentioned in the three summary chapters, yet it is fundamental to any understanding of the region's economy. My computations suggest that the unweighted averages of county 1950-60 outmigration and natural increase rates show sharp contrasts between the counties classified as agricultural and those classified as coal counties. Outmigration proceeded at 4.98 per cent average annual rate from the coal counties and at 3.14 per cent from the agricultural counties. Yet no breakdown of behavior between the rural farm, rural nonfarm, and urban population was undertaken. Indeed, throughout the book, rather than analyses in terms of, say, "coal counties," "agricultural counties," and "manufacturing counties," to suggest but one possible breakdown, the text frequently degenerates to a county-by-county description of data. This not only results in confusion to the reader unconversant with individual counties, but also in *ad hoc* explanations of differences.

Similarly, the estimation of the region's future is deficient with respect to migration. Estimates of the 1976 employment gap are computed on the assumption of no net outmigration from 1960 onward. The prospects for East Kentucky involve a choice between raising per capita income and attempting to maintain population. The faster is outmigration, the higher will be per capita income, and conversely. Failure to stress this trade-off between population and per capita income is a fundamental defect of the policy section.

The policy section (Chapters 12 and 13) focuses heavily on the need to improve education. Although it is recognized that migration increases with educational attainment, no consideration is given to means of speeding outmigration. Rather, the authors suggest that Kentucky leaders resign themselves to the fact that outmigration will continue. Acceptance of the inevitability of outmigration would undoubtedly be a distinct improvement. One wonders, however, whether this goes far enough. The authors believe that area redevelopment and other depressed-area programs will not solve East Kentucky's

problem. If this is the case, speeding outmigration would appear to be the alternative. While educational upgrading is important in this regard, surely other measures might also be considered.

Similarly, there is little systematic inspection of local government revenues and expenditure needs. In view of declining population, the questions associated with a shrinking local tax base would appear to be an important policy question. Although the authors contend that East Kentuckians are reluctant to spend on education, they do not consider local revenue problems, aside from recognizing the need for outside financial assistance.

The treatment of coal-associated problems is far more satisfactory than the overview of the economy. While the focus on coal mining may have been overdone, the industry analysis in both Part I and Part II is distinctly superior to the regional analysis. National conditions in the industry, the instability induced in East Kentucky by the presence of many high-cost marginal operators, and the effects of union wage agreements on the region's segment of the industry are treated carefully and exhaustively.

Despite the shortcomings enumerated, economists who have engaged in regional research in other regions will find many points of interest. Contrasted with East Kentucky, the problems of most other regions experiencing net outmigration seem negligible.

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*Development with Stability—The Indian Experiment.* By S. L. N. SIMHA. Bombay: Vora & Company, 1963. Pp. vii, 131. \$2.50.

Mr. S. L. N. Simha, who has served the government of India in a variety of capacities, has written a slim little volume on India's recent economic development. For over a decade India has been committed to a type of development planning that has attempted to raise per capita income with the maximum price stability. The emphasis, according to the author, has been on "democratic development" with a mixture of public and private enterprise. And while both government regulation and the "free market" have been utilized to allocate goods and services, the three five-year plans have placed a great deal of responsibility on fiscal and monetary controls to stimulate the allocation of resources into capital accumulation. Simha concentrates his attention on these monetary and fiscal policies or, as he expresses it, "the financial aspects of the Indian development effort" (p. 11). Basically the book is a summary review of the three five-year plans and covers the years 1951 through 1962.

The first plan began in 1951 and was essentially a preparatory plan for those to follow; it owed much to the Bernstein mission of the International Monetary Fund. It was extremely modest and placed great emphasis on financial discipline and price stability. Taxes were raised and monetary expansion limited. Investment outlays in the government or public sector were relatively small (at least as compared to subsequent plans), and the general decline in prices indicated to the author that perhaps too little was done. However, the average increase in real national income per year of 3.5 per cent was "by no means an unsatisfactory performance" (p. 33). Perhaps less encouraging was the fact that during the last two years of the plan real national in-

come growth declined to 2.5 per cent and 1.9 per cent. No figures were available on unemployment or underemployment.

The second five-year plan covered the years 1956-61 and represented a somewhat greater effort. There was much attention paid to growth (public outlays were increased for a projected 5 per cent annual growth in national income) and less attention was paid to price stability. Bank credit expanded and there was a 32 per cent increase in the money supply. More foreign assistance was utilized (21.5 per cent of investment), which helped to offset the decline in export markets. At the same time that this expansion was under way, a series of taxes was imposed in an attempt to decrease consumption rather than profits. Interest rates were raised in an effort to increase the cost of capital, but not for the purpose of limiting the availability of bank credit. Wholesale prices rose about 30 per cent and the cost of living rose by about 24 per cent during this period. Unfortunately, the growth in real national income (4.3 per cent average) was only slightly greater than in the first five-year plan. As the author says: "[this was a] not too satisfactory rate of growth of real national income" (p. 80).

The third five-year plan has been designed for the period 1961-66 and, in general, appears to continue the policies established in the second plan. Bank credit has been allowed to expand about as rapidly as during the previous period. Interest rates have been raised and taxes have again been boosted in an effort to hold down the growth in consumption. Foreign assistance is expected to increase slightly to about 24 per cent of investment. Increased military outlays, due to the recent dispute with China over the India-China border, are expected to bring about even greater effort to raise taxes and curtail consumption. And, while only two years of the third five-year plan are covered in this review, the author points out that "the first thing that needs to be mentioned about the Third Plan so far is the disappointing rate of growth of national income" (p. 87). National income increased only 2.1 per cent during the first year of this plan, and the second year has been estimated at about 2.0 per cent. On the whole, however, the general rise in prices has been curtailed.

The author does not say whether or not India's rather orthodox financial policies continue to enjoy the support of the International Monetary Fund, but certainly the Indian government appears to have paid close attention to those rules designed to limit inflation, extract the maximum rate of savings from the economy, while at the same time attempting to provide investment opportunities for private capital. As the author points out, the rate of profit and dividends as a proportion of sales have shown significant increases, in addition to the rise in price of corporate shares. "[Expansion of private capital] owes a great deal to the atmosphere of dynamism and growth generated by the Five Year Plans" (p. 114). Salary and wage costs, on the other hand, have been held to a relatively constant proportion of sales.

Simha's own conclusions regarding India's policies for economic growth appear to be somewhat mixed. He feels that one of the lessons to be learned from India is that a "mixed system can work rather well, particularly in stimulating growth" (p. 128); and he appears to support the Planning Commission's statement that "it will be seen that over the past 10 years, the country has made considerable progress in every branch of national life" (pp. 128-29).

Nevertheless, he concludes by saying that "it is now generally admitted that the progress of the Third Plan so far is disappointing in terms of rate of growth of national income. The dynamism of the Five Year Plans appears to have been dormant for some time" (p. 129).

To this reader, Simha's review of the Indian five-year plans reveals the pathetic eagerness of the government to obey all the rules of sound finance and the tragic inability to make progress that would give some hope of raising one of the world's lowest standards of living to a tolerable level in any reasonable period of time. If this experience in India (a country presumably about ready for the take-off into sustained growth) is the best that orthodox finance can accomplish, it would seem doubtful that any other underdeveloped country will see here a serious plan for economic growth.

ARTHUR B. HILLABOLD

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### **Statistical Methods; Econometrics; Social Accounting**

*Méthodes statistique de l'économétrie.* By E. MALINVAUD. Paris: Dunod, 1964. Pp. xiv, 634. F 86.

"Statistical Methods of Econometrics" is in the tradition of Diderot and d'Alembert. (Fortunately, the printer has treated the present work with substantially greater kindness.) Following his predecessors, E. Malinvaud combines a careful critical survey of the field with many original contributions—some large, some small, but all illuminating. His style is vivacious and his text sparkles with a *clarté* which is only partly French and mostly Malinvaud.

The book is divided into five roughly equal parts: Part I gives a general introduction to econometric models and methods, including a first-rate geometrical introduction to simple regression and an entire chapter reviewing almost all recent work on the consumption function. Material on consumption is used to good effect to illustrate identification problems, the inconsistency of ordinary least-squares estimates of the marginal propensity to consume, distributed lag models, aggregation problems, and the like. Part I (and especially the chapter on consumption) serves to motivate the discussion of the remainder of the volume and to demonstrate the relevance to concrete problems of the varied body of techniques presented subsequently. Notable features of Part I are the extraordinarily clear discussions of "bunch map" analysis and its uses, of principal components analysis including its relation to orthogonal regression, and of recursivity. There is some discussion of the problem of model specification errors and the importance of finding statistical procedures which are robust in the face of such errors, but few conclusions relevant to subsequent discussion are drawn, apart from the general warning (p. 145) that the choice of a good model is as important as the choice of a correct statistical procedure.

Part II (four chapters) deals in detail with the general theory of linear multiple regression and tests of linear hypotheses. A geometrical analysis of simple regression is presented and extended, and results in an intuitive exposition of considerable clarity. Malinvaud here follows the work of the French mathematician and statistician, G. Darmois, whose papers do not seem to be

well known in this country. Advanced material, such as the Stein result on the nonadmissibility of least-squares estimators, is introduced in a nonrigorous but clear fashion at several points. Such simple but useful results as the standard error of a constant term in a linear multiple regression are derived. Zero-one variables are introduced, but unfortunately are not used to clarify the nature of the analysis of variance and covariance. Indeed, if this section of Malinvaud's book can be criticized, it is because he fails to use the obvious partitioning of zero-one and other independent variables to bring the analysis of variance and covariance under the general regression framework. The problem of multiple confidence intervals is treated in great detail. The section closes with discussion of various complications such as heteroscedasticity, nonlinearity, omission of variables, and the problem of infinite residual variance. In connection with the latter, it is perhaps worth noting that John Wise has shown that the consequences for the asymptotic properties of least-squares estimates are far from disastrous.

The third part of Malinvaud's book deals with problems of nonlinearity and with errors-in-variables models. It contains, as an appendix to the chapter on nonlinearity, a beautiful discussion of convergence in probability and the properties of probability limits. This is a subject of great importance in econometrics, but not well treated in accessible form elsewhere. The chapter on nonlinear models discusses the minimal distance estimators of Wolfowitz extensively; these are in turn related to some of the simultaneous equations methods in Part V. The method is applied to the problem of estimating a system of demand functions (which, due to their derivation from a common utility maximization process, embody numerous internal constraints). The next chapter is entirely devoted to errors-in-the-variables models, the importance of which had been suggested earlier in Malinvaud's discussion of the consumption function and Friedman's permanent income hypothesis. Weighted regression, the work of Geary, Reiersøl and Sargan on instrumental variables, and Liviatan's work on the consumption function are all treated at some length.

Part IV is entirely given over to time-series analysis and associated problems including distributed lags. This section perhaps contains more original material than the others. The first of the five chapters in this section deals with stochastic processes generally. The autocovariance function and the spectral distribution function of a stationary process are introduced. The Wold decomposition of any stationary process into a purely deterministic and a linear, and purely nondeterministic, process is given, and the alternative representation of the latter by equivalent moving average or autoregressive schemes is discussed. There is a brief but suggestive discussion of the prediction problem along Wiener-Hopf lines. Some readers may at first be confused by Malinvaud's short discussion of *ergodicity*, which appears to equate that property with *stationarity* generally, until they notice that the discussion is restricted to *linear* processes. The following chapter deals with various inference problems, including nonparametric tests for trend, tests for independence (a particularly fine treatment of the Durbin-Watson statistic is reserved for the following chapter), and the decomposition of time series and search for hidden periodicities. There is even a brief—but very brief—treatment of the recent literature on spectral estimation, although regretfully little indication of the uses to which such techniques may be put in economics. Serial correlation in

regression residuals is the subject of the third of the five chapters. Here we find discussion of the Durbin-Watson statistic, of the asymptotic properties of ordinary least-squares estimates (including Wold's 1952 results on correction of error variances), and of various methods for handling residual serial correlation. Aitken's generalized least squares is, however, omitted and the treatment of prediction with autocorrelated residuals leaves much to be desired. The reader may wish to supplement Malinvaud in this area with material from Goldberger.<sup>1</sup> The final two chapters of the section contain extensive and partly original discussions of the estimation of autoregressive models, and the closely related topic of distributed lag models. The biases of least-squares methods are fully treated, and the discussion of lag models is exceptionally complete, covering the work of Fisher, Koyck, Nerlove, Solow, and Theil and Stern, with many little added touches. A minor objection to Malinvaud's treatment in this chapter is that he views the major differences between various types of models generating distributed lags as largely statistical in the sense that differences are reflected in the residuals of the estimating equations. This point of view leads to erroneous conclusions in models containing more than one equation; distributed lags due to expectational causes, for example, are then the same for the same variable in different equations, whereas different variables in the same equation may have other distributions of lag.

The final section of Malinvaud's volume deals with simultaneous equations models and methods. An introductory chapter discusses identification problems, reduced forms, and recursive models. A particularly good discussion of production-function estimation problems is given. Next, various estimation procedures are introduced in terms of a variety of simple models. Two-stage least squares are shown to be simply related to instrumental variables in the overidentified case. An especially notable contribution is the clear statement of the requirements for recursivity and a convincing demonstration of the biases resulting from ordinary least squares when the diagonality of the contemporaneous residual variance-covariance matrix is not realized. There follows a rather technical but quite complete discussion of identification, mainly along the lines of Koopmans and Hood, but also including some of F. Fisher's recent generalizations. Minimal distance methods are effectively applied to the discussion of estimation in the next chapter, and T. M. Brown's recent "simultaneous least squares" is shown to be a special case. The closing chapter deals with the usual range of techniques, albeit with omission of three-stage least squares. Minimal distance methods are again used to illuminate the treatment, and the chapter contains a clear treatment of limited information along the lines it was first presented by Anderson and Rubin.

It is perhaps clear from the foregoing that Malinvaud's book is at once more advanced and of broader coverage than most existing books in the field. It assumes a knowledge of matrix algebra and basic statistical theory, and it develops a number of important special topics rather fully. It might therefore be used following a text such as Johnston<sup>2</sup> or to supplement Goldberger.<sup>3</sup>

<sup>1</sup> A. Goldberger, *Econometric Theory* (New York 1964), especially pp. 231-46; and *idem*, "Best Linear Unbiased Prediction in the Generalized Linear Regression Model," *Jour. Amer. Stat. Assoc.*, June 1962, 57, 369-75.

<sup>2</sup> J. Johnston, *Econometric Methods* (New York 1963).

<sup>3</sup> *Ob. cit.*

*Méthodes statistiques de l'économétrie* is a book with few defects; its chief one at the moment is that it has not yet been translated into English. Let us hope remedial action is soon taken.

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*Measurement in Economics: Studies in Mathematical Economics and Econometrics in Memory of Yehuda Grunfeld.* By CARL F. CHRIST, MILTON FRIEDMAN, LEO A. GOODMAN, ZVI GRILICHES, ARNOLD C. HARBERGER, NISSAN LIVIATAN, JACOB MINCER, YAIR MUNDLAK, MARC NERLOVE, DON PATINKIN, LESTER G. TELSER, AND HENRI THEIL. Stanford: Stanford University Press, 1963. Pp. xiv, 319. \$10.00.

This is a reasonably good book. Some of the papers contributed deserve close and repeated attention. The reviewer is of course expected to comment on the uneven quality of the essays. However, the quality variance is submerged under a potential reader's uneven interest in the material presented.

The book has been well organized into four parts. Part I contains the contributions to the "Theory and Measurement of Consumption" made by Milton Friedman, Nissan Liviatan, Jacob Mincer, and Don Patinkin. Zvi Griliches, Yair Mundlak, and Marc Nerlove wrote the papers in Part II under the heading "Theory and Measurement of Production." Part III covers monetary phenomena and contains papers by Carl Christ and Arnold Harberger. The last part is devoted to statistical theory, and presents the essays contributed by Leo A. Goodman, Lester G. Telser and H. Theil.

The reviewer's attention is certainly more unevenly distributed over the book than the quality of the papers. A passing reference to Part IV should not be interpreted as a negative reflection. The papers are competent, but of somewhat special interest. Still, Leo Goodman's analysis of movements and comovements of time series and Lester Telser's work on transition probabilities might be usefully consulted in particular contexts.

The papers by Friedman and Liviatan deal with the permanent income hypothesis of the consumption function. Friedman's essay emanates from the somewhat obscure methodological situation found in the *Theory of the Consumption Function*. A higher level hypothesis of the consumption function in terms of our inherited choice theory was tentatively sketched in Friedman's book. Furthermore, the book developed two distinct empirical hypotheses. One hypothesis was cast into the form of an error-in-variables model. The other supplemented the specification of a linear homogeneous relation between real consumption and expected income with an expectational hypothesis. Following the basic ideas of the higher level sketch, neither one of the empirical hypotheses related consumption with income measured in the standard manner. In either case the difference between the theoretical and observed income requires some conceptual clarification and involves some subtle interpretations. These issues were not adequately worked out in the book, and Friedman attempts to supply the necessary analysis. In particular, his paper deals with the role of unanticipated changes in measured income in the context of a "permanent income hypothesis." His analysis also attempts to establish a connection between the two subhypotheses developed in the book. The paper is stimulat-

ing, but does not resolve the analytical issues completely.

Liviatan's paper develops a careful appraisal of the subhypothesis cast in the mold of an error-in-variables model. It is an excellent paper and highly recommended. The author exhibits an articulate awareness concerning the logical structure of his evaluations. The role of auxiliary empirical assumptions is carefully noted and their place relative to the hypothesis under consideration explicitly assessed. His procedure is imaginative and resourceful. However, the appended comments by Friedman clearly reveal some of the unsettled issues emanating from the inadequate (and unavoidably indirect) semantic characterization of "theoretical income" in the context of an error-in-variables model.

Part I contained two more papers. Patinkin shows his usual analytic craftsmanship combined with a pervasive misconception of "positive economics" in a discussion of consumer surplus. Jacob Mincer's paper, however, contributes substantively to the volume. His material contains some imaginative applications of economic analysis to the demand for transportation services, the supply of labor, demand for domestic servants and for children, and the effect of information costs on price and income elasticities.

The part on production theory opens with a paper by Griliches on the role of the capital stock in the investment function. The author discusses carefully several concepts of the capital stock and appraises their comparative performance in the explanation of investment behavior. His major conclusion, though, appears somewhat obscure, viz., "that most of the argument favors two different measures of capital simultaneously, one approximating . . . capital as a quantity . . . of machines and the other measure approximating . . . capital as the current value of the stock of machines." Griliches may be correct, but I fail to recognize the economic rationale for this conclusion. Moreover, some of the statements in the initial sections bearing on the nature of the investment function in the absence of an explicit incorporation of the capital stock appear to disregard that the class of stock-adjustment hypotheses is not the only possible class of investment hypotheses. Still, I found the paper useful and very informative.

Marc Nerlove's essay appraises the returns to scale in the electricity industry. A cost function is derived from the specification of a Cobb-Douglas type production function and the stipulation of cost minimization relative to given outputs. The resulting cost function exhibits a parameter which may be interpreted as a measure of the "degree of returns." The empirical estimation confirms the prevalence of increasing returns. A finer analysis indicates an inverse relation between firm size and "degrees of return." Some of the results, however, are difficult to believe for an economist. Several elasticities of output with respect to capital would suggest that capital is almost irrelevant or that capital accumulation should even be avoided. The author emphasized, however, the inadequacy of the capital cost data. This of course renders an interpretation of the results quite difficult. Furthermore, the phenomenon observed may actually be interpreted in the context of an alternative hypothesis which simultaneously incorporates a rate and a stock of output (i.e., the expected program or similar concepts) and actually denies the occurrence of increasing returns with respect to the rate of output (in the sense of a partial derivative). Under this alternative hypothesis the phenomena observed by Nerlove would

yield no evidence in favor of the existence of "increasing returns." Nerlove worked his case carefully and explicitly, but the case of "increasing returns" still remains very open.

The last group of papers, dealing with substantive economic issues, were contributed by Carl Christ and Arnold Harberger. Both papers are informative and useful. Of particular interest in Christ's study of demand for liquid assets is the ascending order of income elasticity associated with more inclusive concepts of liquid assets. The evidence also supports the predicted operation of interest rates in demand behavior, but some puzzling problems arise whenever interest rates are separately incorporated into the regression. Harberger's paper on the inflation mechanism in Chile evaluates the comparative significance of "money push" and "wage push." I find his analysis thoughtful and interesting. However, he will invite the wrath of all those who believe in the superiority of large equation systems. The evidence presented strongly confirms the relevant occurrence of a money push. The existence of a lag in the money push is also revealed. The wage push appears to occur, on the other hand, only with a marginal significance. The author accompanies the regressions with a detailed discussion which clarifies and interprets the results.

In summary, the book should be welcomed. Several papers may be usefully assigned to graduate students and even be usefully absorbed by their teachers (before writing that textbook).

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*Natsional'ny dokhod Ukrayins'koyi RSR v period rozhornutoho budivnytstva komunizmu.* (National Income of the Ukrainian SSR in the Period of Development of the Construction of Communism.) Edited by O. O. NESTERENKO *et al.* on behalf of the Institute of Economics of the Academy of Sciences of the Ukrainian SSR. Kiev: Academy Press, 1963. Pp. 335. Rbl. 1.60.

Unlike the states in our union or in other federations, the national republics of the USSR have both enough statistical data and practical need to compute their own national incomes. Since 1957 their economies have been planned and administered as separate units, and national income accounts are used for such purposes. Signed by 25 authors and six editors, the volume under review is the first comprehensive work since 1925-30<sup>1</sup> on methodology and the problems of actual national income of one of the Soviet republics, the Ukraine.

The concept of national income used in this book is the traditional Marxian "net material product." It continues to exclude most of the services as "unproductive," in spite of the fact that the proportion of the so-called "productive" labor in the Soviet economy decreases and that of the services increases, as it does everywhere else in the industrialized world. Net material product is calculated for each industry and sector by the value-added method. To this they then add the volume of the turnover tax prorated for each industry in

<sup>1</sup> Estimation of the Ukraine's national income was quite advanced before 1930. Since then there was only one admittedly crude attempt. Cf. V. Holubnychy, "Das Volkseinkommen der Ukraine in den Jahren 1940 und 1954," *Sowjetstudien* (Munich), no. 2, March 1957.

which it originated. They consider this tax to be the same thing as profits, i.e., part of the Marxian "surplus value." The book also proposes an innovation: to impute rent to the land, but this is not done in practice yet.

According to the above method, the Ukrainian national income in 1960 was 27,046 million rubles, or \$30 billion at the official rate of exchange. It was growing by 7 per cent per year, but that growth has been slowing down; 58 per cent of it originated in industry, 26 per cent in agriculture, and the rest in the transportation of goods and in commerce (the balance of foreign trade was not counted, however). The Ukrainian share in the total national income of the USSR amounted to 19 per cent. The per capita income was 7 per cent below the USSR average. (If services were included, the per capita national income of the Ukraine would be about \$835 today.)

Calculations of the primary and final distribution of the national income in the book are not quite complete and involve many problems, including some political ones. For example, the authors conclude that 78 per cent of taxes collected in the Ukraine by the federal government (four billion rubles a year!) are not returned to the Ukraine in federal expenditures and investments. The final use of the Ukraine's national income according to the book consists therefore of 71 per cent going for consumption, 23 per cent for investment, and 6 per cent for deficit appropriated by Moscow and transferred "to construct Communism" in some other parts of the USSR. The book also discusses the problem of price differentiation inside the USSR and points out that some additional portion of the Ukraine's national income is transferred to the rest of the USSR via prices. It does not arrive, however, at any estimate of this portion. Another problem which remains unsolved is that of the correct allocation of the turnover tax volume between the Ukraine and the rest of the USSR.

The book contains a lot of valuable statistics which are not available elsewhere. In fact, its data on the components of the Ukraine's national income are more comprehensive than those published so far for the USSR as a whole. There are also some revealing explanations that may be new to the students of the Soviet economy. It is stated, e.g., that the surpluses in the USSR federal budgets are used to expand the credits of the State Bank. Many students suspected, on the contrary, that they were additional hidden expenditures for defense. The book ends with a summary in the Russian language. It is a great pity that the summary is not in English or in some other Western language.

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### **Economic Systems; Planning and Reform; Cooperation**

*Cuba: The Economic and Social Revolution.* Edited by DUDLEY SEERS.  
Chapel Hill: University of North Carolina Press, 1964. Pp. xx, 432. \$7.50.

This is the first substantial treatment of the economic aspects of the Cuban Revolution to appear in English. It is indispensable for students of Latin American economic development, and particularly for students of problems of economic planning in primary export economies. The book is made up of four sections: (1) a long essay by Professor Seers on the economic and social background of the Revolution and a tentative assessment of the over-all perform-

ance of the island's economy from 1959 to 1962; (2) a detailed analysis of the changes in the structure and operation of agriculture placed in the context of a survey of prerevolutionary agricultural problems, written by a Chilean economist, Andrés Bianchi; (3) an exhaustive account by Richard Jolly, an English economist, of the pre-1959 educational system and progress in that field since the downfall of Batista; (4) and Max Nolf's (another Chilean economist) study of Cuban industrial stagnation before the Revolution, together with a description of industrial organizational changes and planning in 1961-62.

Inevitably the essays are uneven, overlap, and differ somewhat in tone and treatment; the volume shows signs of being put together in a hurry. As Seers explains, "what really decided the issue [whether or not to do the book, given the lack of data and rapidity of change] was that information is so badly needed on what is one of the most important political developments in this century." Important areas of the "economic and social" revolution are slighted, thus the volume's title conveys more than is intended; labor developments are given cursory treatment; progress in the field of health receives little mention; there is practically nothing on social services, culture, and recreation. With limited time and resources, the editor chose to focus on what he considers to be the key areas; this is not intended to be a comprehensive, much less definitive, study of the Revolution, as Seers himself points out.

The authors' major findings challenge the official U.S. view that Cuban socialism rests on dangerously shaky economic foundations. A careful appraisal of the available data shows that from 1959 to 1962 "total real disposable income rose considerably in the country districts but perhaps hardly at all in Havana" (p. 37). A shortage of statistics prevented a sector-by-sector analysis, but the chief sources of the expansion are enumerated; the nearly total elimination of rural seasonal unemployment; increases in land under cultivation; the dramatic expansion of social investments in housing, education, and health. On top of this, the abolition of rents and the relative improvement of rural wage rates redistributed income from city to countryside. Food shortages in late 1961 and 1962 (the food situation has improved significantly since then) are correctly attributed to a rise in autoconsumption. This rise was generated partly by a breakdown in the distribution system when the trade sector was nationalized and partly by deterioration in the terms of trade between country and city; organizational problems associated with inexperienced state management of the People's Farms in the sugar, cattle, and rice sectors; and drought. Thus there is no real support for the thesis that Cuban agrarian reform was and is a failure. Quite the contrary, Bianchi carefully shows that organizational changes in key sectors of agriculture were the precondition for crop diversification and expansion, and full employment of the rural labor force. Implicit in the analysis (the authors rarely give political questions their due, or make them explicit, thus viewing economic and social changes divorced from the popular mood, counterrevolutionary forces, U.S. foreign policy, and so on) is the fact that there was mass support for these structural changes; Seers is correct when he writes (p. 60) that the Revolution is "to a great degree irreversible."

Turning to industry, Nolf shows that prerevolutionary industrial develop-

ment lagged well behind objective possibilities (the growth of the market, raw materials, and so on). Although he slights Batista's industrialization program of 1953-57 (an analysis of its limited success would reveal much about the problems of the old economy) and fails to explore fully the unfavorable implications of market organization for industrial development, his conclusions are incontestable. Thus the emphasis that the 26th of July Movement placed on industrialization in its initial programs in 1957-58 was carried over into the revolutionary period, where it underwent considerable alteration. Nolf concentrates on investment planning and industrial policy in 1961-62 and reproduces the first "plans" which were later either modified or discarded as containing too much "subjectivism." These plans projected very optimistic trends and finally awarded high priority to heavy industry, specifically chemicals, steel, and automotive plants. The projections were lowered and investments in heavy industry (excepting chemicals) were postponed as balance of payments problems, shortages of raw materials and technical and administrative personnel, and other difficulties multiplied. In mid-1963 Cuba adopted a "new course" (which Nolf failed to foresee through no fault of his own—Cuba is an island of quicksand for those who attempt short-run predictions) which shifted emphasis to sugar and cattle and the industries complementary to these sectors. Only with respect to electrical power and maritime investments have Cuban planners remained on a straight course; outlays in these areas have risen steadily since 1959.

The problem of sources is the central problem for the authors; only Jolly had full access to needed sources, and he receives more space in the volume than the others; thus the data problem shapes the study itself. It is not likely that Cuban education from 1959-62 will ever again receive the comprehensive descriptive treatment which Jolly gives it. He explores quantitatively and qualitatively Cuba's many new educational programs, describes the rapid expansion of primary education and the all-out war against illiteracy in 1961, and probes the new technical education programs for working adults. Weighing the costs and benefits of the massive outlays on education, Jolly hedges on a final judgment, but his materials support the view that the inevitable waste involved in such a program is more than offset by the cultural and potential economic gains for the Cuban people.

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*Economic Planning in France.* By JOHN HACKETT AND ANNE-MARIE HACKETT, with a foreword by Pierre Massé, Commissaire Général du Plan. Cambridge: Harvard University Press, 1963. Pp. 418. \$7.00.

This study is devoted almost entirely to the procedural aspects of French planning. The Planning Commissariat, the ministries, and the other bodies involved in creating the Plan, are described in detail, including changes and developments in their nature and functions since 1946. The Plan is then followed through the various steps of the drafting process. Finally, the mechanics of implementing the Plan are surveyed. The authors bring to bear an extensive knowledge of the French planning machinery and provide a vast amount of information on this complex and changing process.

The matters of principal interest to economists, such as tests of the soundness of the Plans, how closely they are followed, and what the effects of planning are, for good or ill, on the French economy, lie outside the scope of the book. The authors' reasons for limiting the scope are suggested by these lines in the chapter, "General Conclusions": "If there was no Plan, what difference would it make? We have gradually come round to the view that it is not, on the whole, a very helpful means of approach. It depends too much upon an impossible reconstruction of 'things as they might have been' as France's whole post-war development has been coloured to a, varying, extent by the Plan. Moreover, it is usually put by the observer with his own preoccupations in mind" (p. 357). That is to say, attempting to assess the effects of planning is both empirically impossible and subject to a possibly biased choice of criteria for evaluating the performance.

The empirical impossibility of evaluating the effects of planning does not prevent the authors from repeating, in the next paragraph, the popular and unsupported conclusion: "It is very largely because of the achievements of the planning experiment since 1946 that securing a rapid rate of growth is not the arduous problem for France today that it is, for very different reasons, for the United States or the United Kingdom."

The impression one receives from this book, as well as from most other works on the subject, is that French planning is primarily a ritual. For those interested in the intricacies of the rites, this book will be valuable. Those who want to know whether the ritual really contributes anything to the French economy, and, if so, what there is about France that demands the planning ritual when other economies (Germany, Japan) get along as well or better without it, will have to search elsewhere for the answers.

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*Gospodarka Europejskich Krajów Socjalistycznych (zarys rozwoju w latach 1950-1975).* (Outline of the Economic Development of the European Socialist States from 1950-1975.) By ARTUR BODNAR. Warsaw: Książka i Wiedza, 1963. Pp. 391. Zł. 35.

Artur Bodnar's book merits the attention of students of the communist (or, as the author prefers to call them, socialist) economies in spite of the fact that the main body of his historical statistics ends, for the most part, with data for 1958-59. Also the study covers only those socialist countries of Eastern Europe, plus the USSR, which belonged to the (Communist) Council for Mutual Economic Cooperation (COMECON) around 1960.

In the author's words, the purpose of inquiry is to trace the economic developments in the respective socialist states, analyze their regional interdependence, and compare their economic achievements with those of Western Europe and the United States. To this end, Bodnar has painstakingly collected statistical material directly from the original sources of the socialist countries and has presented it in more than 200 tables. Interestingly enough, he has been less inclined to use similar official Western statistics. When it comes to an intersystem comparison, his data on the capitalist economies are taken, with only a few exceptions, from the studies published in Moscow or Warsaw.

The book is divided into seven parts, but the inquiry is focused on industrial activity, which is presented in Part IV and comprises about one-third of the entire analytical study. Another third is taken up by agriculture and trade, presented in Parts V and VI, respectively. Economic aggregates—services excluded—are scrutinized in the opening Part I. Part II offers a rather superficial and deficient summary of the natural-resource situation, while Part III discusses the COMECON countries' human resources and manpower requirements in the future. In the concluding Part VIII, the author speculates about the consequences of rapid economic progress in the Communist orbit on the economic configuration of the world.

Discussion in each subdivision opens with a review of the COMECON economy in its totality and in the contest of the intercountry relations. It continues with an examination of the particular subject matter within the framework of each individual socialist state.

Among the most significant aspects of Bodnar's study is an indication that, as the Soviet bloc countries approach the stage of high industrialization, structural changes in their economies create problems not unlike those facing the capitalist countries. One is the growing dependence on imported raw materials. Attempts to ensure the future supplies by bilateral resource-development agreements have not proved very successful. There is a marked hesitance, as Bodnar remarks, in the less-developed states to commit large parts of their resources to export industries. Moreover, a kind of anti-import phobia, obviously connected with their recent stage of predominantly agricultural, closed economies, prevents systematic application of the principle of low-cost production throughout the bloc.

Some investment decisions and efficiency of execution may also be questioned, hints Bodnar. Thus, during 1951-57, Rumania spent about 35 per cent of all industrial investment in an attempt to augment output of its indigenous fuels, i.e., chiefly petroleum, with disappointing results. Relatively small expenditures on research and development as well as low labor productivity throughout the bloc are other sore points mentioned by Bodnar.

All this notwithstanding, the author calculates that the COMECON states, and especially the USSR, will become the dominant factors on the world market during the 'seventies. Bodnar sees the U.S. industrial production surpassed by the USSR by 1972, at the latest.

Evidently in the past the COMECON area recorded a rapid growth in industrial output, although not in the living standards that Bodnar accepts as the ultimate criterion of economic progress and intersystem competition. Moreover, his statistical and analytical confrontation of both systems can be questioned on several grounds. For example, it is far from clear how Bodnar has derived his index of total economic growth and industrial production of all socialist countries. Further, his intersystem statistical comparisons suffer from a timelag, about 10 to 15 years, as Bodnar's conclusions usually rest on the socialist economies of 1970-75 and the capitalist world of 1958-1960. Besides that, he takes for granted that the planned high rate of growth of communist economies in the next 10 to 20 years will be attained.

Within five months after the book was published, the results of the first four years of the seven-year plans were proving him wrong.<sup>1</sup> Thus, between

<sup>1</sup> *Zycie Gospodarcze*, March 17 and 24, 1963. *Vestniki Statistiki* (Moscow), March 1963, pp. 39-48.

1958 and 1962, the annual rates of growth of national income declined in the USSR from 12 per cent to less than 6 per cent; in Poland, from 5.6 per cent to 2.5 per cent; in Czechoslovakia, from 8 to 0.5 per cent, etc.

How far all this changed the outlook for the future, as envisioned by Bodnar, we shall have to wait to see. But some comfort may be drawn by Western economists from the fact that the vicissitudes of reality play the same havoc with forecasts no matter whether they are based on estimates of market forces or on the flat decrees of an economic plan.

JAY G. POLACH

*Resources for the Future*  
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### **Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit**

*Money.* By M. L. BURSTEIN. Cambridge: Schenkman Publishing Co., 1963. Pp. xvi, 908. \$13.25.

This is a massive book, over 900 pages long, and replete with explanatory notes at the end of each chapter as well as appendices attached to some of the latter. Broadly, it follows in the footsteps of such contemporary works on money as those of Shaw, Hart and Kenen, and Trescott with their emphasis on the application of economic theory to monetary problems and the relationship of financial to real variables. It goes beyond them, however, in its attention to and coverage of advanced macroeconomic theory in which are included such topics, among others, as Leontief's synthesis of Keynes and Fisher, the Chicago School's modification of the Leontief model, the Duesenberry system, Rose's formulation of the Harrodian problem as well as the Robinson and Kaldor growth models, Hicks's "real" trade-cycle theory, the Friedmanian Quantity Theory, the Gurley-Shaw hypothesis, and the rudimentary dynamics to be found in Keynes's *Treatise on Money*. Also, the book goes well beyond these texts in its explicit formulation of general equilibrium and process analysis and the use of mathematical models. The calculus and matrix algebra are employed extensively, not only in the macrotheory section of the work, but also in the chapters and notes on commercial and central banking theory and policy as well as balance-of-payments theory.

In spite of the considerable richness and diversity of the material and other positive aspects of the work mentioned at the conclusion of this review, the book is exasperating on several counts. For one thing, it abounds in poor pedagogical techniques. Thus, it is badly organized with theory and policy somewhat indiscriminately interspersed throughout, the traditional velocity-of-money chapter followed only some 300 pages later by a chapter on the neo-quantity theory of money, and the latter suspended in limbo and separated from the income-expenditure chapters by one on central banking policy. Along similar lines—and such illustrations could be multiplied—in undertaking an introductory discussion of money's role as a liquid store of value (pp. 4-5), Keynesian liquidity preference is mentioned without ever being defined and assumptions made about the reader's ability to grasp the functional relationship of liquidity preference to different types of monetary standards and in-

voluntary unemployment, assumptions and relations which are not systematically explored until some 500 pages later.

More important, while the author is in good company in prefacing his work with the hope that "many undergraduates will find these materials manageable," even a cursory reading suggests that the material contained therein is much too difficult to be taken seriously by him or by others as a potentially successful undergraduate text either in a course in "Money and Banking" or one in "Macroeconomics." While such exaggerated claims may delight book publishers, they do not lend luster to the economics profession. Indeed, such claims tend to create a carnival atmosphere, nine-tenths of which are compounded of patent nonsense and an eye to the till, while the remaining tenth represents an ingenuous hope that the elixir being offered miraculously may prove serviceable. This book goes even further than most by including, among its potential customers, not only undergraduates and, quite legitimately, graduate students and professional readers, but also bankers and financial analysts. Perhaps commercial bankers could "find many sections of the book pertinent to their everyday business" could they but grasp the very high order of abstraction to be found throughout and the ability to translate such abstractions into meaningful propositions. A good case in point is where the author deals with the theory of the bank as a firm (pp. 190-93). A purely formal statement mathematically couched in terms of maximization subject to side conditions is offered in these pages with the solution technique, nonlinear programming, not presented. As the author himself says, "our model can, by virtue of its *emptiness*, accommodate a large number of bank portfolio problems" (*italics mine*). I am afraid that this is only too true so far as the commercial banker is concerned.

Nor is the going much better for central bankers. While the author does delve into such contemporary monetary issues as the role of nonbank financial intermediaries, the bills-only doctrine, the lock-in effect and the term structure of interest rates, these are analyzed in almost debonair fashion and certainly are not germane to the main body of the text. In fact, from the many pages devoted to the subject, it is apparent that the author's real love—and strength—lies in the exploration and analysis of the innumerable variants of income theory rather than in problems of policy. The trouble lies not in such a bona fide division of intellectual labor as in the fact that so much of the former discussion is devoted to abstruse issues of doctrine, issues which can only result in policies of little practical import for central bankers. Thus, after some 250 difficult pages, which include four approximations to income theory and several appendices, the author concludes this section of his book with a chapter entitled, "The *Pure* Theory of Central Bank Policy" (*italics mine*). This consists primarily of spelling out the policy implications of the many income variants within a general equilibrium framework. One can do no better in criticizing this type of endeavor than to quote from a recent article by Brunner and Meltzer on this matter ["Some Further Investigations of Demand and Supply Functions for Money," *The Journal of Finance*, Vol. XIX, No. 2, Part 1 (May 1964), p. 240]:

For almost two decades a virtually impenetrable curtain has separated two groups of monetary economists. One group has engaged in a learned

and apparently interminable discussion concerned with the subtleties of Pigou effects, Lerner effects, and Keynesian effects, the existence or absence of money illusions dichotomization of real and money prices, Hicksian weeks, and related esoterica. . . .

It does not require an astute observer to notice that many of the issues that agitate monetary theorists have little or no bearing on the policy issues discussed. The analytical frames constructed and vigorously disputed with plausible and counterplausible arguments yield neither propositions clarifying the policy issues nor verified propositions directly applicable to policy decisions.

In spite of the criticisms leveled above, the book merits scrutiny, if not as an undergraduate or trade text, then as a reference work for advanced graduate students. Burstein exhibits not only a high degree of knowledge and competence of advanced monetary theory, but his undoubted, if at times misplaced, flair for the English language and striking the happy phrase stand him in good stead when discussing such traditionally dry subjects as monetary and banking history. These do tend to come to life in his hands. Further, he does not hesitate to tilt lances in engaging fashion with theories he considers unsound or misleading, as witness his critique of the Friedmanian quantity theory of money. While he tends to exaggerate Friedman's much-quoted and misquoted statement on income velocity as an empirical constant (pp. 729-34), he does not hesitate to take on at the same time the real issue of the so-called stability of the demand function for money. His critique of such single-equation models in terms of the now familiar identification problem (pp. 772-75), while not new, is well worth reading, along with many other little gems to be found scattered throughout this very uneven and peculiar book.

MURRAY E. POLAKOFF

*New York University*

*The Economics of Banking Operations—A Canadian Study.* By JOHN ALEXANDER GALBRAITH. Montreal: McGill University Press, 1963. Pp. x, 510. \$7.75.

This is a book for serious students of the economics of banking for its fundamentals are treated at an advanced level. There are seven chapters, each of which is a study in itself. The first chapter, "The Nature and Consequences of Domestic Banking Operations," considers the activities of commercial banks as borrowers, lenders, and investors. The sense in which commercial banks are similar to and differ from nonbank financial institutions is fully explored. Those interested in the issues raised by Professors Gurley and Shaw should find much to interest them here, although on the crucial question of whether new controls are necessary or desirable little is said.

Chapter 2 "The Economics of Banking 'Output,'" begins with a useful summary of the literature on the availability doctrine and then develops a theory of lending behavior, assuming that banks either maximize profits or maximize profits from loans. Credit rationing in modern banking systems is found to be rare, and variations in excess cash are held to alter security holdings rather than loans.

"Competition, Profits, and the Capital Accounts" is the title of the third

chapter. Here the author argues that even though competition between commercial banks will lower their profits in the short run, the results of this competition may restrict the growth of nonbank financial institutions, and bank profits in the long run may be greater.

The implications of monetary policy for bank profits are also considered, and it is held that bank profits are probably increased by a restrictive monetary policy and reduced by a policy of ease, although the converse cannot be ruled out. The analysis is questionable, however. Restrictive monetary policy is said to raise interest rates. But if an economy is operating at full employment with a constant money supply, an increase in investment demand would raise the equilibrium rate of interest and generate inflation. If the inflation is prevented by a contraction in the stock of money, in the new situation the rate of interest would be higher than it was originally, but not necessarily higher than it would have been if the stock of money had not been reduced; tight money need not raise the equilibrium rate of interest.

Chapter 4, "Interregional Banking Transactions and Operations," considers the pros and cons of branch versus unit banking. Most of the arguments are shown to be fallacious. The author concludes, however, that branch banking probably makes a greater contribution to the stability of the economy, since under this system regional shifts of deposits need not cause secondary repercussions. But since he also argues that, under contemporary conditions, banks that gain reserves buy government securities, while banks that lose reserves sell them, the secondary repercussions under a system of unit banking are trivial.

The fifth chapter is concerned with international banking transactions and operations. It considers mainly banking arrangements under flexible exchange rates. The sixth chapter is about government financial operations. The impact of government deficits and surpluses on economic activity is assessed, and the possibilities of shifting government deposits between the central bank and the member banks in order to alter reserves is extensively discussed.

In the final chapter, "Central Banking," the author is first concerned with the definition of a central bank. A bank becomes a central bank if it operates without consideration for its profit position; it is a question of motive. Both open-market operations and the discount rate are considered at length. On whether the discount rate should be changed by formula or by discretion, i.e., whether changes in the discount rate should serve as a signal, Dr. Galbraith suggests a compromise. Ordinarily the rate should be altered by formula, but when a signal appears desirable, the formula should be abandoned for discretionary changes.

One of the important conclusions of this chapter is that the basic principle of central-bank control is the manipulation of excess *cash*. In considering the English banking system, however, the author discovers that a modification or special interpretation of this generalization becomes necessary. The English banks are supposedly subject to a liquidity requirement; they must keep 8 per cent of their deposits in cash and at least 30 per cent in the combined form of cash, money-market loans, and bills. But it is maintained that bills are perfect substitutes for cash, since the Bank of England supports this market. A reduction in the cash requirement without a change in the so-called

liquidity requirement would temporarily create excess cash. But the only consequence of this would be an exchange of cash for other liquid assets.

The issue is clarified if it is said that the English banks have a 30 per cent reserve requirement and receive interest on 22/30 of their reserves rather than both a cash and liquidity requirement. The basic principle of central-bank control can then be said to be the manipulation of excess *reserves*.

There is also an appendix on the expansion of banking systems. The usual explanation of the way in which a commercial banking system expands when it has excess reserves assumes that excess reserves are not averaged. But if excess reserves can be averaged, so that banks tend to keep average excess reserves zero, Galbraith shows that the standard explanation of the expansion process is far from satisfactory.

This is a new book about old questions. It must be judged mainly by its treatment of the literature, its clarification of obscure points, and the freshness of its observations. By these standards it is excellent. So many controversial topics are discussed that everyone will not be able to agree with everything. But until some other able economist surveys this literature again and reflects upon it, the present work will be of great value to experts and will be required reading in graduate courses in banking.

ARNOLD COLLERY

*Amherst College*

*Banking and Monetary Studies*. Edited by DEANE CARSON. Homewood, Ill.: Richard D. Irwin, Inc., 1963. Pp. xiv, 441. \$6.00.

The Comptroller of the Currency has commemorated the Centennial of the National Bank Act of 1863 by sponsoring the publication of a book of 23 essays on banking and monetary subjects. These subjects include banking and monetary history, monetary policy, banking structure, competition and regulation. Many of the essays also contain a generous dose of monetary theory.

Space limitations prevent me from going into a detailed discussion of each contribution. It is sufficient at this point to note that the various contributions differ in quality. Most are very good indeed. Some could have been improved if additional references had been consulted. Others probably simply reflect the busy work schedules of their authors—all the more serious in view of the sponsor's claim that "all of the selections are original contributions written by outstanding economists exclusively for this book."

For purpose of setting the record straight regarding American banking conditions in the years before and after the Civil War, Bray Hammond and Philip Cagan present the reader with two very useful essays. According to Hammond, banking up to the Civil War was not as bad as some people would have it. And Cagan's appraisal of the pre-1914 National Banking System is a very different and far more interesting story than the caricature of that system in standard money and banking textbooks.

Hammond's emphasis on the failure to recharter the Second Bank as the reason for the U.S. monetary muddle in the 1830's is overdrawn. For the most part, U.S. troubles originated abroad. A world-wide rise in prices coupled with capital inflows into the United States enabled the country to expand its stock

of money and prices without incurring external difficulties. This expansion occurred primarily in the form of bank notes and deposits. When external prices declined and capital inflows ceased after 1839, an internal contraction was the only alternative if the United States was to continue on the specie standard and fixed exchange rates. The subsequent U.S. adjustment in the period 1839-43 took the form in part of a 25 per cent decline in the number of banks, including the demise of the Second Bank (under Pennsylvania charter since 1836) in 1841.

John Culbertson's argument that many errors in economic policy since World War II apparently can be traced to the fallacies of the "banking school" rather than economic theory will probably appeal to most economists. I would argue, however, that much of what is wrong with U.S. banking and monetary institutions can be traced back hundreds of years. The specie standard, fixed exchange rates, and "real bills" doctrine are but illustrations of defunct ideas that still sway a considerable portion of the banking fraternity. One effect has been to create an intellectual disaster area in U.S. banking and monetary institutions.

The implication of David Meiselman's novel argument that a rise in interest rates may in fact intensify the U.S. gold outflow should come as quite a shock to those in the grips of the "banking school" mentality described by Culbertson. Meiselman bases his argument in part on the close association between the quantity of money and economic activity in the United States. Canadian data would also tend to support his argument.

The interesting contribution by Deane Carson and Ira O. Scott, Jr. dealing with commercial bank aversion to risk should be useful to people concerned with economic development. The results are consistent with the important role that commercial banks play in development. An independent study employing member bank data for the period 1950-60 tends to support their argument.

GEORGE MACESICH

*Florida State University*

*Financing Economic Development.* By ANTONIN BASCH. New York: The Macmillan Company, 1964. Pp. xiv, 334. \$6.50.

Professor Basch's objective in this book is to analyze the role of financial institutions and financial markets in the saving-investment process in underdeveloped countries. The main problem is how underdeveloped countries can best use their financial structures to increase their saving rates and, hence, their output growth rates. Towards this end, the book is organized around government saving, private saving, and foreign saving; and the author discusses each topic by drawing on the recent experiences of many countries, principally those in Asia and Latin America. He has apparently left few stones unturned in digging for the facts, and he has dug deeply, but his analysis of these facts is, most of the time, rather shallow and, in a few instances, grossly defective.

In the first part of the book, the author takes up the problem of government saving. He surveys several Asian and Latin American countries and concludes

that there are so many pressures for increases in current expenditures by government, and so little income elasticity to the tax systems, that it is doubtful if many countries can generate sizable surpluses of tax receipts over current expenditures. Such surpluses are kept low in many countries, according to the author, partly because government funds are used to subsidize public enterprises which seem to have chronic deficits.

In the second part, the author feels that private saving should be increased and new savings habits acquired by the people. "The policy should be one to guide people increasingly to deposit savings at financial institutions or to invest them in financial assets. This object requires, of course, patient and continuous work and must be supported by appropriate financial institutions" (p. 106). It also requires, the author states, monetary and financial stability, and many countries in South America are censured by him for failing this test. Basch surveys financial institutions in quite a few underdeveloped countries (and Australia), and discusses capital markets in these countries and the possibilities of increasing direct finance. He attempts to link up the financial structure of each country with its saving-income ratio (e.g., an efficient financial structure should raise the saving-income ratio), but not very much emerges from these comparisons.

The role of foreign saving in economic development is discussed in the third part of the book, first in terms of foreign government saving and then in terms of foreign private saving. Here the author evaluates the recent activities of some international financial institutions; he is pessimistic about the prospects for private foreign investment; but he is hopeful about increasing governmental "soft" loans and grants to underdeveloped countries. A final part of the book briefly summarizes the progress achieved (quite a bit in places) and the task ahead (difficult and challenging).

The book contains a wealth of information on financial aspects of development, and it has useful references. It suffers, however, from the author's inability to make very much of the facts, facts, facts. The book is especially weak in its analysis of the financial role of government in economic development. Under what circumstances should a new governmental financial institution be established? What is the role of government in fostering capital markets? When should public enterprises run deficits? The author fails to answer these and similar questions satisfactorily because of the absence of any theory of efficient resource allocation, including a framework for analyzing social vs. private goods, and social benefits and costs vs. private benefits and costs.

Another serious weakness in the analysis is the author's neglect of the money supply as a means of saving. The money supply almost never appears in any of the analyses of saving, nor in the numerous tables that add up the savings of various countries. This omission greatly distorts a good bit of the discussion.

In sum, the author has gathered together a great deal of data in a very useful way, but he has failed to tell us what an optimum financial structure looks like.

JOHN G. GURLEY

*Stanford University*

*Política financiera.* By ANTONIO ORTIZ MENA. Mexico: Editorial PESA, 1962. Pp. xv, 112. 26 pesos.

*Visión crítica retrospectiva del crédito en México.* By HANÍBAL DE ITURBIDE. Mexico: Publicaciones Especializadas, S.A., 1963. Pp. 136. 26 pesos.

*Mercado de dinero y capitales.* By ALFREDO LAGUNILLA IÑARRITU. Mexico: Publicaciones Especializadas, S.A., 1963. Pp. 124. 26 pesos.

These three little books provide useful glimpses of Mexican finance and financial institutions. Antonio Ortiz Mena, Mexico's Secretary of the Treasury, and Haníbal de Iturbide, a prominent commercial banker, discuss the problems of credit and monetary policy from opposite sides of the fence. Alfredo Lagunilla Iñárritu, a professor of the National University, tries to sum up everything about money markets and capital markets, but succeeds chiefly in illuminating the present state of these concepts in Mexico. No great scholarly effort went into the writing of these volumes—the first two are collections of speeches, while the third is an expanded set of lecture notes. Their interest lies in their authorship and the points of view they exemplify.

The speeches of Ortiz Mena and Iturbide are worth reading together. They make excellent counterpoint. Indeed, bits of each can be arranged to form a dialogue that sums up the main issues in Mexican banking policy. In such a dialogue, Iturbide keeps pointing out that the policies of the government are self-defeating; that its encouragement of the development of a capital market is vitiated by the activities of the treasury and of the government banks. He pleads for a change in the holding-company role of the Nacional Financiera, for an easing of the extraordinarily high reserve requirements imposed on commercial banks (up to 100 per cent), for actual rather than verbal encouragement of a capital market, for more sensible banking laws, and for a better monetary policy. Ortiz Mena keeps answering that the government has the responsibility for managing the private sector as well as the public sector; that the monetary authorities are doing their best to halt inflation; that the private sector is not carrying its weight; that everything possible is being done to develop a capital market; and that the government banks are doing a creditable job. Students of Latin American financial institutions will find many colorful and apt quotations in these volumes.

This synthetic dialogue mirrors the continuing argument between the private bankers and the government bankers in Mexico. Opinions as to who is winning the verbal battle will vary, but there is no doubt as to who is winning the real one: each year the public banking sector grows relatively larger.

Lagunilla's book is quite different. It is an expansion of the notes for the course on money markets and capital markets which he gives in the National University of Mexico. Seven of the eight chapters are crammed with terminology and statements about these markets in the rest of the world (not always accurate); the eighth deals with Mexico. This last, "The Mexican Finance Market," will be of interest to readers looking for a bird's-eye view of Mexican financial institutions and their problems.

I. H. OTTO

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*Money and Capital Markets.* By ROLAND I. ROBINSON. New York: McGraw-Hill Book Co., 1964. Pp. ix, 371. \$8.50.

We have a considerable body of literature on the money and capital markets, and about the system of financial institutions related to these markets, but most of it deals with one or another segment or specialized function. Here, now, is a book that views the financial markets and related financial institutions as a unified system. The perspective in which Roland Robinson portrays the money and capital markets should not only aid the task of learning by students but even improve the perception of their teachers.

This is a textbook. The author set out to write an expository treatment of the financial markets; he has designed the contents and adapted the style to that end. He accepts the limitations of textbook writing. Although a leading researcher in the capital markets for many years, Robinson fights no battles over controversial issues and makes no pleas for particular points of view.

Organization of material is an especially difficult task in describing the financial markets. No matter where you begin, there is a great deal that needs to be said almost in the same breath for any of it to be accurate and adequate. Robinson solves the problem of organization about as well as it can be solved by beginning with the most general description of money and capital markets and moving consistently from the general to the particular.

The first group of chapters seeks to place the financial markets in their economic setting at the center of a system of saving and investment. Saving and investment are described first in economic terms and then in terms of the flow of funds through financial institutions. Then the financial markets emerge clearly as the mechanism whereby both institutions and individuals convert funds flows into investment in financial instruments.

Money markets are distinguished from capital markets mainly by the length of maturity of the instruments in which they deal. Robinson conceives the money markets as markets for short-term instruments, ranging in maturity from less than one day to not more than one year. Capital markets deal in equity securities and in obligations with maturities of more than one year. Intermediate maturities are sometimes regarded as money-market instruments, but Robinson thinks they belong to the capital markets because their yields are more closely allied to long-term than to short-term rates.

The section on money markets is a masterpiece both of exposition and of self-restraint on the part of the author. A great deal has been written about the operations of the Federal Reserve System in the money market and about the market for Treasury securities. It would have been very easy to fatten up this book by writing at length on credit policy, fiscal policy, and debt management. Instead Robinson points to the abundance of supplementary materials that are available without cost, or at low cost, and limits himself to three chapters. This brief section establishes the main outlines and leaves to students and teachers the use of supplementary materials to fill in.

The discussion of capital markets is longer, partly because the author adopts a rather broad definition of capital markets. The stock and bond markets are described in three chapters, with separate chapters on the corporate

and municipal bond markets, and there are two chapters on less well-known segments of the market. The mortgage market, of course, has grown greatly in importance in recent years and clearly deserves inclusion in a study of capital markets. Justification for a chapter on consumer credit is not so clear because there is no open market for consumer-credit instruments, but there is a market for instruments issued by institutions that supply consumer credit, and it is to the demands on the money and capital markets by these institutions that this chapter is addressed.

The purpose of describing the financial markets is to provide a basis for analysis. In the fourth part of the book the author turns directly to analysis and offers five chapters on the dynamics of money and capital markets. There is a discussion of cyclical instability in the money and capital markets, followed by one on the problem of forecasting changes in rates and prices. These are conventional topics, but the next two chapters deal with topics seldom mentioned and rarely considered in any depth in the literature of the financial markets. One is on competition in the markets and the other describes evolutionary changes in the markets. These chapters are well worth reading even by people who consider themselves knowledgeable about financial markets.

The appraisal of Robinson's book by this reviewer is so favorable that it is hard to maintain a pose of critical sophistication. There are some things to find fault with. The market for U.S. government securities of all kinds is discussed under the general heading, "The Money Market," which is inconsistent with the proposition that only very short maturities of Treasury securities belong in the money market. The chapter on evolutionary change in the capital markets indicates only incidentally the great change in recent years in the role of financial intermediaries in the market for corporate equities. A book published in 1964 should not include a statement that new issues of Treasury securities are not formally underwritten, in view of the offering of two new Treasury issues for competitive bidding in the early part of 1963.

But the faults are of small magnitude relative to the merits. Perhaps the best quality of this book as a textbook for undergraduate use is that it is very well written. Sentences are short and lucid. Organization is excellent, despite the necessity for some repetition. Concepts are clearly developed and there are no needless displays of erudition to befuddle students. In truth the evidence of Robinson's scholarship is his ability to reduce complex concepts to their fundamentals and to describe them simply.

The other great merit of the book is that it presents such a clear view of the structure of the financial markets and provides such excellent orientation of the markets to the system of saving and investment. These qualities are very important in a text for students at a relatively elementary level of study. With this book they should neither bog down in abstractions of economic theory nor become lost in a maze of technicalities about the operation of markets.

Forecasting is always hazardous, but there is reason to think that this book will have a favorable influence upon the development of undergraduate curricula in business and economics. It provides a basis for a course concerned mainly with understanding of financial markets, rather than mainly with is-

sues of public policy in the area of money and credit. Such a course would be an excellent supplement to the conventional money and banking course in economics departments. In business schools it might plug the curricular gap that permits many students to emerge with degrees in business, but with little knowledge of the market system for financing business.

HARRY C. SAUVAIN

*Indiana University*

### Public Finance; Fiscal Policy

*Taxation of Foreign Investment Income—An Economic Analysis.* By PEGGY BREWER RICHMAN. Baltimore: The Johns Hopkins Press, 1963. Pp. ix, 140. \$5.50.

This slender but weighty book repairs much of economists' relative neglect of the theoretical effects of alternative tax policies for foreign-source income. The United States is the largest supplier of foreign investment funds, and its foreign capital stock is about half the world total. Effects of national tax systems and tax differentials on international capital movements become of increasing importance with rising mobility of funds and closer integration of national economies. To a considerable extent U.S. tax policy has reflected the view that any foreign investment has net advantages to the investing country and to national influence and prestige. Several tax provisions are favorable for income derived from investments abroad, including a foreign tax credit, deferral of tax until income earned by foreign subsidiaries of U.S. corporations is repatriated, and reduced tax rates for Western Hemisphere Trade Corporations. In recent years tax-sparing treaties have been negotiated, but not ratified, with several underdeveloped countries which give tax exemptions or rate reductions to attract private investment from the United States.

With recent balance-of-payments difficulties, the Administration in 1961 recommended tightening of the tax treatment of foreign investment income and elimination of tax deferral for retained profits of U.S.-controlled foreign corporations in the developed countries. The Congress enacted several major changes, the first in some twenty years, that lessened the favorable tax treatment of foreign investment, including a "grossing up" of foreign taxes and income in computing U.S. tax liability and tax credit, the taxation as ordinary income of capital gains arising from the liquidation of reinvested foreign earnings, and limitations on tax deferral where tax-haven and avoidance operations were most often involved. But Congress was unwilling to accept the broad recommendations for disallowal of tax deferral in foreign subsidiary operations in the developed countries. The form of tax treatment that will best serve the general interests of the United States and world-wide economic efficiency remains controversial. While conflicting views have been presented to the Congressional tax committees, comparatively little independent theoretical and empirical work has been done on these issues.

This book, begun as a doctoral dissertation at Johns Hopkins, is a creditable extension of Richard Musgrave's *The Theory of Public Finance* into the

international tax area. The primary, almost exclusive, concern is with the taxing of foreign-source income, particularly under the corporate income tax, and with the economic effects of capital flows as influenced by alternative sets of tax policies. No detailed consideration is given to legal, administrative, and accounting complexities.

At the beginning Peggy Richman systematically examines the criteria and principles by which tax policies for foreign investment income may be judged and reconciled with the varying tax claims of overlapping national jurisdictions. Concepts of tax neutrality and equity become more complex at the international level. Neutrality, for example, may be viewed with respect to all investors of one country so that tax considerations do not influence decisions to invest at home or abroad—capital-export neutrality. Or neutrality may be regarded as subjecting all who invest in one particular country to the same tax treatment—capital-import neutrality. Neutrality may relate not only to the places of investment, but also to tax effects on the residence of shareholders, location of management, place of incorporation, or forms of business organization, and these effects may be of considerable importance in decisions affecting the international distribution of tax base. What a capital-exporting country regards as equitable treatment in taxing the foreign-source income of its investors will vary, depending upon whether foreign taxes paid are regarded as part of taxable income, costs of earning the income, or full offsets against its own taxes. In addition, international equity involves a fair division of the tax revenue from foreign investment between capital-exporting and -importing countries.

No single policy can meet all of the desirable objectives of neutrality and equity. There is no generally accepted international principle to tax either by source of income or by residence of investor, and in practice a variety of policies is in effect in different countries. Mrs. Richman classifies and analyzes alternative tax systems to show how they serve various economic and equity objectives. The maximization of world welfare calls for capital being invested where the real rate of return is highest and argues for capital-export neutrality. Preference is given to the equity concept of total tax burden, so that the aggregate of taxes paid to both domestic and foreign governments is equalized for all investors receiving equal income from whatever source. International divisions of tax bases are analyzed through the development of an economic model involving a capital-export and a capital-import country with assumed identical production functions for capital and labor. Capital transfers in a perfectly competitive market are assumed to equate real rates of return to capital. In this setting different systems of taxing earnings from foreign-owned capital are shown to produce marked changes in national incomes and in distributions of corporate and personal income tax bases. The export of capital is likely to have adverse effects on the income of complementary factors, while gains to capital may be substantially reduced by the profits taxes of the capital-importing country. The analysis leads to the conclusion that the best general policy for the treatment of foreign-source business income, judged by both criteria of equitable distribution of tax base and investment neutrality, would

be a system of taxing both by source of incomes and by domicile of the investor, with a foreign tax credit but no deferral of tax on retained earnings.

The relative strength of various tax incentives to foreign investment are then examined by comparing the after-tax rate of return for the investor or reinvestor under each tax system, by considering whether he invests or reinvests in his own or the foreign country and whether the foreign investor is to measure his net after-tax income at home or abroad. Consideration is given to tax concessions which deliberately depart from tax neutrality with the objective of stimulating private investment from the United States in the underdeveloped regions of the world and also to tax concessions which appear to result in misallocation of capital among the high-income countries.

Influences on the balance of payments of the participating countries are also explored with a view to the tax effects on initial outflows of private investment, on the volume of after-tax earnings available in the capital-importing country for repatriation or reinvestment abroad, and on investors' decisions to remit earnings or retain them abroad. In each case the effects are analyzed by use of two-country models and consideration of the adjustments involved in reaching equilibrium under different assumptions. Basic relationships are expressed in algebraic terms, and then hypothetical though plausible values are assigned to respective terms to enable a numerical comparison of the indicated effects of alternative tax policies.

The final chapter utilizes Department of Commerce data to sketch the size and distribution of U.S. private foreign direct investments, the net outflow of new funds and reinvested profits for a dozen recent years up to 1961, and the foreign taxes paid by U.S. direct investment abroad for 1957. A comparison of private yields of investments made abroad and at home indicates that in most industrial categories foreign investment has been more profitable to the U.S. investor, and tax differentials appear to have been a rather small factor in the attractiveness of foreign investment. But comparisons of gross domestic yields with foreign net yields, to compare national returns from the relative investments, indicate that only in agriculture and petroleum production has it been nationally more productive for the United States to invest abroad.

The author makes a plea for a more neutral system, consistent not only with world welfare conditions but also with the national interest of capital-supplying countries in retaining a larger share of tax bases. These considerations plus the somewhat precarious current balance of payments of the United States lead again to the suggestion of ending tax deferral with respect to investment in high-income countries and also to a modified foreign tax credit that would allow offset of only up to one-half of the full U.S. tax liability on foreign-source income.

The main virtues of the study are in its theoretical analyses of tax effects on capital flows. Only brief allusions are made to indirect effects upon international trade that may be induced by the foreign investment and to the questions of whether these trade effects may or may not be of an equilibrating character. Mrs. Richman readily acknowledges the need for more detailed and

refined empirical work and for intensive consideration of the effects of foreign investment on the pattern of international trade.

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*Taxation and Economic Development in Tropical Africa.* By JOHN F. DUE. Cambridge: Massachusetts Institute of Technology Press, 1963. Pp. 172. \$6.00.

With growing interest in Africa, in taxation, and in economic development, the publication of this book is a welcome addition to the limited materials available to scholars in this field. The book is based on a series of lectures given by the author during December 3-7, 1962, at Cambridge, under the auspices of the Harvard Law School's International Program in Taxation and the M.I.T. School of Industrial Management. It is based in part on four months of field work in 1962 when Professor Due interviewed officials in the Ministries of Finance, Local Government, and Economic Development in eight African countries (Sierra Leone, Ghana, Nigeria, Kenya, Uganda, Tanganyika, Zanzibar, and Rhodesia).

Although the role of tax policy in relation to economic development is examined in the ninth and last chapter, the major emphasis of the book is on tax structure, the use of various taxes, and such problems as that of collecting taxes; these are dealt with in seven chapters after an introductory chapter on the economic position of the eight countries and their main problems of economic growth. Selected references at the end of each chapter and a valuable index will enable researchers to use the study as a reference for special questions, such as double taxation of dividend income and allowances and exemptions for personal income tax, and for specific information on a particular country.

The tax structure in the eight African countries covered in the book is not very much different from that of most underdeveloped countries with a per capita income of less than \$200 per annum. Thus, the total tax revenue collected in these countries in relation to gross domestic product in 1960-61 varied from 9 per cent in Tanganyika to 17 per cent in Zanzibar (p. 26), while in a larger sample of underdeveloped countries it varied from 8 per cent in India to 24 per cent in the United Arab Republic, with a median of 13.7 per cent (see article by the reviewer on "Taxation Problems and Policies of Underdeveloped Countries" in *IMF Staff Papers*, November 1962). The great dependence on indirect taxes as a source of revenue in these African countries is also similar to the position in most underdeveloped countries (about two-thirds to three-fourths of total revenue originating from indirect taxes) except Rhodesia, where direct taxes account for 67 per cent of total tax revenue which approximates the relationship in the more-developed countries. Kenya has also succeeded in collecting a large percentage of total revenue (44 per cent) in the form of direct taxes.

There are two interesting features in the direct tax system in these African

countries. Firstly, with the exception of the tax in Southern Rhodesia, all income taxes were originally based upon the Colonial Model Income Tax Ordinance prepared in 1922 and, despite subsequent modifications, the U.K. case law and tax guides are applicable to a substantial extent in these countries. The other feature relates to direct personal taxes: with the exception of Zanzibar, these African countries have developed a very simplified form of direct personal tax which is in a sense hybrid between a poll tax and an elementary form of income tax (pp. 61-62). The poll tax is not uncommon in other underdeveloped countries, but in the African countries there is a limited degree of graduation because the amounts are fixed in relation to income brackets.

The author reviews the difficulties of levying direct taxes that are common to most underdeveloped countries, including administrative organization and procedures used for collection (pp. 51-56). While the author states that "it is very difficult to evaluate the over-all efficiency of income tax administration in these countries" (p. 56), there is undoubtedly a lack of trained personnel and too few tax-collecting inspectors, especially in West African countries (e.g., Ghana, where the degree of Africanization has been complete). The trend toward Africanization in East Africa is likely to proceed at a rapid pace in the future. Their system, unlike that of the United States, and following the British tradition, does not permit the taxpayer to calculate his own tax, this being done by the assessing officer. For cases where it is difficult to assess incomes, such as those for traders, the 1961 Ghanaian income tax law provides the method of presumptive income, i.e., the income tax authorities presume the figure of taxable income to be not less than a certain amount, e.g.,  $2\frac{1}{2}$  per cent of gross turnover for traders.

Most underdeveloped countries would like to mechanize record keeping by using data-processing equipment, but these countries have not rushed into using the latest devices. The Sierra Leone Administration quite rightly believed the use of mechanization to be unwarranted at present. Governments can raise more revenue by tightening collecting machinery through simple enforcement measures and giving adequate rewards to tax collecting officers than through mechanization of record keeping.

A number of details are presented on the exemption given to newly established industries either directly or through lower customs duties on equipment (pp. 35, 86, 153-54), and Due mentions the administrative difficulties experienced in Ghana and Nigeria in using the tax-holiday type of legislation. However, he does not conclusively state whether even the tax-incentive types that he prefers, namely accelerated depreciation allowances and the provision for loss carry-forward, have been instrumental in promoting industrialization and economic development.

The indirect taxes covered are customs, excise, and export duties, but the space devoted to indirect taxes is much less than the amount warranted by their importance. While customs duties are dealt with adequately, very little information is given on taxes on motor fuels, for example, and the uses made of such receipts. The collection of indirect taxes is also hampered by lack of trained personnel and encounters enforcement difficulties. On customs duties

he concludes that "all developing countries that rely heavily on customs must look forward to the future, when, with foreign trade constituting a constantly declining portion of national income, other tax resources must be developed" (p. 159), which to the reviewer is not self-evident. Undoubtedly, there is need to develop other sources of revenue than customs because such revenues are subject to cyclical variations, and not because there will be a long-term decline in the importance of foreign trade. In some countries foreign trade has become more important with development, while in others it has become less important. There is no necessary correlation between growth and relative importance of trade that merely depends on the type of development.

The author, in the discussion of direct taxes versus indirect taxes (pp. 147-51, 156-58), points out the usual advantages and disadvantages without taking a definite position between the two. But the present reviewer feels that African and other underdeveloped countries should attempt to develop direct taxes only as a long-run objective and should concentrate on collecting revenue by the easier method of indirect taxes because the advantages of equity and stability of direct taxes are outweighed by the major problem of trying to obtain noninflationary sources of government finance to avoid deficit financing. Lastly, Due's discussion of the important tax device of marketing boards, as in Ghana, is very limited, and despite the disadvantages of such devices, the revenue-yielding aspects have generally been understressed by its critics.

U TUN WAI

*International Monetary Fund*

*Public School Finance—Economics & Politics.* By JESSE BURKHEAD WITH B. M. GROSS AND C. S. BENSON. Syracuse: Syracuse University Press, 1964. Pp. xii, 394. \$8.75.

"It is the theme of this volume that the resources for a more adequate support of public education are available; the task is to mobilize these resources and to utilize them effectively" (p. 359). To accomplish this task, the author hopes for organizational innovation, both within the school systems and, externally, at all levels of government. Many suggestions are made, for example: school boards should increase citizen involvement, chief state school officers will have to immerse themselves in the political process of public resource allocation, federal agencies should collaborate with each other and coordinate with state and local educational agencies and above all stimulate the states (pp. 363-64).

The audience for which this book was intended presumably covers a wide range of persons professionally concerned with the support of public education. Each reader will be familiar with some of the topics. The specialist in public finance will recognize recurrent quests: the attempt to measure the benefits of educational expenditures, the search for the optimum method of dividing these expenditures among the three levels of government, the groping for better formulas for state and federal aid, the pursuit of ways to improve or find alternatives to the property tax. The political scientist will have

studied the tensions impeding federal aid—race, religion, localism, urban-rural rivalries, and control of the educational apparatus. The sociologist will have tried to unravel the complex of ills that beset the depressed areas of our large cities. The historian will know the halting course of federal aid to education from its wellspring in 1787. The school administrator will be all too aware of the special demands of his job, the necessity to provide an intangible and complex output for clients with whom he must come into unusually close contact.

As an economist, I liked best a report (Chapter 3) of empirical research on the determinants of school expenditures. Four dependent variables (total and local expenditures, each per capita and per pupil) for selected school systems in 23 states were used in least-squares multiple regressions. With 20 independent variables, the results were profuse, although none of the coefficients of multiple correlation was high. By comparing the coefficients for total and for local expenditures, it was possible to discern the effects of state and federal aid. As might be expected, total expenditures tend to vary with cost while local expenditures reflect community income levels. I also enjoyed three chapters (12-14) which describe the work of the Physical Science Study Committee, the government's experience in administering the National Defense Education Act, and the aid given to federally affected schools.

As a taxpaying parent, I was cheered to learn that there are avenues for improvement in addition to mobilizing my checkbook. Almost *any* experiment in teaching technique seems to push students ahead because of the "strong interest-enthusiasm factor" (p. 89). The quality of instruction may be improved by team teaching and by altering the technology of education, which has hardly changed since 1890 (p. 80 ff.).

As an aging scholar, I fuss about finding the footnotes in a block at the end. Anyone who reads this sort of compendium wants *instant* footnotes! My other comment is also captious. Perhaps I have merely failed to adjust to the new entrepreneurial style of research, but I still feel that it fosters dutiful—and, hence, inert—exposition; lonely thinkers are more likely to impart life and purpose to their thoughts. This book is really the work of 18 men, who in turn acknowledge the help of a host of others. Two chapters were "specifically commissioned for this volume."<sup>1</sup> Eleven of the chapters are summaries by Jesse Burkhead of monographs published by the Syracuse University Press as "The Economics and Politics of Public Education Series." This leaves the first and last chapter as fresh territory for the author, but only in the last few pages does he seem to get free from the task of trying to blend an interdisciplinary seminar into a point of view. Too many minds can spoil the flavor of research. I fancy that this book would have been more provocative if each monographer had distilled his own work. What I *really* long for is the lifework of some dogged schoolman who has neglected a despairing wife and three wan children to prove to the nation that public education should not be stinted.

ALICE JOHN VANDERMEULEN

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<sup>1</sup> "Administration of the Public Schools" by Bertram M. Gross and "State Aid Patterns" by Charles S. Benson.

*Public Finance—An Introduction to the Study of the Public Economy.* By ANSEL M. SHARP AND BERNARD F. SLIGER. Homewood, Ill.: The Dorsey Press, 1964. Pp. xiii, 411. \$7.50.

I teach a junior-senior-level course in public finance for students majoring in economics, most of whom are not likely to do graduate work in the subject. Working within the context of a liberal education, I conceive the major objective of such a course to be teaching the students to think intelligently about the economic aspects of certain public issues. For a text to be adequate for this course, it must apply the tools of economic analysis to the major problems of concern to public finance: allocation of resources between the public and private sectors of the economy and within the public sector; division of the burden of government support; effect of government activity on the private economy; and problems of stabilization. Ancillary issues which should also be covered briefly are: the burden of the debt, the techniques of budgeting, and the particular fiscal problems of a federal system.

On my shelves are eight textbooks published or revised since 1959, all of which indicate in the preface that they have been written for courses like mine. Each of these books considers all of the major issues listed above, but there is a decided imbalance in presentation. Just short of half the pages (47 per cent) on the average are devoted to the discussion of government revenues. Student interest, importance, or complexity cannot justify this much attention being given to this one aspect of public finance. Much of the material presented on taxes is historical and institutional, with most of the analysis being confined to a partial-equilibrium examination of particular taxes. At best this type of analysis is a demonstration of a useful tool; frequently it is dull and inconclusive; and at its worst it is misleading and misdirected. What are we able to say on the effect of a particular tax on work effort; risk acceptance, saving, price, output, etc.: only that our theoretical analysis is inconclusive and our empirical evidence almost nonexistent. Certainly we should show students these tools but not belabor our poverty.

Concentration on taxes means that the other problems of public finance get short shrift. Only 14 per cent of the pages in the same eight texts were devoted to fiscal problems and only 12 per cent to the question of allocation. Perhaps the brief coverage of compensatory finance can be justified, since most colleges do offer a separate course in macroeconomics in which policy is covered. But the question of allocating resources between the public and private and within the public sector does deserve more attention. Ansel Sharp and Bernard Sliger say: "One of the very few topics on a college campus that can compete with football for student interest is taxes" (p. 193). Perhaps so, but I have never heard students discussing the effects of the income tax on the labor-leisure choice. My students argue about the size of the government sector, the propriety of Medicare, federal aid to education, the authoritarian nature of social security; in other words, the allocation of resources. Unfortunately, instead of exploring the basis for these controversies, most texts give little more than a historical elaboration of the growth of particular components of the public budget.

The over-all balance of the Sharp and Sliger book is better than most.

While the tax analysis suffers from the failings mentioned above, their presentation has the advantage of being short—only 30 per cent rather than half the book is devoted to public revenues. Furthermore they do give some consideration to the allocation problem. Unfortunately they suggest that the resolution is simple: "It follows, then, that the distribution of resources between public and private use should be carried to the point where the last unit of a resource used by the public adds the same to social welfare as it does to social cost, for, in this case, total welfare of society is maximized" (p. 18). However, they recognize that this principle is not used in practice: "Two reasons are: many public officials are unaware of the principle of equating marginal social benefit and marginal social cost; two, application of the principle implies that reasonably close estimates of social benefits and cost can be made" (p. 18). The immediately following paragraph starts: "The difficult problem of estimating social benefits and social costs, in some instances, is not insurmountable" (p. 19), and goes on to give an 18-line description of cost-benefit analysis. Nothing is said of the dubious nature of community indifference curves, or, except for the quote above, the impossibility, in most cases, of establishing the location of social indifference curves. It seems to me misleading to leave students with the impression that the disagreements over public policy could frequently be resolved were it not for the ignorance of simple economics on the part of our lawmakers. Even Senator Douglas may have difficulty applying their dictum.

The one rather different feature of this book is the amount of space devoted to the examination of four specific programs: education, highways, social security, and city government. Again it is unfortunate that the authors do not take this opportunity to explore the complexities of the allocation problem except with regard to roads. For education it is solved by fiat: "This coefficient [an income elasticity of one] reveals that people will have to place different values on education, or that government will have to be more arbitrary in its handling of school finance if the nation is to have adequate—or—better school facilities in the future" (p. 343). Students are gullible; authors of texts have a duty to make explicit what are only value judgments.

The failings enumerated above are common to most texts on public finance. For a course such as mine this is as good a book as any I have seen. It is well written, easily understood, and the presentation is a useful balance of theoretical and institutional material. It deserves consideration for classroom adoption.

MILLARD F. LONG

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### International Economics

*Industrial Growth and World Trade—An Empirical Study of Trends in Production, Consumption and Trade in Manufactures from 1899-1959 with a Discussion of Probable Future Trends.* By ALFRED MAIZELS. New York: Cambridge University Press, 1963. Pp. xxiii, 563. \$14.00.

In this voluminous work, Alfred Maizels attempts a comprehensive explanation of the effects of industrial growth on the pattern of world trade in

manufactured goods over the past 60 years. For both its statistical coverage and analytical quality, this should rank as one of the leading studies of the past decade in the field of international trade.

Maizels has two central themes. The first is the relationship between industrial growth in the developing countries and the structure of their foreign trade. The second is the changing set of trade relations among the established industrial countries. The main feature of the latter is the decline of Britain as the dominant exporter of manufactured goods since the beginning of the century. These two analyses are skillfully interwoven to provide an over-all explanation of the changing patterns of production and trade in manufactured goods.

The last study of similar scope was Hilgerdt's classic *Industrialization and Foreign Trade*, published in 1945. Maizels extends Hilgerdt's empirical base to include a much larger coverage of less developed countries and adds another dimension to the analysis by subdividing manufactured goods into 12 categories. Hilgerdt's principal conclusion—that the trade-reducing effects of industrialization are far outweighed by the expansion of total demand for manufactured goods—is amply supported and extended in the new study.

Maizels makes a major empirical contribution by constructing trade matrices of the exports of 12 categories of manufactured goods from principal suppliers which account for 90 per cent of world exports. To the extent possible, these are compiled for 47 countries of destination and cover seven selected years between 1899 and 1959. The countries of destination are chosen to give fairly equal coverage to three types of economy: (i) advanced industrial countries; (ii) countries in process of industrialization; (iii) nonindustrial (primary-producing) countries.

Changes in the volume and composition of world trade in manufactures are traced to demand factors (indicated by the rise in each country's GNP and total consumption of each commodity group) and changes in supply conditions (indicated by variations in suppliers' shares of markets and their relative export prices). While these variables are never put together in a formal model, they are used fairly systematically to link up the growth in total demand for the several categories of manufactured goods, the rise in the share of their internal demand that is supplied by the industrializing countries, and the changing shares of exports in each category provided by the principal suppliers. As a by-product of this comprehensive analysis, Maizels is able to describe the typical growth and trade patterns of each of his three groups of countries and to study the changing pattern of trade in each major commodity group separately.

Much of Maizels' explanation of the observed changes in demand and supply consists of listing contributory factors on a fairly intuitive basis. Some of his most interesting pieces of analysis, however, are those in which he attempts a more formal test of alternative hypotheses by the use of multiple regression analysis. Among the most useful conclusions reached on this basis are:

1. The demonstration in Chapter 2 of the similarities and differences between cross-country patterns of growth in manufacturing and patterns derived from time-series for ten countries.

2. The demonstration in Chapter 5 that the differences in export performance of the developing countries have stemmed mainly from differences in demand for their principal exports, with no evidence of a general reduction due to supply limitations (although there are some notable individual examples).

3. The relations among rise in levels of income, the degree of import substitution, relative prices and net demand for manufactured imports (Chapter 6). Inter-country analysis shows that the rise in consumption of manufactured goods outweighs the effects of import substitution and typically leads to a rise in manufactured imports. An analysis of individual countries over time shows only a few exceptions—notably Argentina, India, and the United Kingdom—to this general conclusion.

The major faults of this otherwise admirable study are its loose organization and somewhat erratic methodology. Maizels' failure to set out a general model or to analyze the relations between the variables which he can observe and the underlying explanatory variables makes it more difficult to assess his results. It would be useful to know, for example, whether an alternative grouping of countries would give significantly different results, since this critical assumption is made without a very convincing justification. Offsetting this methodological weakness, however, is a good sense of what is empirically relevant and "feel" for the data.

Since international trade is the net result of a large number of factors affecting the internal workings of each economy, it is probably inherent in the subject matter that an empirical study should turn out to be large, repetitious, and difficult to sum up in simple terms. Much of Maizels' analysis is admirably clear, but he sometimes appears to be the prisoner of his multi-year trade matrices, which almost force him to produce many more analyses and tables than the reader can readily digest. It would be most unfortunate, however, if the occasional tedious going should limit the understanding and dissemination of Maizels' principal conclusions. There is a wealth of source material and insights here for policy-makers concerned with development in both the advanced and the less developed countries.

HOLLIS B. CHENERY

*Agency for International Development*

*The Politics and Economics of European Integration—Britain, Europe, and the United States.* Rev. ed. By U. W. KITZINGER. New York and London: Frederick A. Praeger, 1963. Pp. 246. \$5.50; paper, \$2.25.

No book that I have read provides as clear and concise an account of the origins and postwar development of the European integration movement as Kitinger's brief volume. This is no plodding chronology of events and recital of treaty provisions, but a lively and perceptive account of the dynamics of the integration process written by one who is obviously moved by a deep personal commitment to the supranational idea. U. W. Kitinger, Fellow at Nuffield College, Oxford, was for six years an economist with the Council of Europe, a body which, ever since its establishment in Strasbourg in 1949, has

served as a high-level forum and stimulator of new steps in the movement for a united Europe.

The first half of the book consists of four closely related chapters: a historical summary of the main forces behind the movement toward European unity; a brief exposition of the economic provisions of the three treaties and an account of how they had been carried out in practice until approximately mid-1962; an examination of the institutions and political implications of the treaties; and a discussion of the association of African countries with the Community. None of these matters is treated in any great depth, but Kitzinger does have a sharp eye for the highlights of his subject.

If one were to describe a dominant theme running through these chapters and indeed throughout the book, it would relate to the self-generating character of the integration process, once the movement gets under way, on the basis of a core of precise and irrevocable commitments. Each move toward freer intra-Community trade, for example, not only expands opportunities but also creates problems (e.g., the need for tax harmonization) which, on the assumption of no backsliding, can only be solved through new regional initiatives in other spheres. In this process a deeper community of interests is consolidated and a new plateau is established from which regional solutions are again sought to new problems. In projecting this dialectic, the author sees the Community institutions as playing a key role. The dedicated Europeans in these bodies exploit tactical situations in order to advance Community solutions to rectify the imbalances caused by partial integration. In Kitzinger's view, "a thoroughgoing community of economic interests will lead more and more to a political community of interests: and then a Political Community may not be far off."

Although there is a great deal of substance to the slippery-slope thesis of European integration, two caveats should be entered. First, as the author himself recognizes, progress is conditioned on political will and support. Despite the internal dynamics of the integration process, a De Gaulle can slow it down or stop it at any point beyond the precise but sharply circumscribed commitments of the treaties; and somehow Europe could learn to live with its problems and contradictions. Second, outside the commercial policy field, some of the key issues facing the members of the Common Market do not naturally lend themselves to solutions on a Community scale but rather on a world-wide or industrial-country basis. The Group of Ten rather than the Six is the more logical body within which to develop reforms of the international monetary system; Working Party 3 of the OECD is a natural place in which to deal with the policy implications of imbalances in the over-all payments positions of EEC member countries; and the Development Assistance Committee rather than some EEC institution would appear to be the most effective forum within which to coordinate bilateral aid policies toward less developed countries. To be sure, the EEC does try to develop a common point of view on these matters, but the inherent pressures for a Community policy are weaker where rational solutions require action on a world-wide scale.

According to the publisher, the present volume is a revised and expanded

edition of a book previously published in Great Britain under the title *The Challenge of the Common Market*. Not having read the earlier edition, this reviewer cannot comment on the changes or additions included in *The Politics and Economics of European Integration*. In the present version, all of the second half of the book deals with Britain's relationship to the Common Market, starting with the Europe-wide free-trade-area negotiations which collapsed in 1958, and ending with the abrupt rejection early in 1963 of Britain's application to join the Community. With profound sympathy and understanding for the United Kingdom's predicament, the author carries us through the soul-searching that led to the historic British decision to end its separation from Europe and reviews the main issues in the detailed negotiations with the Community in 1961-62.

In the light of all the attention devoted to the British-European relationship and the absence of any separate treatment of U.S. policy toward the unity of Europe, one cannot but feel that the book's subtitle, *Britain, Europe, and the United States*, is a misnomer. So much mystique surrounds the recent concepts of community and partnership in the Atlantic area, that a real service would have been rendered by including a comparable analysis of the evolving U.S. conception of its relationship to Europe.

In his last chapter Kitzinger tries to assess the implications of the French veto of British membership in the EEC. Here the author takes off into space and finds a cloud with a bright silver lining. He recognizes that the major issues of our time transcend both national and regional solutions and sees in this fact the basis for a new role for Britain. In attacking such problems as commodity arrangements and "world development plans," the United Kingdom would take the lead in building a network of "functionally oriented, democratically controlled and geographically interlocking institutions as the basis . . . of a higher, more civilized, even perhaps a more democratically responsible world order." In working toward this goal, the United Kingdom would take several leaves from the Common Market book, since the EEC has after all pioneered a new approach to transcending national interests. Kitzinger provides no blueprint for his soaring idealism, no clue as to the nature of the solutions for which a new institutional structure is needed. One wonders, therefore, whether his cosmic chariot has not been put before its team of horses.

ISAIAH FRANK

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*World Monetary Reform: Plans and Issues*. Edited by HERBERT G. GRUBEL. Stanford: Stanford University Press, 1963. Pp. xiii, 446. \$10.00.

Perhaps I should have disqualified myself as a reviewer of this volume, since I am one of the authors of the writings collected. But being only one of 22 authors, I do not think the conflict of interest is serious.

The purpose of the volume is to bring together the major contributions to the discussion of world monetary reform; to let the authors and the critics of the various reform plans speak in their own words; and thus to let the student find the most representative quotations on the subject within the covers of a single volume. The editor modestly confines himself to the task of selecting

the papers, articles, testimonies, and abstracts, and of contributing a competent introduction, only 11 pages long.

Part I, "Centralization of International Reserves," contains a 40-page condensation of Triffin's book (published in 1960); Keynes' "Proposals for an International Clearing Union" (1943); the "Stamp Plan—1962 Version"; Angell's *EJ.* article (1961); and four critiques of the Triffin Plan, by Harrod, Altman, David Rockefeller, and Leland Yeager (all published in 1961).

Part II, "Improvement of the Gold Exchange Standard System," contains one of Bernstein's statements prepared for Congressional hearings (1962); a condensed fusion of two articles by Harrod (1961 and 1962); the English version of an article by Per Jacobsson (published in French in 1962); Lutz's Princeton essay on the multiple-currency standard (1963); a brief statement by Machlup prepared for Congressional hearings (1962); one of Roosa's articles (1962); one by Wallich (1961); and a piece excerpted from Zolotas' writings (1961 and 1962).

Part III, "Flexible Exchange Rates and Return to the Gold Standard," includes the revised version which the Joint Economic Committee published of Meade's article from the *Three Banks Review* (1961); Rueff's plea for a return to the full gold standard (1961); Heilperin's endorsement of this plea (1962); a review article by Balogh (1960); a not easily available survey by Harry Johnson (1962); a panel discussion before Congressman Reuss by Bernstein, Harry Johnson, and Machlup (1962); the chapter, slightly condensed, on "Recommendations" from the Brookings Committee Report, for which Walter Salant is chiefly responsible (1963); and finally another article by Triffin (1961).

If Grubel's collection had appeared one year later, it might have included the newest crop of plans, such as the Posthuma Plan (1963), the Kaldor Plan (1964), and the newest Bernstein Plan (1963-64). But, although "Of making many books there is no end," there must be an end of making one book. Every anthology must have a cut-off date, and Grubel's collection is unquestionably as representative of the present-day discussion as one may ask.

Experts in the field will appreciate this volume as a convenient reference book; teachers in monetary economics and in international economics will be glad to have this time-saving aid in the preparation of their reading lists; students will prefer to find so many assigned writings within the covers of a single volume; and librarians will delight in being saved the trouble of putting 20 different volumes of journals and public documents on their reserved-book shelves.

Fritz Machlup

Princeton University

### **Business Organization; Managerial Economics; Marketing; Accounting**

*Quantitative Decision Procedures in Management and Economics—Deterministic Theory and Applications.* By C. R. CARR AND C. W. HOWE.  
New York: McGraw-Hill Book Co., 1964. Pp. xv, 383. \$8.95.

This book is intended for use in a one-semester course in quantitative meth-

ods for students with little mathematical background (some understanding of calculus is assumed, however). The entire work is devoted to a study of deterministic optimization problems. A forthcoming work will deal with stochastic decision problems.

The current work is divided into three sections. The first, consisting of 121 pages, deals with the optimization of a function of a single real variable. It attempts to give some notion of what is involved in constructing a mathematical model and what is meant by an optimization problem. It also gives definitions pertaining to functions, continuity, derivatives, sets, etc. The section begins and ends with a discussion of the simple lot-size model of inventory theory. The second section, consisting of 149 pages, treats optimization problems involving a finite number of variables. The main topic covered here is the simplex method of linear programming along with an introduction to duality and parametric programming (no discussion of the transportation method is given). Also, brief discussions of the Kuhn-Tucker conditions and quadratic programming are provided as well as a somewhat more extensive discussion of all integer linear programming. A brief treatment of vectors and matrices is to be found in an appendix to the chapter on linear programming. The third section of 100 pages continues the study of optimization problems involving several variables, and introduces the notion of step-wise maximization that is so important in dynamic programming. A discussion of dynamic programming, which is restricted mainly to showing how the recurrent relations can be solved analytically in certain simple cases, is included. The text ends with an additional discussion of the lot-size model. Problems for solution are to be found at the end of most sections.

The authors' objectives are to provide a text that familiarizes the student with the "mathematical way of thinking" and which also develops sufficient competence to use the techniques studied. The task of achieving these objectives in a one-semester course with students of limited mathematical background is a very difficult one. It is the reviewer's belief that this text is not completely successful in meeting these objectives. The fundamental reason for this is that the entire method of development seems inappropriate for the audience, even if one agrees that the material covered is precisely what should be covered. The authors have not chosen to develop the material from an elementary point of view, with a strong emphasis on giving the reader a good intuitive understanding of the subject, but have instead chosen an approach which is based on making use of a large number of advanced theorems that are simply quoted without proof. Only occasionally is some very elementary result proved, and when this is done, other unproved theorems are usually required in making the proof. A further unfortunate feature of this procedure is that often not enough theorems are quoted to cover all cases. For example, in the development of the simplex method, it is assumed that the set of feasible solutions is a convex polyhedron. Thus no mention is made of the fact that the rules developed break down if all  $x_i \leq 0$  for the vector to enter the basis (thus in reality indicating that there is an unbounded solution). Similarly, no discussion is given of the fact that degeneracy could cause problems (the authors claim that the objective function increases at each step in the

simplex method—something that cannot be guaranteed when degeneracy is present), but that in practice it doesn't. Neither is any theorem stated which guarantees that when all  $z_j - c_j \geq 0$ , one has an optimal solution (indeed the criteria for terminating the iterative procedure receives only a passing mention). Even something so important as the artificial variable technique for finding an initial basic feasible solution receives only a cursory description without any detailed example. Still another unfortunate feature of this approach is that rules are sometimes given without any indication whatever of why they are needed. An example of this occurs in the discussion of integer programming, where it is stated that the matrix of the coefficients must be converted to an all-integer matrix before starting the computational procedure. No mention is made as to why this is necessary (the reason is simply that all the constraints are assumed to be inequalities, and in order to guarantee that the slack variables will also be integers, the conversion must be made). The method of development thus makes the text something very close to a cookbook, even though one which appears very learned and lends somewhat of a touch of mystery to the subject.

The authors also chose to omit completely any discussion of the problems involved in numerical computations. This can indirectly give a number of seriously erroneous impressions. For example, in reading the text one gets the impression that it is just about as easy to solve integer programming problems as it is to solve linear ones. One case where the authors do make a reference to computations is very misleading, if not totally incorrect. They assert that the equivalence under certain conditions between saddle-point problems and optimization problems is important because one wants to convert a nonlinear programming problem to a saddle-point problem so that one can solve it efficiently using a gradient method (in reality, one would much prefer not to use the Lagrangian when applying a gradient method).

Finally, the text does very little to develop the student's ability to formulate mathematical models from real world situations. In the text the only example illustrating this is the simple lot-size model of inventory theory, and even though almost 50 pages are devoted to it, one never really gets a clear idea of what the model is or precisely what assumptions are being made. The discussion of this model never even reaches the stage of introducing the notion of a reorder point or indicating how to handle nonzero lead times.

Several other minor annoying features of the text which might be mentioned include the use of many abbreviations such as GMP (general maximum problem), GDP, BMP, BOP, etc., which make it read like a military-procedures manual, and the insertion of seemingly vacuous propositions (as the authors call them) at various points in the text, such as "Reality is a subject for philosophers, not scientists."

Because of the above considerations, the reviewer cannot provide a very enthusiastic endorsement of this work for the purpose that the authors have in mind. It would also seem to have only limited value as a reference work.

G. HADLEY

*University of Chicago and Universidad de los Andes*

### Industrial Organization; Government and Business; Industry Studies

*Profitability and Size of Firm.* By H. O. STEKLER. Berkeley: Institute of Business and Economic Research, University of California, 1963. Pp. 112. \$2.50.

During the past 30 years, there have been a number of studies, by Epstein, Crum, Steindl, Osborn, and others, of the relationship between a firm's size and its profitability, the primary purpose being to obtain clues regarding economies of scale and to measure the likelihood that firms of various sizes can survive and expand. H. O. Stekler's monograph, prepared under the direction of Richard Holton and Joseph Garbarino, is the latest addition to this literature.

Using the data from the *Statistics of Income*, Stekler describes the relationship in all manufacturing and in each two-digit industry between firm size, measured by total assets, and profitability, measured in a variety of ways—before taxes, after taxes, with and without adjustments of officers' compensation, profits as a percentage of net worth, and profits plus interest as a percentage of total assets. The average relationships during 1947-54 and 1955-57 are presented in each case.

His principal findings are as follows: First, including all manufacturing firms (whether they made profits or losses), there is in most cases a tendency for the after-tax profit rate to increase with size of firm up to \$10-50 million in assets, but to decrease thereafter. Turning to individual two-digit industries, the size of firm, measured by assets, where the after-tax profit rate<sup>1</sup> in 1947-54 was highest was about \$1 million in printing, petroleum, and primary metals; \$5-50 million in apparel, furniture, and rubber; \$50-100 million in food, fabricated metals, machinery, and electrical machinery; and over \$100 million in the others.

Second, using the after-tax profit rate as a rough measure of the ability to expand, Stekler concludes that "the smaller firms [under \$250,000 in assets] as a group are able to survive, but their ability to expand is less than that of the larger firms. The largest firms can generally expand as fast as the remainder of the industry, indicating that no general decline in concentration can be expected" (p. 102).

Third, after applying Alexander's adjustment of officers' compensation, he compares the pretax relationship between size and profitability with the post-tax distribution and concludes that "the tax system probably does not handicap the *relative* expansionary ability of the smaller firms and may actually aid it. As for the larger firms, their ability to grow does not seem to improve" (p. 102).

Fourth, adjusting officers' compensation and excluding firms with losses, he finds an inverse relationship in most two-digit industries between profitability and size of firm. Moreover, including all firms, there is a tendency for

<sup>1</sup> Without any adjustment of officers' compensation and using profits as a percentage of net worth.

the interfirm variation in profit rates to be inversely related to size of firm, the standard deviation among the smallest firms (assets under \$25,000) being about ten times that among the largest firms (assets over \$250 million).

Turning to an evaluation of the book, it seems somewhat long on description and short on analysis. Unfortunately, Stekler makes little attempt to link his empirical findings in any precise manner with the important theoretical and policy questions in this area. He says that he knows of no way to use his empirical findings to estimate economies of scale; and he makes no attempt to explain the observed size-profitability relationships or to relate his findings to the recent attempts to construct econometric models to represent the process of firm growth. His discussion of the relationship between profits and the ability to expand seems somewhat casual.

In addition, I do not understand why Stekler believes that firms with losses should be omitted in Chapter 5, where he is concerned with "the relative ability of firms of different sizes to earn profits." It seems to me that the relationship between size and profitability among firms with positive profits is of little interest. Obviously, because of the inverse relationship between size and the variability of profits, there will generally be an inverse relationship between size and average profitability if one excludes firms with losses (so long as the average profitability of all firms does not rise too rapidly with firm size—which it usually doesn't).

Nonetheless, Stekler's findings should be of interest to many economists interested in industrial organization, and by providing another set of data regarding profitability and size of firm, it should help to stimulate further econometric work in this area.

EDWIN MANSFIELD

*University of Pennsylvania and Harvard University*

*Studies in Process Analysis—Economy-Wide Production Capabilities.* Edited by ALAN S. MANNE AND HARVEY M. MARKOWITZ. Cowles Foundation Monograph 18. New York: John Wiley & Sons, Inc., 1963. Pp. 427. \$14.00.

Process analysis is a search for numerical estimates of the alternative physical outputs which a workshop, farm, industry, or entire economy is capable of producing over the course of time, given estimates of the relevant resource availabilities and the present state of the arts. A process-analysis model is ordinarily drawn up to include alternative known engineering or agricultural techniques that would be serious candidates for adoption under plausible changes in resource or demand constraints. Thus, at the expense of being much more arduous in initial set-up than input-output analysis, process analysis reintroduces into the disaggregated structural model some part of that substitutability among resources that is naïvely exaggerated in GNP projections, but that is clearly oversuppressed by the very act of disaggregation. Non-linear relationships within each technical process are allowed, where appropriate, but an attempt is made to keep the various processes independent and additive. The hope of the authors of these initial studies is that a stock of jigsaw-puzzle pieces will accumulate upon the shelf, each piece being a set of processes for a particular industrial or agricultural sector. The individual sec-

tor studies can then be combined into multisector models at incremental costs lower than the original outlays. When taken from the shelf for fitting into the larger pattern, the pieces will need to be more or less drastically reshaped in the light of newer engineering knowledge; but presumably "the updating of production coefficients (which should be done periodically for all models) is a much easier task, more subject to delegation and routine procedures, than is the original model-building."

The sectoral models brought together in this volume are studies of U.S. petroleum refining (A. S. Manne), U.S. petroleum transportation (T. A. Marschak), South American fertilizer production and plant location (T. Vietorisz and A. S. Manne), the spatial pattern of U.S. grain production (E. O. Heady and A. C. Egbert), iron and steel production (T. Fabian), and metal-working shop activities (H. M. Markowitz and A. J. Rowe). The book culminates in a ten-year investment-planning model of a number of key industrial sectors of the Mexican economy (A. S. Manne), which shows the kind of results that can be obtained in one summer, with a total expenditure of less than one man-year, when some of the industries have already been characterized through individual sector studies. In addition, there are brief general discussions of problems and possible future approaches to handling multisector models of agricultural production, transportation, and marketing (K. A. Fox), metal-working (T. Vietorisz), and economy-wide development planning (T. Vietorisz). The chapters by Vietorisz are particularly stimulating overviews. The brief appendix entitled "Basic Concepts of Activity Analysis" is obscure from the viewpoint of a novice, fragmentary and unedifying for one already acquainted with the mathematical techniques, but apparently is only intended as a few prefatory words to its bibliography.

While this book gathers together a number of studies carried out since the mid-fifties, it is not a compendium of all the engineering production functions lined up on the shelf to date, and unfortunately no attempt is made to supply a complete bibliographic compilation of references to such studies. Even for the sectors here analyzed, the processed data offered in chapter appendices are not complete. If one should decide to move into the field, he had better contact the authors directly. Indeed, the book seems to be mainly intended to inspire potential research workers to carry on with the job of enlarging the body of sectoral studies, and increasing detail in areas already broached. The book is not addressed to students; it is written from the point of view of a professional worker explaining to his successor on the project just what has been accomplished so far, and offering exceedingly valuable practical suggestions about how to make the next ten years easier than the pioneering decade. From one paper to the next the authors protest: "This paper represents no more than a modest attempt to demonstrate the feasibility of constructing a process analysis model for a single sector." "The new models which we have underway make several improvements over those summarized here." "The problem of analyzing the capabilities of the metal-working industries, it seems to us, is neither hopeless nor solved." "The reader would do well to remain skeptical about the specific conclusions reported here."

As would be expected, the multisector Mexican model is particularly unre-

liable. All sorts of unfortunate simplifications had to be indulged in to avoid searching out any more information than could be turned up within the summer of 1961: labor inputs were entirely neglected; the U.S. metal-working industries' technology described earlier in the book was transferred whole hog to Mexico; demands from several important extractive sectors (sulfur, coal, wood), as well as agriculture, textiles, construction and the distributive industries, remained exogenous in this model, in the same category as government and household final demand. The calculations were aimed at minimizing foreign loans and aid, subject to allowing the "key" sectors (electricity, rail freight, heavy chemicals, paper, oil, steel, aluminum, copper, cement, and certain metal products, including capital equipment for the key industries) to expand in order to satisfy the demands associated with two alternative growth rates of real GNP, 5.5 per cent and 8.0 per cent over the decade 1960-70, and at the same time to ensure compound growth in investment activity so that the GNP growth rate would continue beyond the planning horizon. The reader may remain skeptical of the results, as advised, but he would do still better to remember that short-order decisions in these important areas are continually being made, if only by default. A close look at the intricate workings of a technical mechanism will give more aid in controlling the apparatus than just winding it up and trusting its maker.

HELEN RAFFEL

*University of Pennsylvania*

*Steel Decisions and the National Economy.* By HENRY W. BROUDE. Yale Studies in Economics No. 16. New Haven and London: Yale University Press, 1963. Pp. x, 332. \$7.50.

In this study, Professor Broude applies a variety of analytic methods from economics and related disciplines to the examination of some aspects of U.S. steel industry performance. Specifically, he asks: (1) To what extent have limitations on steel capacity constrained U.S. economic development? (2) How are management decisions concerning investment in steel capacity made in response to external demand conditions and internal corporate objectives? (3) What, if any, policies should be adopted to improve the response of the steel industry to national economic needs?

Broude first investigates the extent to which steel has played a critical role in cyclical and secular movements of the economy. He finds little historical evidence of stability in capital-output ratios for steel or of stability in relationships between steel and total output. Nor does he find that investment in steel capacity is potentially significant enough to forestall the upper turning point in a boom, except under the most rarified and critically timed conditions, although outlays in steel might play an important role in a larger investment program.

Broude then examines the extent to which bottlenecks in steel could constrain growth during an upswing. He presents a model with a constrained cycle characterized by accelerator and multiplier feedbacks, similar to those developed by Hicks and Goodwin, with the process dependent upon discrepancies between desired and actual stock. Although this model is not empirically im-

plemented, it does demonstrate the theoretical possibility of the constraining effect that selective underinvestment, e.g., in steel, can exert upon the economy. While recognizing that overcapacity has also been a problem in the industry, Broude concludes that steel shortages have in fact at times limited the growth of the economy. This conclusion would have been strengthened if it could have been shown rigorously in a general equilibrium framework that shortages of steel were more crucial than shortages of other outputs.

Broude examines reasons for these shortages, beginning with the quality of demand projections for steel. He finds that within the steel industry, demand projections, which have generally been based upon time-trend projections, output per capita, and other simple relationships, consistently exhibit a strong downward bias. Government and academic economists, whether using similarly primitive or more sophisticated techniques, have generally arrived at projections of capacity requirements that were much higher and closer to the steel consumption which actually developed.

In order to explain this divergence between national requirements and steel industry projections of capacity needs, Broude makes a study of entrepreneurial behavior, attitudes, decision-making, and planning practices in the steel industry. Using a combination of approaches, including interviews, role perception, organization theory, content analysis, and case studies, Broude develops a concept of the "representative entrepreneur," whose attitudes and motivation influence the steel industry view of capacity requirements. This "representative entrepreneur" is a servant of management capitalism, who is not so much concerned with profit maximization, technological innovation, competition, and risk-taking as he is with his own prestige, power, job security, corporate continuity, protection of sunk capital, social approval, and public relations. Broude cites these factors, in conjunction with fear of idle capacity, as forces which have contributed to downward bias in industry projections of requirements for steel capacity.

More accurate projections of steel capacity requirements have been made by government and academic economists, using two major approaches. The first is regression analysis, which typically has not related steel output to needs of specific purchasers, but does have the advantage of reflecting the effects of major underlying trends, such as technological change.

The other approach, which Broude finds much more promising, is structural analysis of the Leontief interindustry type, which treats consumption of steel as a function of the output of all major steel-consuming sectors, and which explicitly relates changes in the size and composition of final demand to changes in consumption of steel. In a dynamic formulation of the interindustry model, he is able to account for interactions between output, investment, and changes in direct and indirect requirements for steel. The conceptual model is not empirically implemented, and Broude does not examine the predictive success or reliability of interindustry models to date. Nor does he discuss the operational problems that would arise in actual application of interindustry models to the problems of projecting steel capacity requirements. However, Broude expresses confidence that advances in the work of Leontief, Orcutt, Klein, Koopmans, and others, will provide techniques for making reli-

able projections of the steel capacity required to meet national objectives.

Broude examines a wide range of public policies which might be designed to assure sufficient investment in steel capacity. He rejects the usual proposals for public regulation or intervention, which he fears would unduly disrupt existing institutional structures. His major recommendation is for the establishment of a federal review board, whose primary function would be to project steel industry capacity requirements in light of the public interest, within guidelines suggested by the Employment Act and by national economic growth objectives. Broude concludes with the hope that participation on such a public board would impart to steel industry leaders a more affirmative outlook concerning U.S. economic growth and a greater willingness to build forward capacity. He also notes that the creation and maintenance of excess steel capacity should be accepted as one of the real costs of economic growth, and he recommends appropriate tax relief.

In focusing sharply upon one public policy question in the steel industry, i.e., capacity shortages, Broude omits examination of significant related factors, such as wages, prices, profits, and international competition. It is scarcely conceivable that a fact-finding board would be able to analyze capacity problems in isolation from these other fundamental issues. Assertion of the public interest in determination of these policies for the steel industry might well lead to similar intervention in other critical sectors of the economy. Thus the logic of Broude's analysis draws attention to a widening range of policy questions related to national economic performance, now left to the initiative of corporate management, in which there may emerge valid grounds for public participation.

If this should prove to be the case, Broude's own eclectic blend of methodologies from economics, sociology, and related disciplines very ably points out many of the problems and potentialities inherent in such an approach.

JOHN H. CUMBERLAND

*University of Maryland*

*The Transport Problem.* By C. D. FOSTER. London: Blackie & Son, 1963. Pp. xii, 354. 40s.

The tendency in the United States has been toward specialization in the analysis and the discussion of transportation policy. The problem typically is divided into a series of subproblems, e.g., competition for intercity freight between railroads, trucks, barges, and pipelines; competition for intercity passenger travel between the private automobile, air lines, buses and railroads; and competition for urban commuter travel between public transit and private autos. Mr. Foster, by contrast, includes in his rich and diverse treatment of Great Britain's transportation problems and policies all British domestic transportation, excepting air.

This difference in the analytical approach and empirical content between Foster's book and its counterparts in the United States is partially justified by differences in the distribution and density of population, differences in the distribution of freight and passenger traffic as between modes, and differences in the adequacy or extent of redundancy of capacity of the various modes.

There is obviously a much greater interdependence between inter- and intra-city freight and passenger traffic in Great Britain. British railroads still provide for relatively large amounts of commuter and intercity passenger traffic.

This broader and more general view of the transportation problem is more justified, and possibly even essential, in the British context. For that matter it might even be desirable for the analysis of some U.S. problems where conditions approximate those in Britain. But it is not accomplished without some losses. In particular, the analysis and discussions of policy lack some of the sharpness and detail that might have been obtained from the analysis of a narrower range of problems. While this does not necessarily detract from the book's usefulness, it may cause it to appear somewhat shallow at points to the U.S. transportation specialist.

The book "is primarily about Government policy, discussing the issues raised by the decisions the Government has to make, particularly in two capacities—through the Ministry of Transport for roads, and through the Ministry and the British Railways Board for railways" (p. 1). The first of its four sections evaluates and discusses a number of simple, and, in Foster's view, unacceptable, solutions to rail and highway problems; presents some alternative policies that might be followed by public enterprise; and introduces Foster's basis for an economic solution of the transport problem. The second and third sections discuss rail and road problems in some detail; and the final section is primarily a tour de force in support of "consumer surplus maximization," Foster's chosen solution to "the transport problem."

Most professional economists will like many of the book's policy recommendations. However, they certainly also will be dissatisfied with the rather cavalier abandonment of marginal cost pricing in favor of "consumer surplus maximization." The majority of economists, who would instinctively turn to marginal cost pricing as a pricing policy for public enterprises, will be less distressed by the attempt to develop an alternative investment criterion and pricing policy than by the manner in which marginal cost pricing is dismissed. "The damning objection to using it [marginal cost pricing] as the guiding principle for solution of the transport problem is that there would seem to be no chance of its political acceptance. An economist building his recommendation on this foundation would not be listened to" (pp. 29-30). This, the sole reason offered for choosing his "consumer surplus maximization" over marginal cost pricing, seems totally unsatisfactory. What is, and is not, politically acceptable is extremely transitory and much subject to judgment. Moreover, comparative advantage would argue for leaving such decisions to others, and to the politician in particular. It is the economist's responsibility to spell out in some detail the implications of alternative criteria and alternative policies, leaving the politician to convert these judgments into public policy. The book suffers from the author's effort to combine the roles of theoretical economist and policy maker. The book has much excellent analysis and would certainly have been improved if it had been segregated from the campaigning for "consumer surplus maximization," which Foster clearly sees, to a greater extent than seems necessary, as the cornerstone of his policy recommendations.

There are, moreover, theoretical difficulties with his "consumer surplus maximization." It is not always possible to be certain precisely what "consumer surplus maximization" consists of, but it clearly implies average, as opposed to marginal cost, pricing. Furthermore, "consumer surplus maximization" would at first appear to imply a zero rate of return, or profit, on invested capital. Efforts to provide precision on this point produce, on the whole, more confusion than enlightenment. For example, "a consumers' surplus policy is also compatible with the retention of profits to form reserves" (p. 12). And, "a consumers' surplus policy is compatible with requiring an enterprise to pay the interest costs of the capital it uses" (p. 122). Summing these and various other profits which a "consumer surplus maximizing" enterprise might be permitted, or required, to earn, it appears that, in fact, the rate of return might have to be sizable, and conceivably might even exceed that for a private firm.

There are, moreover, additional theoretical difficulties. Foster states that the objective of the firm should be to maximize the consumer surplus of the consumers of the firm's product. In the case of railways, for example, he states, "we will define our consumers' surplus policy as one in the interest of consumers of railway services, that is, simply railway-users" (p. 117). However, it is not clear whether this includes only current consumers, given existing prices of railway services and of all other goods; all potential consumers at prices dictated by consumer surplus maximization; future consumers; or several other groups of actual or potential consumers.

In general, the book's treatment of the rail problem is vastly superior to its treatment of the road problem, although it is hard to explain the author's failure to bring to bear on his discussion of railway costs the very considerable amount of statistical costing carried out for railroads in the United States and Canada. The discussion of the roadway problem will seem fairly elementary to U.S. economists. Nearly all of Chapter 11, for example, is devoted to a fairly elementary description of a cost-benefit analysis of the London-Birmingham motorway. However much English readers benefit from this presentation, it will seem dated to the U.S. reader.

Despite these reservations, the book deserves the attention of all those concerned with transportation problems since it is a valuable contribution to the growing literature dealing with the economics of transportation. Although much of the policy discourse concerns English problems, and its greatest potential contribution is certainly to the growing debate about Great Britain's transportation policy, U.S. economists, and transportation planners and policy makers everywhere, will certainly benefit from reading the book. While readers may not entirely agree with the author's policy prescriptions, its worth by no means depends on either the correctness of the proposed solutions or their adoption.

JOHN F. KAIN

*United States Air Force Academy*

*Modern Transportation Economics.* By HUGH S. NORTON. Columbus: Charles E. Merrill Books, Inc., 1963. Pp. 463. \$8.00.

Any author approaching the task of writing a usable college textbook in as

broad and diverse a field as transportation faces a number of difficult decisions. He must decide, for instance, how descriptive he intends to be, how much discussion of current policy issues and problems he will include, how much history and development of regulatory institutions and the field itself is desirable, and how much application of current managerial practices is needed for complete understanding. No doubt the compromise solutions to these problems contained in Professor Hugh Norton's text will not satisfy all persons in the field. However, by and large, Norton has produced a readable and teachable text which makes a definite contribution to the literature in the field even though it has several weaknesses.

Norton continues the laudable trend toward shorter books. He states at the outset that the text will not be an exhaustive reference book, and it is not. This book leaves the instructor a good bit of latitude to cover material in lectures and outside readings and to use other supplementary devices. Such an approach is desirable if the text itself provides a good foundation upon which the student may build. This is the case with this text.

Norton also continues the desirable trend of using an approach which integrates the various modes of transportation (rail, motor, air, water, pipelines) into the discussion rather than treating each separately. He does this job exceptionally well even though separate chapters are included on the regulation of railroads, regulation of motor carriers, regulation of air carriers, etc., as well as separate descriptive chapters on each mode.

The attempt to integrate economic theory (both micro and macro) into all parts of the discussion of transportation is somewhat less successful, although Norton does go a long way toward this stated goal. Use of theory considerations, graphs, and repeated references to theoretical works are scattered throughout. Yet, in total, one wonders if perhaps the task Norton sets himself is too great considering the limitations of length, or perhaps the integration of economics and transportation is an impossible goal.

Norton makes very good use of the technique of including a "summary" at the end of each chapter and has a number of lists of "Selective References for Further Study." Unfortunately, the listing of references for further reading is not carried out uniformly and at times is too limited.

The desire to keep things short shows up most noticeably in Norton's tendency to list in tabular form and his disarming propensity to write some chapters almost completely in a slightly expanded outline form. While the "skeleton" is there for the instructor to "flesh out," the student may at times skip over many important and pivotal points or fail to recognize or appreciate that all points in the list or outline should not be equally weighted.

Of the five parts of the book, Part I, "The Industry and the Modes: An Aggregate View," is by far the strongest and the best. Norton has provided an excellent overview or survey here and has good balance in a well-written and surprisingly brief description. The last part, Part V, "Problems and Prospects," is also very well written and is almost as strong as Part I. While the three chapters in this part are somewhat disconnected ("Carrier Management Problems," "Problems and Policies of Regulation," "Review and Policy Consideration"), Norton comes through here as an individual and scholar more

than any other place in the text. A number of provocative questions are raised, analyzed, and discussed meaningfully. The only weakness here is that the student may not have adequate background from the other parts of the book to fully use and appreciate Norton's excellent policy discussion or realize the full import of the points analyzed.

Part II, "The Economics of the Industry," and Part III, "The Basic Framework of Public Control," are the weakest sections of the text. Part II, too brief and segmented, covers transportation cost, demand, pricing, and rates too lightly and with too little analysis. Such important aspects of transportation deserve better treatment. Elasticity of demand is weakly considered, the classification system and various pricing systems (rate structures) are not well developed. Some applications, illustrations, and development of economic relationships would help these chapters. Perhaps the strongest chapter in Part II is the brief discussion of resource allocation. Part III is mixed. The discussion of the background of regulation tends to be too brief, and the use of the outline form may rob the student of a meaningful understanding of the institution of control. Here the instructor will certainly wish to lecture and supplement Norton. Finally, the chapter on administrative agencies and policy making shows Norton's excellent grasp of the subject, but unfortunately brings up solutions to policy problems not yet posed at this point in the text. Some instructors may wish to delay the students' reading of this chapter until the problems are better discussed in Part IV.

Part IV, "The Application of Public Controls," contains some excellent chapters (Chapter 17 on fair value and rate level, and Chapter 20 on control of competition are especially good) as well as some chapters which again illustrate the author's tendency to list, outline, and be overly brief. (Chapter 13, "Regulation of Railroads," Chapter 14, "Regulation of Motor Carriers," and Chapter 19, "Control Over Rates," illustrate this approach.) The remaining chapters, covering regulation of air carriers; regulation of water carriers, pipelines, and indirect carriers; discrimination; and consolidation and integration are very adequate and keep within the stated objectives of the text.

On balance, Norton's work is a contribution to the field that will be welcomed by many instructors, especially those well versed in this area and able to use supplemental materials. As a whole, the weaknesses are more than offset by the strengths, and a readable and teachable text is the result.

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### **Land Economics; Agricultural Economics; Economic Geography; Housing**

*Natural Resources and International Development.* Edited by MARION CLAWSON. Baltimore: Johns Hopkins Press for Resources for the Future, 1964. Pp. xi, 462. \$7.50.

Students of the problems of economic development must be grateful to Resources for the Future for these essays. They maintain a remarkably high standard for a volume of "commissioned" conference papers on a subject

which seems to call forth a disproportionate number of repetitious generalities about what is wrong, together with vacuous prescriptions about what ought to be done about it. At least four are of outstanding interest: M. A. Adelman on oil, for originality; Anthony Scott on fisheries, for concise and imaginative treatment; Arthur Gaitskell on Africa, for shrewd wisdom, compassionate understanding, and English style; and Chandler Morse on private foreign investment, for a balanced and comprehensive discussion of a difficult subject.

The ten papers are grouped under three headings. The first, "Commodities of International Significance," contains papers on agriculture, oil, and fisheries. Perhaps the most disappointing of the papers is that by D. Gale Johnson on "The Role of Agriculture in Economic Development." His primary conclusion is that increasing productivity in agriculture is essential for economic development—a conclusion that hardly startles us. Johnson does not deal with agriculture in general, but only with food production, and he shows that the demand for food has grown at different rates in the industrial and nonindustrial countries and in different areas of the world, that the poor countries are faced with a rapid rate of growth in demand, and that the policies of the rich countries are detrimental to the development of the poor countries. Most of the discussion is general, relying heavily on "aggregate" data covering large areas of the world without reference to the doubtful quality of agricultural statistics in undeveloped countries or to the great differences between countries in the same region, and is written in a style clumsy, at times unclear, and in places even ungrammatical.

Adelman's paper on "The World Oil Outlook," by contrast, is a remarkable tour de force, challenging and effectively demolishing the almost universally held view that oil production is inherently subject to long-run "decreasing cost," and presenting in a workmanlike, lively, and witty style, the best analysis I have ever seen of the basic economics of the industry. He shows that, subject to random disturbances (an important condition), output can be expected to adjust to price changes through the usual mechanism of rising marginal cost acting against marginal revenue, although he recognizes that the adjustment may at times be difficult and painful and may thus be socially and politically unacceptable, especially in the presence of much excess capacity. This is a first-class piece of work which is not only an important contribution to the study of the international petroleum industry, but also an outstanding model of how economic theory should be applied.

Anthony Scott in "Food and the World Fisheries Situation" discusses the reserves and productivity of the oceans, the economics of fisheries, and various forms of international cooperation in their exploitation. I find it extraordinary how little we seem to know about wild fish, and Scott explores, briefly but imaginatively, the possibilities of man increasing this source of food through knowledge and appropriate management. The paper is but a prelude to a longer forthcoming study, which should be of great interest.

The next general section of the book deals with "Resources in Europe, Africa, and Latin America" and contains essays on the Soviet Union (Dimitri B. Shimkin), Europe (P. Lamartine Yates), Africa (Arthur Gaitskell), and Latin America (Joseph Grunwald). Shimkin gives a valuable and extensive

statistical survey of resources and their use in the Soviet Union and adds some remarks about Soviet allocation procedures. Grunwald analyzes some of the special problems created for developing countries by foreign exploitation of their raw materials and describes, again statistically, some aspects of the importance of natural resources for the development of the Latin American countries. Yates discusses the manpower, agriculture, forest lands, and energy of Western Europe and shows how very rich the continent is in these resources, although it will have to depend increasingly on imports of energy to meet its growing demand. The efforts of the Common Market countries to obtain self-sufficiency in agriculture are described, but the significance of European agricultural autarky for third countries, and especially for the underdeveloped countries, is not analyzed. Yates merely asserts that the improved economic strength of a "united and rich Europe" will in the end benefit the rest of the world. He seems to be little disturbed by strictures contained in some of the other essays about the adverse effect of protectionism in the rich countries on both the rich and the poor countries.

The most outstanding essay in this section is that of Arthur Gaitskell, "Resource Development Among African Countries," which, I again suggest, should be required reading for students of economic development. Mr. Gaitskell does not attempt to catalogue the resources of Africa but, instead, describes the nature of the environment and the changing position of man in the area as the result of political changes and the rise of the independent African states, primarily in "British" Africa. His discussion of African attitudes towards development and the ways in which the Western world can fit in with these attitudes, if it so desires, is marked by a shrewd understanding of both worlds.

The third section of the book, lamentably labeled "Problems, Situations, Processes," contains three papers. Charles Kindleberger gives us a lively, brisk, and down-to-earth essay on the "Terms of Trade for Primary Products." After an admirably succinct review of the various arguments about and statistical analyses of the subject, he turns to discuss the future in the light of the different policies recommended or adopted by various governments and organizations. Kindleberger frankly recognizes the conflict that has appeared between an acceptable distribution of income and an appropriate allocation of resources: "The fact is that the world does not know how to stabilize commodity prices in ways which will produce the appropriate distribution of income without messing up the allocation of resources in production" (p. 361). Discussing somewhat flippantly (and indeed cynically) the various propositions designed to evade the dilemma, he ends with his own prescription: "not much primary-product price stabilization, . . . and all of that temporary, and accompanied by foreign aid for the purpose of alleviating dependence on limited specialization" (p. 362).

In the second paper, on "Potentials and Hazards of Direct International Investment in Raw Materials," Chandler Morse has produced a masterly and well-balanced discussion of this intricate issue, one of the chief merits of which is its open recognition of the extent to which private investment is not an unmixed blessing for the undeveloped countries. His careful discus-

sion of differences in legal systems and social attitudes between the Western world and the undeveloped countries goes to the root of many problems and is a refreshing change from the carping and superficial criticism which characterizes the attitudes of many Western businessmen and economists. His review of the literature, mostly in footnotes, is remarkably complete, and the essay as a whole will provide a valuable and balanced introduction for students. Only occasionally is his clear and straightforward style marred by the use of jargon which can be justified neither by the technical nature of the discussion nor by the demands of precision. It is not excusable in an essay of this type to describe the role of society's political institutions as the control of "The parameters of private choice functions," or to talk about "efficiency paths defined by the parameters of a situation" (cf. p. 409).

The concluding brief essay by Egbert de Vries on "International Transfers of Knowledge and Capital" is somewhat scrappy but it stresses one important point often overlooked—the danger of losing information either because reports disappear in private or governmental files or because so much is drowned in the vast seas of material. He urges the importance of establishing international procedures for retrieving information.

The book is well indexed; some of the essays have extensive statistical appendixes, and many contain much useful bibliographical material.

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*Location and Land Use—Toward a General Theory of Land Rent.* By WILLIAM ALONSO. Cambridge: Harvard University Press, 1964. Pp. xiv, 204. \$5.50.

The bulk of this ingenious book is devoted to the construction of a model of the equilibrium pattern of land use and land values in a city. This city is in a featureless plain, and proximity to its center is assumed to be attractive to all land-users. The central tool of analysis is the bid price curve, a line joining those combinations of land price and distance from the city center between which the land-user is indifferent.

Families of bid price curves are rigorously derived by price-theory type of analysis for each of three types of land use: agriculture, urban firms, and residential. In the last case, for instance, the individual's utility is assumed to be an increasing function of the size of his residential lot and of his consumption of other things, and a decreasing function of commuting distance. He maximizes utility subject to a budget constraint upon the sum of his expenditures upon his lot, upon other things, and upon commuting (whose cost is an increasing function of distance from the city center). Given his income, his commuting-cost function, and the price of other things, he will have one single-valued bid price for each level of utility. In the same way, farmers will have a bid price curve for each agricultural product price level, and an urban firm will have a bid price curve for each level of profits. These curves all show bid price falling as distance from the center increases; and they take into account the optimization of the size of lot for each price and location.

The next step is to analyze the equilibrium constellation of lot sizes, loca-

tions, and land prices, given the number of residents, the agricultural price level, and the number of urban firms. This is done in Chapter 5, where it is shown that, given well-behaved bid price curves, "in general, the steeper is his bid price curve, the nearer the center of the city will a user of land locate." Another way of putting this would be to say that central locations are occupied by those who put the highest premium on proximity to the center.

The analysis summarized so briefly here takes up a good deal of space. To say this is not to complain, however, for Dr. William Alonso has set it out both algebraically and geometrically and has provided intuitive explanations of his results. That the harvest is relatively meager for such a large effort is disappointing but not surprising, since formal rigor in economic theory often requires a great deal of elaborate technique.

Alonso regards his theory as relating to the long run, i.e., as concerning the equilibrium pattern of land use. Thus the occupant of one floor of an  $n$ -story building can be regarded as occupying a site whose size is one  $n$ th of that of the building. A complete analysis would therefore have to include consideration of the costs of building high and of preferences between ground-floor and upper-floor location. Since these factors are left out of the analysis, Alonso's theory could equally well be regarded as relating to the short run, explaining the equilibrium allocation of a given amount of floor space in a stock of existing buildings.

For those who do not enjoy technique for its own sake, the main interest of the book will be in its final chapter: "Some Applications of the Model and an Outline of Empirical Research." It starts with a convincing refutation of the Haig notion that the equilibrium pattern of land uses and land values minimizes costs of friction. It is interesting to note that the argument here hardly rests upon the model developed in the first part of the book, and that when the model is used in subsequent sections, its consistency with observed phenomena (e.g., peripheral location of higher-income residents) is not necessary: it would also be consistent with an alternative set of observations. The model, that is to say, is not a strong one, and it requires numerical values of some of the parameters to make it so. For this reason particular interest attaches to an empirical section where a regression with Philadelphia data shows that family expenditure on land is an increasing function of income and a decreasing function of distance from the center. On a theoretical plane there are also admirable sections on the effects of density zoning (though without any reference to external diseconomies) and on city shapes.

This brief mention of the topics discussed in the final chapter should suffice to show how stimulating it is. All that remains to be said is that, in a branch of economics too often characterized by purely discursive treatment, Alonso's book stands out for its rigor and clarity.

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*Land Use Policy and Problems in the United States.* Edited by HOWARD W. OTTOSON. Lincoln: University of Nebraska Press, 1963. Pp. x, 470. \$6.00.

What significance for today's analysts can be found in the Homestead Act?

One answer is that the Act provides experience in adapting policies to problems arising from economic change. Concern with the adaptation of land policies to change is the main connection between the first third of this book, which looks back, and the last two-thirds, which is more current. Presented at a conference on the occasion of the hundredth anniversary of the Homestead Act, the contributions are primarily from economists experienced in analyses of land resources and institutions. Historians, political scientists, and a geographer are also represented.

The policies that finally culminated in the Homestead Act are viewed by some of the authors as retarding economic development and as political schemes enabling private citizens to obtain capital gains that might better have gone to government. Others defend the Homestead Act. As formalizer of the squatting method for public domain to pass into private ownership, the Act is seen to have speeded economic development by lowering the cost of settling and by making land use a condition of initial ownership. According to their views, the Act reduced the holding of idle large tracts for speculation and helped to distribute any private speculative gains to the less wealthy. Commendably, evidence is offered that throws light on whether the policies had these and other effects. Especially since some effects were favorable to economic development and some unfavorable, one hopes that future analysts will estimate the magnitude instead of only the sign of the effects. The essays mention numerous effects that deserve such estimation.

Thomas Jefferson is an absent and sometimes controversial contributor in the sense of receiving much mention. In a chapter on the present relevance of the Jeffersonian ideal, John M. Brewster concludes that the Jefferson ideal, interpreted as a family-farm goal, is still desirable and will be tenable until more knowledge about photosynthesis and animal feeding results in large-factory farming. W. B. Back believes that the family farm is only a powerful myth and that it cannot be defined usefully in a modern setting. One senses a differing political philosophy in the background of this disagreement that is more apparent than real. Both authors suggest that any fostering of family-farm goals will be successful and desirable only if consistent with income goals.

On another recurring theme, Brewster and Back and many of the other contributors agree: Modifications in ways of acquiring control over assets will be needed as the amount of resources used by a single farm continues to increase. Many years ago the old ideal of debt-free owner managership became obsolete, as profitable farm sizes grew so large as to be achievable only by borrowing or renting. With further growth in value of assets needed by a farm manager, there is now talk of modifying the old ideal more sharply by separating managership from farm firm equity through corporate forms.

Researchers might usefully carry further by estimating more specifically the future seriousness of this balance-sheet problem. One can point out that in many places in the South large land holdings have been kept intact between generations. A run of a few good years of income can enhance family wealth, with the result that a potential manager may inherit sufficient equity

to start a good farm even if he is from a large farm family. Relevant to recent years, sufficient inheritance can be made possible by accrual to families of capital gains on land. There might, too, be more concern about the incongruity of almost unconsciously assuming it is desirable to ease entry into farming when there are still surplus problems. Finally, a balance-sheet consideration that could be at least as important as the one so heavily emphasized is the vulnerability of present farm owners to adverse runs in income, such as could be brought on by weather or low prices, or asset-value losses that might accompany an adjustment in the rate of capitalization in the economy as a whole.

One section of the book that may be particularly valuable to nonland economists competently summarizes knowledge about demands on land by major categories of use: Harry A. Steele and Norman E. Landgren on agriculture, Raleigh Barlowe on recreation, and Lowdon Wingo on urban land.

Several other themes recur in sections widely separated from one another. Marion Clawson's general consideration of policy contemplates the changing interaction between interest groups, the public at large and professional land managers, and sees an increasingly influential role for the last. Ross Talbot's is almost a companion piece, providing a primer on politics of land and on the many government policies concerned with land. The Clawson and Talbot pieces, much more strongly together than separately, convey a picture of small rather than great changes underway in the over-all land-policy setting at the present time.

M. M. Kelso, Marshall Harris, and Gene Wunderlich, bringing in interdisciplinary considerations, explore, each in a different manner, criteria for land policy. Policy issues for Asia, Europe, and Latin America are analyzed by Walter Neale, Philip Raup, and Raymond Penn and Kris Kristjanson. They complete a circle providing an implicit reminder that present policy needs of other countries are as different from those of the United States as present needs are in this country from needs at the time of the Homestead Act.

To review this book is almost to review current land economics because it reflects well a large part of activity in the field. Strong points include rising theoretical competence, concern with looking ahead, attention to the right variables for analyzing policy, and operational contributions to administration and legislation. Some of these very features are suggestive of current needs to use more intensively the acquired economic theory in empirical analyses and, given recognition of the variables required for an adequate policy framework, to develop further the analytical context within which to use the variables. Four possibilities for making progress along these lines may be mentioned. First, there could be more estimates of magnitude of effects of alternative policies. Many needs exist in addition to those mentioned earlier for estimating output effects and regional, sectoral, and personal income distribution effects of present and contemplated programs. Second, drawing on the lesson of the impact that benefit-cost analysis has had, can we develop measures of the contribution of land-reform policies to economic development? In view of the recent attention to land reform, efforts now to devise

such measures might be more fruitful than they would have been several years ago. Third, while relevant goals have been recognized, there is still need to develop policy criteria. The criteria would aid in trading off between different goals, taking account of policy-making processes. Fourth, it would be desirable to encompass predictions of policy as well as predictions of economic events. Several of the contributions to this volume contain insights about probable future policies, indicating the feasibility of pursuing such analyses formally. These possibilities are mentioned to suggest, not that a different book should have been written, but that this book brings to mind a desirable sequel building on the progress it exemplifies.

G. S. TOLLEY

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*Dynamics of Adjustment in the Broiler Industry.* By B. F. TOBIN AND H. B. ARTHUR. Boston: Division of Research, Graduate School of Business, Harvard University, 1964. Pp. xii, 164. Paper, \$3.00.

This study deals with the growth of the modern broiler industry, a leading example of an "agri-business." Despite the growth of industrial volume, coupled with continued horizontal and vertical integration, the industry has been plagued by price instability and declining profit margins.

In the description of the technical production process, the authors demonstrate that they are able to observe the industry clearly and coldly without taking sides.

In the second chapter, the authors make a key point which is only partially understood by the industry—"markets are integrators just as truly as are contracts or mergers" (p. 41). In the third chapter, the authors try to show that the relationship of supply to demand is complicated by the several decision centers affecting production. These decision centers are not always correctly attuned to the final consumer demand on which, in their opinion, production should be based. In other words, a temporary supply shortage brings an overreaction by decision centers. Thus firms controlling the basic breeding stock may put too many units into production with resultant severe supply pressures 18 months later. The authors point out that the lack of effective coordination in the industry is a basically unsettling factor and that the "very forces leading to vertical integration were also unstabilizing." Basic production facilities were duplicated, leading to possible overcapacity.

In Chapter 4, the authors develop their major thesis that the decision centers controlling production must move toward the final point of sale. The developing industry has had to recognize the final market rather than falling production cost. One of the study's most interesting criticisms deals with how cuts in bulk grain freight rates have stimulated southeastern production without regard to the total consumer demand for finished broiler meat.

As a possible solution to the continuing price instability plaguing the broiler industry, the authors suggest three possibilities.

1. Consumer acceptance of frozen broilers with resulting ability to stockpile in periods of temporary glut. (However, the source of the additional working capital to finance the inventories is not mentioned.)

2. The use of demand marketing which would better schedule current production in terms of future markets.

3. Government control of supplies and industry acceptance of the fact that competition will not create equilibrium.

The authors believe that continued mergers and consolidations will eventually prove to be the final solution. This will be a "rational and not an exploitative process." It is interesting to note that the authors emphasize the economies of scale and better management as the end result of industrial "rationalization." They do not emphasize the factor that shifting from a purely competitive model to a monopolistic model may offset to some degree the oligopsonistic power of the relatively few large retail chains.

While human decisions are the key to possible solutions they are also the possible basis for continued instability. As the study shows, only a minority of the industry has to act irrationally to upset the demand-supply relationship.

In the final analysis, the industry resembles Schumpeter's concept of the role of innovation. Only a few businessmen have the imaginative power and energy to initiate successful new ways of doing old things. But while only a few can lead, many can follow. The crowd of imitators prevents the industry from settling down to a more profitable equilibrium point. Many theoretical questions are left untouched, but as a basic industry study, this work is admirable and highly recommended.

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### Labor Economics

*Manpower in Economic Growth: The American Record Since 1800.* By

STANLEY LEBERGOTT. New York: McGraw-Hill, 1964. Pp. xii, 561. \$8.95.

This is *not* an economic handbook, despite inclusion in Seymour Harris' distinguished "Economics Handbook" series. Only a sadist would inflict it on students as a text. Neither is it a labor history, in the conventional sense of John R. Commons, his Wisconsin disciples, or their Socialist opponents. Neither "trade unionism" nor "labor movement" appears in its 10-page index; the single reference to "strikes" refers to 1835-36; "Lewis" is the Chicago economist H. Gregg, not the U.M.W. hero John L.; and so on.

Well then, what manner of witches' brew has Professor Lebergott served up for us? Primarily, he has done for U.S. labor force, employment, unemployment, and wage statistics for the period 1800-1960 the kind of job which Simon Kuznets and Raymond Goldsmith have done for U.S. national income, wealth, and capital statistics over a period roughly half as long. Nor is it only in the size of historical canvas that Lebergott at times out-Kuznetses Kuznets and out-Goldsmiths Goldsmith, but in two other features of his work as well. One is the tender loving care of his literary style. It really matters, for example, not only to Lebergott but to his readers, that faulty census-enumerator instructions led in 1910 to overestimates of the labor force and employment as compared to censuses prior and subsequent; and it matters, too,

what adjustments had best be made to "the facts" as reported, to make the 1910 data comparable with the rest. An allied feature of Lebergott's presentation is humbleness. Any reader, even the most casual, can see the chewing-gum and baling-wire with which the estimates were put together (unless of course he limits himself to copying figures) and the enormous variations between different estimates of the same entity for the same period, particularly if that entity is unemployment.

This reviewer must confess to being more impressed with Lebergott's one-man National Bureau set of tables, broken down by age, sex, race, region, and/or occupation, and with his labor of choice among alternative estimates (plus defenses of the choices made) than with the resultant economic history which forms Part I, "Sources and Consequences of Growth." Two major factors, copiously documented but largely independent of the detailed statistics which follow, are, (1) the pressure for labor-saving invention and innovation, brought on less by the height of U.S. wages than by the expectation of their long-term increase, and (2) a steady decline of the "uncertainty" factor in both labor and capital costs, as markets, law, and order were extended across the continent. It is largely by these that Lebergott explains the simultaneous growth in the labor force and in income per worker, to the relative neglect of enlarged educational and training opportunities, which Edward Denison regards as paramount in his study of *The Sources of Economic Growth in the United States* (Washington: Committee for Economic Development, 1962). It is regrettable that Lebergott provides no direct confrontation of his surmises with Denison's. This reviewer's guess, which may reflect primarily the occupational bias of the professional educator, is that Denison is more nearly right than Lebergott, but crucial experiments distinguishing between the rival theories have yet to be undertaken. (Dissertation hopefuls please note.)

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*Labor Markets, Unions and Government Policies.* By EVERETT JOHNSON BURTT, JR. New York: St. Martin's Press, 1963. Pp. xix, 454. \$8.75.

There are two traditions of textbook writing in the labor field. One tradition is embodied in those texts which seemingly encompass anything that could be taught under the rubric of labor. Their contents range from labor market analysis to trade union history and institutional practice; labor market and social welfare policy to industrial relations processes and procedures. The other emphasizes the economics of the labor market and analysis of the relationship of manpower problems, policies, and institutions to basic socio-economic phenomena.

Happily, this work is in the latter stream of thought. It attempts, though not always successfully, to utilize economic analysis as a tool for understanding labor and job markets in a world of trade unionism, collective bargaining, and governmental welfare and employment policy. Further, it reflects the substantial changes in emphasis which have been occurring in labor economics as a result of recent attention to key manpower problems and to the economics of the utilization of the human resource.

Everett Burt avoids that encyclopedic descriptive type of emphasis that induces many economists and analytically oriented undergraduates to feel that the field is either devoid of genuine analytical content or is unrelated to the central core of economic theory. Instead, stress is placed upon the essentials of our discipline, including labor market theory and the analytical basis for present day labor market, social welfare, and manpower policies. This textbook should assist the student in understanding how the economist defines labor problems and how he uses his tools to try to solve them. From the student's or worm's-eye point of view, it is eminently satisfactory because it is readable and interest evoking.

The book can be divided into five sections: (1) The Labor Market, Its Dynamics and Structure, (2) The History, Structure and Nature of American Trade Unions, (3) Collective Bargaining, Objectives and Policies, (4) Wage Theory, Wage Structure and Union Impact upon Wages, and (5) Economic Security and Governmental Policies. If one agrees with the author's premise that the union movement is an outgrowth of the problems facing the worker as a supplier of labor, one should not then quarrel with his sequential development, e.g., introducing labor market analysis before union history, collective bargaining, wage theory, and current governmental labor policy. But it would be missing the obvious to deny some compensating disadvantages and awkwardness stemming from this approach. For instance, since the chapters on labor markets and union wage objectives in collective bargaining are presented before the chapters on wage theories and wage structure, the student may find it difficult to perceive either the effect of union wage policy upon allocation of the labor supply or the validity of the stated separation between the wage and job markets.

The most meritorious features of this work result from the author's ability to differentiate between the important and less important and to omit the latter. Much of the recent research in labor economics has been blended into the analysis. Additionally, the work covers some basic but often neglected topics such as equalizing differentials, union and governmental effects upon wage rates, and the theory of the labor movement. Burt is not merely content to describe such subjects as the internal wage structure, work rules, and wage differentials. Instead, he consistently attempts to apply the analytical tools previously developed in the work to understanding these phenomena. Full employment, economic security, and labor market policies of the federal government are generally well described and analyzed. Considerable ability to utilize effectively recent research is displayed in the analysis of minimum-wage legislation. The discussion of work versus leisure and the short-run labor-supply curve shows insight into both the theoretical and applied literature and is one of the more satisfactory features of this work. Another excellent discussion is on contract regulations and the economic basis for bargaining power. In general the author has successfully avoided the pitfalls of many labor and industrial relations texts, namely of confusing description with analysis.

Other sections, however, are less well executed, and even the implementation of many of the unique features of this text leaves much to be desired.

This reviewer must report much more satisfaction with coverage, emphasis, and design, than with execution.

Burt's discussion of equalizing differences is confusing and fails to cover the major causes for their existence. His analysis of theories of the labor movement is oversimplified and does not provide any rationale for understanding either the theories or how they can be related to the specific growth process of American trade unionism. The author fails to relate union attitudes and work rules, many of which superficially seem nonrational, to the economic problems arising during organized labor's history and growth. The discussion of corruption and racketeering fails to focus upon relevant questions: what, for example, are the work environments and labor market situations in which invidious practices are likely to appear? The chapter on "Theories of Wages" is, at best, too episodic and, at worst, fails to develop insights into the nature of the fundamental forces impinging upon both the aggregative wage level and wages in different occupations.

Despite these and other defects that stem from failure to make some necessary qualifications and the use of value-laden terminology, the text meets the test as a satisfactory teaching instrument. It asks most of the right questions, and its emphasis upon how the labor economist answers them rather than upon the answers themselves is a proper one. In his avoidance of that defect characteristic of the more encyclopedic texts of "being all things to all men," Burt manifests some substantial ability to vitalize the subject matter and stimulate the student.

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### Related Disciplines

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# NOTES

## SEVENTY-SEVENTH ANNUAL MEETING OF THE AMERICAN ECONOMIC ASSOCIATION

Conrad Hilton Hotel, Chicago, Illinois—December 28-30, 1964

### *Preliminary Announcement of the Program*

*Monday, December 28, 1964*

8:30 A.M. ECONOMIC HISTORY: ITS CONTRIBUTION TO ECONOMIC EDUCATION, RESEARCH,  
AND POLICY—A PANEL DISCUSSION

*Chairman:* WILLIAM N. PARKER, Yale University

*Papers:* A "New" Economic History for the United States?

DOUGLASS C. NORTH, University of Washington

ROBERT W. FOGEL, University of Chicago

Comment: MOSES ABRAMOVITZ, Stanford University

Has the Early History of Developed Countries any Relevance?

BARRY SUPPLE, University of Sussex and Harvard University

RICHARD A. EASTERLIN, University of Pennsylvania

Comment: EVSEY DOMAR, Massachusetts Institute of Technology

Has Economic History a Role in an Economist's Education?

HENRY ROZOVSKY, University of California

RONDO E. CAMERON, University of Wisconsin

Comment: R. A. GORDON, University of California

### NON-MARKET DECISION MAKING

*Chairman:* JAMES BUCHANAN, University of Virginia

*Papers:* Bureaucracy Considered as a Self-Interest System

ANTHONY DOWNS, The RAND Corporation

Bargaining and Conflict Situations in the Light of a New Approach  
to Game Theory

JOHN HARSANYI, Wayne State University

Barriers to Entry in Politics

GORDON TULLOCK, University of Virginia

*Discussants:* JAMES MARCH, University of California, Irvine

JESSE MARKHAM, Princeton University

To be announced

10:30 A.M. SURVEY RESEARCH: THREE SURVEYS; FINDINGS AND IMPLICATIONS FOR THEORY  
AND POLICY

*Chairman:* DAN TEROOP SMITH, Harvard University

*Papers:* Some Results of a National Survey of Financial Asset Holdings

DOROTHY PROJECTOR and GERTRUDE WEISS, Board of Governors,  
Federal Reserve System

Some Results of a National Survey of High-Income People's  
Asset Management

JAMES MORGAN, HARVEY BRAZER, and ROBIN BARLOW, Survey  
Research Center, University of Michigan

Some results of a National Survey of Attitudes Toward Liquidity

EVA MUELLER, Survey Research Center

HARLOW OSBORNE, Federal Reserve Board

*Discussants:* JOHN CULBERTSON, University of Wisconsin  
 GEORGE BREAK, University of California  
 LAWRENCE E. THOMPSON, Harvard University

#### RECENT CAPITAL AND PRODUCTION THEORY

*Chairman:* HOWARD SCHALLER, Indiana University

*Papers:* Recent Developments in Capital Theory

ABBA P. LERNER, Michigan State University

Factor Substitution and Technological Progress in the Neo-Classical Theory of Production

C. E. FERGUSON, Duke University

*Discussants:* R. W. PFOUTS, University of North Carolina

MARVIN FRANKEL, University of Illinois

ROBERT SOLOW, Massachusetts Institute of Technology

#### BALANCE OF PAYMENT AND RELATED PROBLEMS (Joint Session with the American Statistical Association)

*Chairman:* E. M. BERNSTEIN

Full slate will be supplied by A.S.A. Program Chairman

12:30 P.M. ANNUAL FORECAST LUNCHEON (Sponsored jointly by the American Economic Association, the American Finance Association, and the American Statistical Association)

*Chairman:* MARTIN GAINSBROUGH

*Speaker:* HENRY WALLICH

2:00 P.M. DEFENSE ECONOMICS: APPLYING ECONOMIC CRITERIA

*Chairman:* DAVID NOVICK, The RAND Corporation

*Papers:* Using Costs to Select Forces

STEPHEN ENKE, Institute for Defense Analyses

Estimating Military Costs

ROBERT N. GROSSE, Research Analysis Corporation

*Discussants:* JAMES R. SCHLESINGER, The RAND Corporation

WILLIAM H. MECKLING, Center for Naval Analyses

ROLF PIEKARZ, Institute for Defense Analyses

#### THE CONTRIBUTION OF THE HISTORY OF ECONOMIC THOUGHT TO THE UNDERSTANDING OF ECONOMIC THEORY, ECONOMIC HISTORY, AND THE HISTORY OF ECONOMIC POLICY

*Chairman:* ALFRED F. CHALK, Texas A & M University

*Papers:* The Relation of the History of Economic Thought to the Study of Modern Economic Theory

DONALD F. GORDON, University of Washington

The Relation of the History of Economic Thought to the History of Economic Policy

WILLIAM D. GRAMPP, University of Illinois at Chicago

The Relation of the History of Economic Thought to Economic History

FRANK W. FETTER, Northwestern University

*Discussants:* WILLIAM R. ALLEN, University of California, Los Angeles

WARREN J. SAMUELS, University of Miami

WARREN SCOVILLE, University of California, Los Angeles

8:00 P.M. RICHARD T. ELY LECTURE

*Invited Lecture:* A Review of Economic Development

W. ARTHUR LEWIS, Princeton University

*Tuesday, December 29, 1964*

8:30 A.M. THE EVOLVING INTERNATIONAL MONETARY MECHANISM

## THE AMERICAN ECONOMIC REVIEW

*Papers:* The Report of the Group of Ten

J. DEWEY DAANE, Member, Board of Governors, Federal Reserve System

The Report of the International Monetary Fund

JACQUES J. POLAK, Director, Research and Statistics Department, the International Monetary Fund

The Report of the Non-Government Economists Study Group

FRITZ MACHLUP, Princeton University

*Discussants:* MILTON FRIEDMAN, University of Chicago

E. M. BERNSTEIN, Washington, D.C.

To be announced

AGRICULTURAL STRUCTURE AND PRODUCTIVITY (Sponsored jointly by the American Economic Association and the American Farm Economic Association)

*Chairman:*

*Papers:* The Structure and Productivity of Brazilian Agriculture

WILLIAM H. NICHOLLS, Vanderbilt University

RUI MILLER PAIVA, Fundação Getúlio

Agriculture and Economic Development

BRUCE JOHNSTON, Stanford University

*Discussants:* To be announced:

10:30 A.M. TECHNOLOGICAL CHANGE: STIMULI, CONSTRAINTS, RETURNS

*Chairman:* BURT KLEIN, The RAND Corporation

*Papers:* Rates of Return from Industrial Research and Development

EDWIN MANSFIELD, University of Pennsylvania and Harvard University

Market Structure, Business Conduct, and Innovation

JESSE MARKHAM, Princeton University

Economic Theory and Technological Change

JACOB SCHMOOKLER, University of Minnesota

*Discussants:* MORRIS ADELMAN, Massachusetts Institute of Technology

ZVI GRILICHES, University of Chicago

RICHARD TYBOUT, Ohio State University

ECONOMIC THEORY AND NON-PROFIT ENTERPRISE

*Chairman:* M. A. EGGERS, Syracuse University

*Papers:* Economics of the Hospital

MELVIN REDER, Stanford University

Economics of the University

ALLAN CARTER, American Council on Education

Economics of the Performing Arts

WILLIAM BAUMOL and WILLIAM G. BOWEN, Princeton University

*Discussants:* MANUAL GOTTLIEB, University of Wisconsin at Milwaukee

SIMEON LELAND, Northwestern University

MARTIN BRONFENBRENNER, Carnegie Institute of Technology

MANPOWER AND WELFARE PROGRAMS: BENEFIT-COST ANALYSIS (Sponsored jointly by the American Economic Association and Industrial Relations Research Association)

*Chairman:* HERBERT PARNES, Ohio State University

*Papers:* Benefit-Cost Analysis of Manpower Retraining

GERALD SOMERS, University of Wisconsin

ERNST STRONSDORFER, University of Wisconsin

Benefit-Cost Analysis of Social Welfare Questions

JOHN S. MACDONALD, United Nations

Benefit-Cost Analysis of Labor Relocation

To be announced

*Discussants:* JUANITA KREPS, Duke University  
 SELMA MUSEKIN, Advisory Commission on Intergovernmental  
 Relations  
 BENSON SOFFER, Committee for Economic Development

2:00 P.M. DOMESTIC AND EXTERNAL EQUILIBRIUM: WESTERN EUROPEAN OBJECTIVES AND  
 POLICIES

*Chairman:* EMILE DESPRES

*Papers:* TIBOR SCITOVSKY, University of California, Berkeley  
 HERBERT STEIN, Committee for Economic Development  
 MILTON GILBERT, Bank for International Settlements

*Discussants:* JAMES INGRAM, University of North Carolina  
 R. A. MUNDELL, McGill University  
 JAMES TOBIN, Yale University

ECONOMIC GROWTH: INTERNATIONAL COMPARISONS

*Chairman:*

*Papers:* The Economic Growth of Argentina and Australia Compared  
 ARTHUR SMITHIES, Harvard University  
 The Economic Growth of India and Mainland China Compared  
 SIDNEY KLEIN, Rutgers—The State University

*Discussants:* W. M. CORDEN, Australian National University  
 WILFRED MALENBAUM, University of Pennsylvania  
 M. C. URQUHART, Queen's University

4:00 P.M. SIR JOHN HICKS: THE KEYNESIAN THEORY OF DISTRIBUTION

8:00 P.M. PRESIDENTIAL ADDRESS

*Chairman:* FRANK H. KNIGHT, University of Chicago

*Presidential Address:* GEORGE J. STIGLER, University of Chicago

*Wednesday, December 30, 1964*

8:30 A.M. NATIONAL WEALTH INVENTORY (Sponsored jointly by the American Economic  
 Association and the American Statistical Association)

*Chairman:* RAYMOND BOWMAN, Bureau of the Budget

*Papers:* Balance Sheets and Wealth Estimates as Part of the U.S. Economic  
 Accounts—Recent Progress

MILTON MOSS, Bureau of the Budget

Problems in the Valuation of Tangible Assets

JOHN KENDRICK, George Washington University

Trends in the Use of Wealth Data in Quantitative Economic  
 Analysis

JOEL POPKIN, Wealth Inventory Planning Study and George  
 Washington University

*Discussants:* PATRICK HUNTLEY, George Washington University

RICHARD RUGGLES, Yale University

COMPARATIVE ECONOMIC SYSTEMS: NATIONALIZED INDUSTRY (Sponsored jointly  
 by the American Economic Association and the Association for Comparative  
 Economics)

*Chairman:* ROBERT DORNBERGER, University of Chicago

*Papers:* British Socialism: Whether and Whither

BEN LEWIS, Oberlin College

Efficiency and the Socialist Firm in Yugoslavia

BENJAMIN WARD, University of California, Berkeley

*Discussants:* GEORGE MACESICH, Florida State University

HERBERT GRUBEL, University of Chicago

WILLIAM SHEPHERD, University of Michigan

## THE AMERICAN ECONOMIC REVIEW

## THE ECONOMICS OF POVERTY

Changed to Wednesday, Dec. 30, 10:30 A.M.—*Editor*

*Chairman:* MARSHALL COLBERG, Florida State University

*Papers:* The Economic Basis of a Concept of Poverty

T. W. SCHULTZ, University of Chicago

Approaches to the Reduction of Poverty

ROBERT J. LAMPFMAN, University of Wisconsin

Poverty: The Special Case of the Negro

ALAN B. BATCHELDER, Kenyon College

*Discussants:* GEORGE HILDEBRAND, Cornell University

HARRY G. JOHNSON, University of Chicago

MALCOLM LIGGETT, University of California; Santa Barbara

## GRADUATE STUDENT SESSION

*Chairman:* EVSEY DOMAR, Massachusetts Institute of Technology

*Papers:* An Analysis of Technological Change in the U.S. Fertilizer Minerals Industries During 1936-1960

G. S. SAHOTA, University of Chicago

A Stochastic Model of Commercial Bank Behavior

H. I. GROSSMAN, Johns Hopkins University

Growth Implications of Optimum Allocation in Diminishing Cost Industries

M. Z. FABRYCY, New York University

*Discussants:* To be announced

## 10:30 A.M. INVITED STUDENT DISSERTATIONS

*Chairman:* JAMES MORRIS, University of South Carolina

*Dissertations:* Income and the Allocation of Effort

GORDON WINSTON, Ph.D. Stanford University, Williams College

An Adaptive Production Function and Its Economic Implications

FERDINAND K. LEVY, Ph.D. Carnegie Institute of Technology, Graduate School of Business, Stanford University

Some Aspects of Technical Change, Capital Longevity, and Economic Growth

E. PHILIPS HOWREY, Ph.D. University of North Carolina, Princeton University

*Discussants:* VINCENT F. BOLAND, University of Arizona

HANS BREMS, University of Illinois

DAVID MCCORD WRIGHT, University of Georgia

## THE NEW NATIONAL ECONOMETRIC MODEL: ITS APPLICATION (Sponsored jointly by the American Economic Association and the Econometric Society)

*Chairman:* ROBERT STROTZ, Northwestern University

*Papers:* The SSRC-Brookings Quarterly Econometric Model of the United States: Model Properties

LAWRENCE KLEIN, University of Pennsylvania

GARY FROMM, Brookings Institution

Large-Scale Econometric Models—Is a New Age Dawning?

EDWIN KUH, Massachusetts Institute of Technology

*Discussants:* MARC NERLOVE, Stanford University

To be announced

## 2:00 P.M. ECONOMIC EDUCATION: EXPERIMENTS IN THE TEACHING OF ECONOMICS

Changed to Monday, Dec. 28, 2:00 P.M.—*Editor*

*Chairman:* G. L. BACH, Carnegie Institute of Technology

*Papers:* The Effectiveness of "Programmed Learning" in Economics

RICHARD ATTIEEH, Yale University

KEITH LUMSDEN, Stanford University  
 Role-Playing in Teaching Economics  
 MYRON JOSEPH, Carnegie Institute of Technology  
 Tests of the Success of Principles Courses  
 SIMON WHITNEY, Rutgers—The State University  
*Discussants:* IRWIN HERENSTADT, Northeastern University  
 DANIEL FUSFELD, University of Michigan  
 RENDIGS FELS, Vanderbilt University

#### SOME PUBLIC POLICY ISSUES IN REGULATED INDUSTRIES

Changed to Tuesday, Dec. 29, 2:00 P.M.—*Editor*

*Chairman:* ROBERT HARBESON, University of Illinois

*Papers:* Evaluation of Public Policy Relating to Radio and Television  
 Broadcasting: Social and Economic Issues

RONALD COASE, University of Virginia

The Interrelationship of the Changing Structure of American  
 Transportation and Changes in Industrial Location

MARVIN BARLOON, Western Reserve University

Effects of the Merger Movement upon the Structure of the Trans-  
 portation Industry: Considerations of Public Policy

R. A. NELSON, University of Washington and U.S. Department  
 of Commerce

*Discussants:* H. H. GOLDIN, Federal Communications Commission

W. H. DODGE, University of Wisconsin

W. N. LEONARD, Hofstra College

#### UNESCO

UNESCO wishes to ascertain the opinions of American economists regarding economic programs it had best undertake in the future. Accordingly, UNESCO and the A.E.A. have scheduled a joint session at the Association meetings in Chicago, December 28-30. Meanwhile members of the Association are invited to submit written proposals for consideration by UNESCO; some of these will be discussed at the joint session, together with the role of UNESCO in the field of international economic inquiry. A panel discussion may be utilized. Members who have suggestions or proposals may send them to Kenneth Boulding, Economics Department, University of Michigan, Ann Arbor, Mich., or to Joseph J. Spengler, P.O. Box 4723, Duke University, Durham, N.C. Those desiring basic information on UNESCO may address: Program Officer for the Social Sciences, U.S. National Commission for UNESCO, Department of State, Washington 25, D.C.

#### ART EXHIBITION

Arrangements have been made for an art exhibition in conjunction with this year's Annual Meeting of the A.E.A. Members who would like to display one or more of their works at the exhibition should write by October 31 to Harry G. Johnson, 1126 E. 59th Street, Chicago 37, indicating the number of works, the medium, and the space required for display. (It may be necessary to ration display space, as accommodation in the main convention area of the hotel is limited.)

#### NEW JOURNALS

Under the title *Co-existence*, a new journal began publication this year. The journal is devoted to "the comparative study of economics, sociology and politics in a changing world." It "will advance responsible thinking on co-existence, which is recognized as the alternative to the cold war. . . . The Journal will include survey articles, correspondence and documentation."

The editor is Dr. Rudolph Schlesinger, Inverloch, Kilmun by Duncon, Argyll, Scotland.

A new journal, *Demography*, began publication this year. It is a publication of the Population Association of America; the editor is Donald J. Bogue, 1126 E. 59th Street, University of Chicago, Chicago, Illinois 60637.

### DIRECTORY OF WORKERS IN THE ECONOMICS OF FORESTRY

The Division of Forest Economics and Policy of the Society of American Foresters is in process of compiling a world-wide directory of workers in the economics of forestry. The Division plans to include the names of all persons who have a major professional interest in economics as applied to producing, harvesting, processing, marketing, or consumption of forest products or services.

Registration forms for the directory were mailed during the first week in September to all forest and forest-industry economists of record. Those who do not receive forms but who wish to be included in the directory are asked to write to the chairman of the directory committee: William A. Duerr, State University College of Forestry, Syracuse, New York, 13210.

### CORRESPONDENCE COURSE

The School of Extension Studies of the Industrial College of the Armed Forces invites managers in business, industry, and the professions to join senior military officers and government officials in studying the economic and industrial aspects of our nation's position in today's world through the correspondence course entitled "The Economics of National Security."

Admission requirements for private citizens are a bachelor's degree or 12 years of business experience; for military officers, the rank of major or above. Apply directly to the Commandant, Industrial College of the Armed Forces, Washington 25, D.C., Attn.: Correspondence Course Division.

### FELLOWSHIPS

The Office of the Comptroller of the Currency announces the establishment of research fellowships in banking and monetary affairs. Fellowships will be awarded to outstanding economists from colleges and universities for a period of up to one year, and holders will pursue their research in the Department of Banking and Economic Research in the Office of the Comptroller of the Currency.

The Society for Religion in Higher Education is offering a number of postdoctoral fellowships to encourage the growing interest in the relation of scholarship in religion to scholarship in other fields of study. These fellowships are made possible through a grant from the Danforth Foundation.

Grants are available to scholars in the humanities, social and natural sciences for a year of study in religion and to scholars in religion for a year of study in another discipline. These fellowships are open to scholars in any institution of higher learning in the United States or Canada without respect to discipline, religious affiliation or non-affiliation. Scholars applying for these fellowships grants must have a Ph.D. degree or its equivalent plus at least three years of teaching experience. Preference will be given to scholars under 50 years of age. The maximum stipend is \$8,500. Stipends will be based on experience, present salary, and availability of other resources.

Completed applications must be received by the Society by December 1, 1964. Application forms and additional information may be secured from Lawrence P. DeBoer, Executive Director, The Society for Religion in Higher Education, 400 Prospect Street, New Haven, Connecticut 06511.

### *Deaths*

Emerson Wirt Axe, March 26, 1964.

Edward G. Bennion, Stanford University, October 1963.

Costantino Bresciani-Turroni, December 7, 1963.

Orin K. Burrell, professor of finance, University of Oregon, May 26, 1964.

Rosemary C. Griffith, assistant professor of economics, University of New Mexico, May 12, 1964.

William W. Hewett, professor, economics department, University of Cincinnati, April 1962.

Grover Gerhardt Huebner, March 9, 1964.

H. L. H. Smith, March 31, 1964.

Pascal K. Whelpton, April 6, 1964.

### *Retirements*

Paul E. Alyea, professor of finance, University of Alabama.

James F. Cusick, Dartmouth College, June 1964.

Fern Willard Gleiser, professor of institution economics and management, University of Chicago, September 30, 1964.

William R. Hockenberry, associate professor of industry and director, Evening and Extension Schools, July 1964.

Paul T. Homan, professor of economics, Southern Methodist University, July 1964.

Robert B. Mitchell, professor of accounting, Wharton School, University of Pennsylvania, July 1964.

Royal E. Montgomery, professor emeritus, Cornell University.

Margaret G. Myers, professor, department of economics, sociology, and anthropology, Vassar College.

S. H. Nerlove, professor of business economics and policy, University of Chicago, October 1964.

Edward Petty, professor of economics, University of Oklahoma.

Andrew B. Schmidt, professor of economics, University of Arizona, August 1964.

Harry F. R. Shaw, Dartmouth College, June 1964.

Richard T. Stevens, department of economics, Ohio State University.

Amos E. Taylor, professor, department of economics, American University, June 1964.

### *Visiting Foreign Scholars*

Sotiris Agapitidis, Technical University of Athens: visiting professor of economics, University of Utah, autumn quarter 1964.

John Graeme Head, Australian National University: visiting lecturer, department of economics, Princeton University, spring term 1964-65.

Ryutaro Komiya, University of Tokyo: visiting professor of economics, Stanford University, academic year 1964-65.

Michio Morishima, Osaka University: visiting professor, department of economics, Stanford University, autumn quarter 1964-65.

Robert H. Wallace, University of Adelaide: visiting assistant professor, department of economics, Stanford University, academic year 1964-65.

### *Promotions*

F. Gerard Adams: associate professor, economics department, Wharton School, University of Pennsylvania.

Edward C. Atwood, Jr.: professor of economics, Washington and Lee University, February 1962.

John H. Auten: professor of economics, Rice University.

Morris Beck: professor of economics, Rutgers—The State University.

Robert C. Bingham: associate professor of economics, Kent State University.

V. F. Boland: professor of economics, University of Arizona.

Arthur M. Borak: professor, department of economics, University of Minnesota.

Morris Bornstein: professor of economics, University of Michigan.

W. Donald Bowles: professor of economics, American University.

David Grant Brown: associate professor of economics, University of North Carolina.

Colin D. Campbell: professor, economics department, Dartmouth College.

- Arnold Coltery: professor of economics, Amherst College, July 1964.
- Anthony Costantino: professor of economics, State University of Iowa.
- Darwin W. Daicoff: associate professor, department of economics, University of Kansas.
- J. Kenneth Davies: professor, economics department, Brigham Young University.
- Douglas F. Dowd: professor, department of economics, Cornell University.
- James E. Dugan: professor of economics, University of Colorado.
- Walter D. Fackler: professor of business economics, University of Chicago.
- J. Kaye Faulkner: assistant professor of economics, Western Washington State College.
- James M. Ferguson: associate professor of economics, University of Virginia.
- Leslie Fishman: professor of economics, University of Colorado.
- Arthur M. Freedman: associate professor, finance department, Wharton School, University of Pennsylvania, July 1964.
- Seymour Friedland: professor of business, Claremont Graduate School.
- Daniel R. Fusfeld: professor of economics, University of Michigan.
- Jack Gelfand: professor of economics, Temple University.
- Dorothy C. Goodwin: associate professor of economics, University of Connecticut.
- Osmond L. Harline: professor, economics department, University of Utah.
- Louis T. Harms: professor of economics, Temple University.
- Bernard P. Herber: associate professor of economics, University of Arizona.
- Robert E. Hill: professor of economics, Kent State University.
- Thomas Holland: associate professor of economics, University of Alabama.
- David S. Huang: associate professor, department of economics, University of Texas.
- John D. Huffnagle: assistant professor of economics, Kent State University.
- Louis J. Junker: associate professor of economics, Western Michigan University.
- John H. Kareken: professor, department of economics, University of Minnesota.
- Heinz Köhler: associate professor of economics, Amherst College, July 1964.
- John M. Kohlmeier: assistant professor of accounting, University of Chicago.
- Viola Wykoff Komarek: professor, New York University.
- A. J. Kondonassis: professor of economics, University of Oklahoma.
- Harvey J. Levin: Augustus B. Weller professor of economics, Hofstra University.
- Wallace F. Lovejoy: professor, department of economics, Southern Methodist University.
- David B. McCalmont: associate professor, department of economics, Ohio State University.
- Donald McClurg: associate professor, department of economics, University of Colorado.
- Donald A. MacRae: professor of business, Mankato State College.
- Mildred Massey: associate professor of economics, Los Angeles State College.
- Erwin S. Mayer: professor of economics, Western Washington State College.
- Daniel C. Morgan, Jr.: associate professor, department of economics, University of Texas.
- Eva L. Mueller: professor of economics, University of Michigan.
- Edward Nelson: professor of finance, Los Angeles State College.
- Alvan J. Obelsky: associate professor of economics, University of Michigan, Dearborn.
- John L. O'Donnell: professor of financial administration, Graduate School of Business Administration, Michigan State University.
- Ronald R. Olsen: associate professor, department of economics, University of Kansas.
- Robert S. Osaki: associate professor of economics, California State College, Hayward.
- Wyn F. Owen: professor of economics, University of Colorado.
- B. P. Pashigian: associate professor of business economics, University of Chicago.

- Robert F. Pethia: assistant professor of economics, Duke University.
- Charles Franklin Phillips, Jr.: associate professor of economics, Washington and Lee University, September 1963.
- Gaston V. Rimlinger: professor of economics, Rice University.
- Gerhard Rosegger: associate professor of economics, Case Institute of Technology.
- Myron H. Ross: professor of economics, Western Michigan University.
- Samuel R. Sapienza: professor, accounting department, Wharton School, University of Pennsylvania.
- Hugh H. Schwartz: assistant professor, department of economics, University of Kansas.
- Martin Segal: professor, economics department, Dartmouth College.
- Harry G. Shaffer: associate professor, department of economics, University of Kansas.
- William D. Shipman: associate professor, economics department, Bowdoin College.
- Werner Sichel: assistant professor of economics, Western Michigan University.
- Tadeusz A. Siedlik: professor of business and economics, University of Maine.
- Morris Singer: associate professor of economics, University of Connecticut.
- Matthew J. Stephens, Jr.: assistant professor, accounting department, Wharton School, University of Pennsylvania, July 1964.
- James N. Tattersall: associate professor of economics, University of Oregon.
- Lester G. Telser: professor of applied economics, University of Chicago.
- Richard H. Tilly: assistant professor of economics, University of Michigan.
- Arnold R. Weber: professor of industrial relations, University of Chicago.
- Larry T. Wimmer: assistant professor, economics department, Brigham Young University.
- Richard B. Wirthlin: associate professor, economics department, Brigham Young University.
- Shih-Cheng Yu: professor of business and economics, University of Maine.

### *Administrative Appointments*

- Russell L. Ackoff: professor and chairman, department of statistics and operations research, Wharton School, University of Pennsylvania, July 1964.
- Kenneth O. Alexander, Michigan Technological University: professor of economics and associate director, Bureau of Business and Economic Research, State University of Iowa.
- Richard M. Bailey, Council of Economic Advisers: chief economist, National Aeronautical and Space Administration.
- Robert L. Bishop: acting dean, School of Humanities and Social Science, Massachusetts Institute of Technology, spring 1964.
- Patrick M. Boarman, Bucknell University: manager of economic research, General Electric Co., New York.
- O. H. Brownlee: chairman, department of economics, University of Minnesota.
- Harvey C. Bunke: chairman, department of economics, State University of Iowa.
- Ronald K. Calgaard: associate chairman, department of economics, University of Kansas, September 1964.
- Peter M. Cody: director, Office of Vietnam Affairs, AID, June 1964.
- Robert G. Cox, Wharton School, University of Pennsylvania: dean, School of Business Administration, Syracuse University.
- John V. Deaver: vice president and associate director, economic research division, Chase Manhattan Bank, April 1964.
- Bernard Hall: chairman, department of economics, Kent State University.
- Bernard P. Herber: assistant dean, College of Business and Public Administration, University of Arizona.
- Frederick G. Kempin: vice dean, undergraduate division, Wharton School, University of Pennsylvania, July 1964.

Leland C. Lehman: chairman, department of economics, Denison University, September 1964.

Robert L. MacDonald: director, evening and extension schools, Wharton School, University of Pennsylvania, July 1964.

H. David Maloney: in charge, department of economics, DePauw University.

Leonard G. Mathy: dean, School of Letters and Sciences, Los Angeles State College.

Charles L. Merwin: deputy director, African Department, International Monetary Fund.

Bruce R. Morris: acting head, department of economics, University of Massachusetts, academic year 1964-65.

John C. Norby: chairman, department of economics, Los Angeles State College.

Herbert R. Northrup: chairman, industry department, Wharton School, University of Pennsylvania, January 1964.

Rudolph R. Rhomberg: chief, Special Studies Division, Research and Statistics Department, International Monetary Fund.

Gaston V. Rimlinger: acting chairman, department of economics, Rice University, first semester 1964-65.

Robert W. Rudd: acting dean, College of Commerce, University of Kentucky.

John C. Sawhill, New York University: associate executive director, National Bureau of Economic Research, August 3, 1964.

Leon Schur, Louisiana State University: director, Wisconsin Council on Economic Education and professor of economics, University of Wisconsin-Milwaukee, September 1964.

Floyd R. Simpson: dean, School of Business and Economics and senior dean, Los Angeles State College.

John M. Stockton: associate professor and chairman, business law department, Wharton School, University of Pennsylvania, July 1964.

C. F. Joseph Tom: associate professor of economics and chairman, department of economics and business administration, Lebanon Valley College.

John G. Turnbull: associate dean, College of Liberal Arts, University of Minnesota.

Jacob Weissman: chairman, department of economics, Hofstra University.

Richard B. Wirthlin: chairman, department of economics, Brigham Young University.

Richard S. Woods: professor and chairman, accounting department, Wharton School, University of Pennsylvania, January 1964.

### *Appointments*

William W. Abendroth: lecturer in accounting, Management Science Center, Wharton School, University of Pennsylvania.

Norman D. Aitken: instructor in economics, University of Massachusetts.

George L. Almond: associate professor of business and economics, School of Business Administration, University of Maine.

Takeshi Amemiya: acting assistant professor, department of economics, Stanford University, winter and spring quarters, 1964-65.

Elaine Andrews: instructor, economics department, Boston University.

Sven W. Arndt: acting assistant professor, department of economics, University of California, Los Angeles.

Lowell DeWitt Ashby, University of North Carolina: U.S. Department of Commerce.

Trevor Bain: assistant professor of economics, University of Arizona.

Alan B. Batchelder, Ohio State University: Kenyon College.

Nevins Baxter: assistant professor, finance department, Wharton School, University of Pennsylvania.

William F. Beazer: acting assistant professor of economics, University of Virginia.

Jack Newton Behrman: associate professor of international business administration, University of North Carolina.

Walter Bennett: assistant professor, School of Business Administration, Old Dominion College.

Frank Benson: visiting associate professor, department of statistics and operations research, Wharton School, University of Pennsylvania, July 1964.

Charles H. Berry: senior staff, Brookings Institution, September 1963.

Gary W. Bickel: acting assistant professor, department of economics, University of Colorado, 1964-65.

John W. Birch, University of Illinois: associate professor of econometrics, University of Wyoming.

Roger Bird, University of Pennsylvania: instructor in economics, Lafayette College.

Aryeh Blumberg: instructor in business economics, University of Chicago, July 1964.

Edward J. R. Booth, Oklahoma State University: associate professor of economics, University of Connecticut.

E. N. Botsas, Wayne State University: instructor in economics, Lafayette College.

Camille J. Botte: assistant professor, department of economics, Ohio State University, October 1964.

Patricia F. Bowers: instructor, department of economics, Brooklyn College.

Leonard R. Burgess, North Texas State University: associate professor of business administration, Texas A. and M. University, September 1964.

Glen D. Camp: professor, department of statistics and operations research, Wharton School, University of Pennsylvania, July 1964.

Doris C. Cash: visiting assistant professor of economics, Mercer University, summer quarter 1964.

William S. Cassels: department of economics and sociology, Mount Holyoke College.

R. Woodrow Castle: associate professor of economics, West Virginia Wesleyan College, June 1964.

M. Umer Chapra: assistant professor of economics, University of Kentucky.

Sam B. Chase, Jr.: senior staff, Brookings Institution, April 1964.

Manas Chatterji: visiting assistant professor, regional science department, Wharton School, University of Pennsylvania, July 1964.

Yung-Ping Chen: assistant professor, department of economics, University of California, Los Angeles.

Hollis B. Chenery, Stanford University: professor of economics, Harvard University, August 1964.

Alpha C. Chiang, Denison University: professor of economics, University of Connecticut, September 1964.

Gerald L. Childs: assistant professor, Dartmouth College.

Roger K. Chisholm: assistant professor, department of economics, University of Kansas, September 1964.

Gregory C. Chow: visiting professor, department of economics, Cornell University, fall term 1964.

Kuk Soo Chung: instructor, department of economics, sociology and anthropology, Vassar College.

Ronald H. Coase: professor of economics, University of Chicago.

Sanford Cohen: visiting professor of economics, University of Michigan, 1964-65.

Peter M. Costello: assistant professor of economics, Smith College.

John H. Cover: economic planning consultant, Government of Barbados.

Clyde Crobaugh: lecturer in business and economics, University of Maine, Portland, fall 1964.

Thomas D. Curtis: assistant professor of economics, University of Arizona.

Irene Davidson: instructor, School of Business Administration, Old Dominion College.

Frank S. Deming: lecturer, accounting department, Wharton School, University of Pennsylvania, July 1964.

David S. DeShon, University of Missouri: instructor in economics, Western Michigan University.

Ely Devons, London School of Economics: John Hinkley professor of political economy, Johns Hopkins University, fall term 1964-65.

William G. Dewald, University of Chicago: associate professor, department of economics, Ohio State University, October 1964.

Phoebus J. Dhrymes: associate professor, economics department, Wharton School, University of Pennsylvania, July 1964.

Robert M. Dunn, Jr.: assistant professor, Dartmouth College.

John D. Durand: professor, department of economics and sociology, Wharton School, University of Pennsylvania, July 1964.

Frank A. Durgin, Jr.: associate professor of business and economics, University of Maine, Portland, fall 1964.

Dazimiere Dziewonski: visiting associate professor, department of regional science, Wharton School, University of Pennsylvania.

Henry Einhorn, Pennsylvania State University: assistant professor of economics, The City College of New York, 1964-65.

W. Duane Evans: professor of economics and statistics, Cornell University.

J. Robert Ferrari: lecturer, insurance department, Wharton School, University of Pennsylvania, July 1964.

David L. Foulkes: visiting lecturer, business law department, Wharton School, University of Pennsylvania, July 1964.

Stanley L. Friedlander: instructor in economics, The City College of New York, 1964-65.

Gene A. Futrell: assistant professor, department of economics and sociology, Iowa State University.

Herman Gadon: associate professor of business administration, Whittemore School, University of New Hampshire.

Lowell E. Galloway: associate professor, industry department, Wharton School, University of Pennsylvania, July 1964.

Marvin Gelfand: instructor in economics, University of Massachusetts.

Henry N. Goldstein, Federal Reserve System: associate professor of economics, College of Economics and Business, Washington State University.

Richard Gordon: associate professor, School of Business Administration, Old Dominion College.

Walter Gross, Texas College of Arts and Industries: associate professor of marketing, University of Georgia.

Benton E. Gup: instructor in economics, University of Cincinnati.

William Habacivch, University of Illinois: instructor in economics, University of Delaware.

Carl W. Hale: regional economist, Federal Reserve Bank of Dallas.

James D. Hammond, Ohio State University: Pennsylvania State University.

Giora Hanoch: assistant professor, department of economics, University of Chicago, academic year 1963-64.

Everett D. Hawkins, Mount Holyoke College: department of economics, University of Wisconsin.

Bruce H. Herrick: acting assistant professor, department of economics, University of California, Los Angeles.

Tracy G. Herrick, Federal Reserve Bank, Cleveland: corporate planner, Ferro Corporation, Cleveland, April 6, 1964.

Sherry Hessler: lecturer, regional science department, Wharton School, University of Pennsylvania, July 1964.

George H. Hildebrand: visiting professor of industrial relations, department of economics, Massachusetts Institute of Technology, academic year 1964-65.

- Clifford G. Hildreth: professor, department of economics, University of Minnesota.
- James R. Hoath: visiting lecturer in economics, University of Nebraska, 1964-65.
- Shinsuke Horiuchi: instructor, department of economics, University of Alabama.
- Byron L. Johnson: professor of economics, University of Colorado, February 1965.
- Stanley Johnson: assistant professor of economics, University of Missouri, fall semester 1964-65.
- Walter Johnson: assistant professor of economics, University of Missouri.
- William A. Johnson: research staff, RAND Corporation.
- Lamar B. Jones: assistant professor, department of economics, Kansas State University.
- Max B. Jones: associate professor, School of Business Administration, Old Dominion College.
- Pamela H. Kacser: assistant professor of economics, Catholic University of America, September 1964.
- W. E. Kahn, Federal Reserve Bank, Chicago: Fulbright lecturer, University of Dacca, East Pakistan, academic year 1964-65.
- Laveen Kanal: visiting associate professor, department of statistics and operations research, Wharton School, University of Pennsylvania, July 1964.
- David Kaun: research associate, Brookings Institution, July 1964-August 1965.
- Douglas H. Keare: instructor, department of economics, Princeton University.
- John H. Kendrick, George Washington University: professor of economics, University of Connecticut.
- Earl W. Kersten, University of Nevada: visiting lecturer in economic geography, Washington University, St. Louis, June 1964.
- M. V. Khambadkone: visiting lecturer, finance department, Wharton School, University of Pennsylvania, July 1964.
- Edward R. Kittrell: associate professor of economics, University of Alabama.
- Walter P. Klein: instructor, economics department, Boston University.
- Ernest Koenigsberg: professor, industry department, Wharton School, University of Pennsylvania, July 1964.
- Frederick W. Kohlmeyer, University of Illinois: Illinois Normal University.
- Susumu Koizumi: visiting associate professor, economics department, Wharton School, University of Pennsylvania, July 1964.
- Anthony Y. C. Koo, Michigan State University: professor of economics, University of Michigan.
- Iwan S. Koropecyk: assistant professor of economics, Temple University.
- Dwight R. Ladd: professor of business administration, Whittemore School, University of New Hampshire.
- James Clayburn LaForce: assistant professor, department of economics, University of California, Los Angeles.
- Maw Lin Lee, University of British Columbia: assistant professor of economics, Washington University, St. Louis, July 1964.
- Axel Leijonhufvud: acting assistant professor, department of economics, University of California, Los Angeles.
- James R. Leonard, Ohio State University: Robert Nathan Associates.
- Bertram F. Levin, University of Virginia: associate professor of economics, University of Delaware.
- Ferdinand Levy, Stanford University: associate professor of economics, Rice University, February 1965.
- W. Arthur Lewis: distinguished visiting professor of economics, The City University of New York, spring semester 1965.
- E. E. Liebhafsky, University of North Carolina: professor of economics, University of Houston, September 1964.

Jung-Chao Liu, Southern Methodist University: department of economics, McGill University, 1964-65.

David McFarland: associate professor of economics, University of North Carolina.

L. R. McGee: assistant professor of economics, University of Kentucky.

James McGreevey: assistant professor of economics, University of Oregon.

Roland N. McKean: professor of economics, University of California, Los Angeles.

Miles W. Martin, Jr.: assistant professor, department of statistics and operations research, Wharton School, University of Pennsylvania, July 1964.

William P. Matlack: instructor, department of statistics and operations research, Wharton School, University of Pennsylvania, July 1964.

Abraham Melesin: assistant professor of economics, The City College of New York.

Allan Meltzer, Carnegie Institute of Technology: Ford Foundation visiting professor, department of economics, University of Chicago, 1964-65.

John Merriam: assistant professor of economics, Idaho State University, academic year 1964-65.

Lloyd A. Metzler: visiting professor of economics, University of California, Los Angeles, fall semester 1964.

J. Sayer Minas: associate professor, department of statistics and operations research, Wharton School, University of Pennsylvania, July 1964.

Jora Minasian: associate professor of economics, State University of New York, Buffalo.

Michael Mischaikow, West Virginia University: associate professor of economics, Western Washington State College.

Edward Mitchell: lecturer, economics department, Wharton School, University of Pennsylvania, July 1964.

John E. Moes: associate professor of economics, Florida State University, September 1964.

John R. Moroney, Jr.: assistant professor of economics, Florida State University, September 1964.

Clarence C. Morrison: acting assistant professor of economics, University of Virginia.

Michael Moskow, Drexel Institute of Technology: instructor in economics, Lafayette College.

James Lee Murphy: assistant professor of economics, University of North Carolina.

Ramon H. Myers, University of Hawaii: department of economics, Australian National University, Canberra.

Wladimir Naleszkiewicz: associate professor of economics, Notre Dame University.

G. V. L. Narisimham, Kansas State University: assistant professor of economics, Western Michigan University.

S. H. Nerlove: senior lecturer in business economics and policy, Graduate School of Business Administration, University of California, Los Angeles.

Peter Newman: visiting professor of political economy, Johns Hopkins University, academic year 1964-65.

Jan M. Novotny: lecturer, economics department, Boston University.

Jeremiah H. O'Connell: lecturer, industry department, Wharton School, University of Pennsylvania, July 1964.

David M. O'Neill: lecturer, economics department, Wharton School, University of Pennsylvania, July 1964.

Allan V. Palmer: associate professor, School of Business Administration, Old Dominion College.

Alfred L. Parker, Ohio State University: assistant professor of economics, University of New Mexico.

- Jan Parker, Wellesley College: assistant professor of economics, Wheaton College.
- Sam Peltzman: acting assistant professor, department of economics, University of California, Los Angeles.
- Donald K. Pemberton, University of Kansas: assistant professor of economics, Hiram College.
- Richard Perlman, Adelphi College: professor of economics, University of Wisconsin-Milwaukee, September 1964.
- Lon Polk: lecturer in economics, University of Michigan, Dearborn.
- Robert A. Pollak: lecturer, economics department, Wharton School, University of Pennsylvania, July 1964.
- Richard C. Porter, Yale University: associate professor of economics, University of Michigan.
- Richard H. Puckett: instructor in economics, American University.
- Allan G. Pulsipher, Tulane University: assistant professor, department of economics, Texas A. and M. University, academic year 1964-65.
- Chong-Soo Pyun: assistant professor of economics, Mercer University.
- John Trout Rader: assistant professor, department of economics, University of Illinois.
- Cadwell L. Ray: assistant professor, department of economics, Texas Western College.
- Ray C. Roberts: visiting associate professor of economics, Duke University, 1964-65.
- Robert F. Rooney: acting assistant professor, department of economics, University of California, Los Angeles.
- David F. Ross, Bethany College: professor of economics, Cuttington College, Liberia.
- Ernest Rubin: adjunct professor of economics, Catholic University of America, February 1964.
- Neville L. Rucker: assistant professor of economics, Old Dominion College.
- George Sadler: lecturer, department of economics, University of South Alabama.
- Arnold E. Saffer, Rice University: economist, Humble Oil Co., Houston.
- Hyman Sardy: assistant professor of economics, Brooklyn College.
- Donald T. Savage: assistant professor of economics, Clark University.
- Thomas T. Schweitzer: economist, Economic Council of Canada.
- Satya S. Sengupta: associate professor, department of statistics and operations research, Wharton School, University of Pennsylvania, July 1964.
- Larry G. Sgontz, Western Illinois University: assistant professor of economics, State University of Iowa.
- Harold T. Shapiro, Princeton University: assistant professor of economics, University of Michigan.
- William F. Sharpe: research staff, RAND Corporation.
- Surendra S. Singhvi: instructor in business administration, Whittemore School, University of New Hampshire.
- Ralph Smith, University of Maryland: instructor in economics, Southern Methodist University.
- Stanley Smith: senior investment analyst, Hemphill, Noyes and Co., New York.
- Eugene Smolensky, Haverford College: Lilly Faculty Fellow, Graduate School of Business, University of Chicago, academic year 1964-65.
- Hugo Sonnenschein: lecturer, department of economics, University of Minnesota.
- Gary W. Sorenson: instructor in economics, College of Economics and Business, Washington State University.
- Shyamalata Srivastava: lecturer, department of economics, Goucher College.
- William W. Stevenson: economist, Office of Tributary Area Development, TVA, Knoxville.

Vladimir Stoikov, Queens College: economic division, International Labour Office, Geneva.

Helen M. Stone: lecturer in economics, Smith College.

Raymond S. Strangways: assistant professor of economics, Old Dominion College.

Sang C. Suh: assistant professor of economics, Clark University.

Vincent Joseph Tarascio: lecturer in economics, University of North Carolina.

Vern Terpstra: lecturer, marketing department, Wharton School, University of Pennsylvania, July 1964.

Samuel L. Thorndike, Jr., Columbia University: instructor in economics, University of Wisconsin-Milwaukee, September 1964.

James E. Thummel: instructor in business administration, College of Economics and Business, Washington State University.

Harold G. Vatter, Carleton College: Lilly Faculty Fellow, Graduate School of Business, University of Chicago, academic year 1964-65.

Thomas J. Velk: assistant professor, Dartmouth College.

Roger N. Waud: instructor in business economics, Graduate School of Business, University of Chicago, October 1964.

Ross A. Webber: lecturer, industry department, Wharton School, University of Pennsylvania, July 1964.

Donald C. Wellington, University of Chicago: assistant professor of economics, San Diego State College.

Andrew Whinston: associate professor of economics, University of Virginia.

William G. Whitney: lecturer, economics department, Wharton School, University of Pennsylvania, July 1964.

John H. Wicks, Ohio State University: Montana University.

Peter J. D. Wiles: visiting professor of economics, The City College of New York, 1964-65.

H. Francois Wilkinson: assistant professor, Dartmouth College.

Walter Williams: associate professor of economics, University of Kentucky.

William H. Wrean: assistant professor, Dartmouth College.

Dwayne E. Wrightsman: assistant professor of finance, Whittemore School, University of New Hampshire.

De-Min Wu: assistant professor, department of economics, University of Kansas.

Shin Yen Wu, Los Angeles State College: visiting associate professor of economics, State University of Iowa.

Lee Van Zant: assistant professor, department of economics, Texas Western College.

Arvid M. Zarley: assistant professor, department of economics, University of Kansas, September 1964.

Raymond Zelder, Northern Trust Co.: associate professor of economics, Western Michigan University.

Arnold Zellner, University of Wisconsin: Ford Foundation visiting professor, department of economics, University of Chicago, 1964-65.

Don R. Ziegenbein: instructor in business and economics, University of Maine, fall 1964.

Oleg Zinam: assistant professor of economics, University of Cincinnati.

Laszlo Zsoltos, University of Kentucky: associate professor of economics, University of Delaware.

### *Leaves for Special Appointments*

Jarvis M. Babcock, University of Michigan: staff, Council of Economic Advisers.

Raymond R. Beneke, Iowa State University: AID Iowa-Peru project on agrarian reform and economic development, June 12-August 31, 1964.

Gordon Bertram, Los Angeles State College: visiting associate professor, University of Washington, academic year 1964-65.

Arthur I. Bloomfield, Wharton School, University of Pennsylvania: Ford Foundation in Malaysia, fall term 1964-65.

Diran Bodenhorn, Ohio State University: University of Ibadan, Nigeria.

W. Donald Bowles, American University: American University School of International Service project in African countries, September-December 1964.

Eugene A. Brady, Iowa State University: AID Iowa-Peru project on agrarian reform and economic development, June 1964 for eighteen months.

Andrew F. Brimmer, Wharton School, University of Pennsylvania: deputy assistant secretary for economic affairs, U. S. Dept. of Commerce, academic year 1964-65.

John A. Buttrick, University of Minnesota: University of Minnesota-Los Andes-AID project in Bogota, Colombia, 1964-65.

Meredith O. Clement, Dartmouth College: Brookings research professorship.

James V. Cornells, Columbia Teachers College: economist with technical assistance team, Peru.

Dorothy W. Douglas, Hofstra University: visiting professor of economics, University College, Nairobi, academic year 1964-65.

Ian M. Drummond, University of Toronto: visiting assistant professor of economics, Princeton University, spring 1964-65.

Edgar O. Edwards, Rice University: Ford Foundation economic adviser, Government of Kenya until January 1965.

Alan Fechter, University of Pittsburgh: Institute for Defense Analysis, 1964-65.

Reidigs Feis, Vanderbilt University: National Bureau of Economic Research, June 1-October 31, 1964.

Lehman B. Fletcher, Iowa State University: AID Iowa-Peru project on agrarian reform and economic development, March 1-May 31, 1964.

Edward M. Foster, University of Minnesota: Chief of Party, University of Minnesota-Los Andes-AID project in Bogota, Colombia, 1964-65.

Philip L. Gamble, University of Massachusetts: Tunghai University, Taichung, Taiwan, academic year 1964-65.

Cornelius W. Gillam, Wharton School, University of Pennsylvania: Stanford University, academic year 1964-65.

Bela Gold, University of Pittsburgh: Nuffield College, Oxford, April-November 1964.

William Hamburger, University of North Carolina: Institute for Defense Analysis, academic year 1964-65.

Morrison Handsaker, Lafayette College: Fulbright lecturer, Yokohama National University and Waseda University, Japan, academic year 1964-65.

George Heitmann, Pennsylvania State University: economic adviser, Kingdom of Libya.

Benjamin H. Higgins, University of Texas: economic adviser to U. S. Embassy, AID Mission to Brazil and Brazilian government, first semester 1964-65.

Ralph H. Hofmeister, University of Minnesota: professor, University of Minnesota-Los Andes-AID project in Bogota, Colombia, 1964-65.

Robert W. Johnson, Michigan State University: reporter, National Conference of Commissioners on Uniform State Laws, Chicago.

Paul W. MacAvoy, Massachusetts Institute of Technology: visiting assistant professor of business economics, University of Chicago, summer quarter 1964.

Wilfred Malenbaum, Wharton School, University of Pennsylvania: Ford Foundation faculty fellowship, academic year 1964-65.

Joseph A. Martellaro, Indiana University: Fulbright lecturer in economic development, National University of the South, Argentina, summer 1964.

F. Eugene Melder, Clark University: Fulbright lecturer, Haile Selassie I University, Addis Ababa, Ethiopia.

Jerome W. Milliman, Indiana University: AID consultant, Colombia, Ethiopia and Thailand, summer 1964.

Edwin S. Mills, Johns Hopkins University: staff, President's Council of Economic Advisers, academic year, 1964-65.

George R. Morrison, Cornell University: research fellow, National Bureau of Economic Research, academic year, 1964-65.

Edward Nelson, Los Angeles State College: international development team, Brazil, summer 1964.

John L. O'Donnell, Michigan State University: consultant, Turkish Academics of Economics and Commercial Sciences, Ankara, Eskisehir, Istanbul and Izmir, 1964-66.

James R. Prescott, Iowa State University: AID Iowa-Peru project on agrarian reform and economic development, June 1-August 31, 1964.

S. S. Schor, Wharton School, University of Pennsylvania: director of statistics, American Medical Association, Chicago, academic year 1964-65.

J. T. Scott, Iowa State University: Chief of Party, AID Iowa-Peru project on agrarian reform and economic development, April 1964 for one year.

Richard U. Sherman, Ohio State University: War College, Washington.

Gerald Sirkin, The City College of New York: Rockefeller research grant, Manila, Philippines, 1964-65.

Milton H. Spencer, Wayne State University: visiting professor of economics, Graduate School of Business Administration, New York University, summer session 1964.

John R. Tabb, Old Dominion College: AID Cornell-Liberia project, University of Liberia, Monrovia, Liberia for three months.

Roger C. Van Tassel, Clark University: director of research, New England Foundation.

Douglas Vickers, Wharton School, University of Pennsylvania: Ford Foundation faculty fellowship, academic year 1964-65.

Sidney Weintraub, Wharton School, University of Pennsylvania: general economics theorist, National Council of Applied Economic Research, New Delhi, fall term 1964-65.

Sam Wu, Los Angeles State College: visiting associate professor, State University of Iowa, academic year 1964-65.

### *Resignations*

Richard Attiyeh, Stanford University, August 31, 1964.

Heinz Biesdorf, University of Pittsburgh.

Jacob G. Birnberg, University of Chicago, September 30, 1964.

John D. Bowman, University of Illinois.

Franklin B. Evans, University of Chicago, September 30, 1964.

James M. Folsom, Duke University.

James Gherity, State University of New York, Buffalo.

Paul T. Hartmann, Stanford University, August 31, 1964.

David G. Hayes, Pennsylvania State University.

Harmon Hayden Haymes, Washington and Lee University, June 1964.

Sidney Miller, Wharton School, University of Pennsylvania, July 1, 1964.

John G. Myers, University of Colorado, June 30, 1964.

Richard Siegel, State University of New York, Buffalo.

Larry A. Sjaastad, University of Minnesota.

Hirofumi Uzawa, Stanford University, August 31, 1964.

Stanislaw H. Wellisz, University of Chicago, September 30, 1964.

Carl R. Wendoloski, University of Massachusetts.

William M. Wendt, American University.

## SIXTY-FIRST LIST OF DOCTORAL DISSERTATIONS IN POLITICAL ECONOMY IN AMERICAN UNIVERSITIES AND COLLEGES

The present list specifies doctoral degrees conferred during the academic year terminating June 1964, and theses undertaken in the same period.

### General Economics; Methodology

#### *Degrees Conferred*

JAMES F. HOFHEINZ, Ph.D. Texas 1964. The evolution of economic philosophy.

THOMAS O. NITSCH, Ph.D. Ohio State 1963. The redistributive impact of inflation: an historical and theoretical-methodological study.

#### *Theses in Preparation*

VINCENT J. TARASCIO, B.A. San Jose State 1961. Pareto's methodological approach to economics: a study in the history of the scientific aspects of economic thought. *Rice*.

### Price and Allocation Theory; Income and Employment Theory; History of Economic Thought

#### *Degrees Conferred*

CLEVELAND W. BATEMAN, Ph.D. Harvard 1964. Investment behavior and the acceleration principle.

ROBERT A. BERRY, Ph.D. Princeton 1963. Welfare implications of monopoly: a static analysis.

BYRON B. BROWN, JR., Ph.D. Houston 1964. An export-employer multiplier analysis of the Houston metropolitan area.

CARL CAMPBELL, Ph.D. California (Berkeley) 1964. Economic growth, capital gains, and income distribution: 1897-1956.

ROBERT J. CARLSSON, Ph.D. Rutgers 1964. Effects of the tax variable on the saving ratio in growth models.

ROBERT J. CHENEY, Ph.D. Georgetown 1964. Goetz Anthony Briefs on the history and development of modern capitalism: a critical examination.

KONG CHU, Ph.D. Tulane 1964. Computer simulation of certain stochastic relationships in micro-economic systems.

CHARLES L. COLE, Ph.D. Southern California 1963. Income distribution and the social welfare function: a study of theories of distributive justice.

JOHN G. CROSS, Ph.D. Princeton 1963. A theory of the bargaining process.

PETER L. DANNER, Ph.D. Syracuse 1964. An inquiry into the social aspects of Adam Smith's theory of value.

JOSEPH B. FETZER, Ph.D. Stanford 1963. An analysis of the applicability of certain optimal capital structure concepts to a selected group of domesticated oil companies.

CHARLES R. FRANK, Ph.D. Princeton 1963. Production theory and indivisible commodities.

GEORGE H. HAINES, JR., Ph.D. Carnegie Inst. Technology 1964. A study of innovation on nondurable goods.

RICHARD M. HALFYARD, Ph.D. California (Los Angeles) 1964. The determination of aggregate consumption expenditures in the United States: 1929-1961.

WIHIDUL HAQUE, Ph.D. Stanford 1964. Mathematical programming in allocation over time.

CHING-YAO HSIEH, Ph.D. George Washington 1964. Pre-Smithian macroeconomics.

MICHAEL D. INTRILIGATOR, Ph.D. Mass. Inst. Technology 1963. Essays on productivity and savings.

- RICHARD A. JENNER, Ph.D. Colorado 1964. Product competition in a neo-Marshallian model.
- JAMES A. JOHNSON, Ph.D. Minnesota 1963. The welfare effects of the Minnesota prohibition on the sale of colored margarine.
- STANLEY KAISEH, Ph.D. New York 1964. Levels of aspiration and the theory of economic behavior.
- PAUL W. KUZNETS, Ph.D. Yale 1964. Financial determinants of manufacturing inventory behavior.
- DAVID LEVHARI, Ph.D. Mass. Inst. Technology 1964. Essays on optimal economic growth.
- FERDINAND K. LEVY, Ph.D. Carnegie Inst. Technology 1964. An adaptive production function and its economic implications.
- ROBERT LUCAS, Ph.D. Chicago 1964. Substitution between labor and capital in U.S. manufacturing: 1929-1958.
- SHANE H. MAGE, Ph.D. Columbia 1963. The law of the falling tendency of the rate of profit—its place in the Marxian theoretical system and relevance to the U.S. economy.
- MICHAEL D. MCCARTHY, Ph.D. Southern Methodist 1964. Technical progress: a theoretical and empirical analysis of its effects on aggregate output and of its sources.
- JOHN J. MCGOVERN, Ph.D. Georgetown 1964. Philip Murray: an analysis of his activities and economic thought.
- KERMIT L. MOWBRAY, Ph. D. Kansas 1964. An examination of the employment and income multipliers.
- ALAN NICHOLS, Ph.D. Wisconsin 1963. An analysis of capital rationing in terms of cost of capital.
- NORMAN D. O'BANNON, Ph.D. Tulane 1964. The geographic association of manufacturing industries in standard metropolitan areas—1950.
- ALGIMANTAS PETRENAS, Ph.D. Columbia 1964. The effect of interdependence of consumption and the variability of incomes on estimated demand parameters.
- HELEN RAFFEL, Ph.D. Columbia 1963. A short run model of functional distribution, factor mobility and the rate of economic growth.
- CHARLES E. ROCKWOOD, Ph.D. Indiana 1963. Wage controls and the problem of inflation: a critical appraisal of the Lerner proposal.
- THADDEUS RODDENBERRY, Ph.D. Boston 1963. Technical change and the production function: disaggregation to the single firm.
- HOWARD N. ROSS, Ph.D. Columbia 1964. Theory and evidence of price flexibility.
- SCOTT E. SEAGER, Ph.D. Indiana 1963. Leverage and the cost of capital.
- ARJUN K. SENGUPTA, Ph.D. Mass. Inst. Technology 1963. Study in the constant-elasticity-of-substitution production function.
- JOSEPH SKEHAN, Ph.D. Georgetown 1964. Mid-twentieth century crisis in British socialism.
- RONALD SOLIGO, Ph.D. Yale 1964. The short-run relationship between employment and output.
- VINCENT D. TAYLOR, Ph.D. Mass. Inst. Technology 1964. Technical progress in two-sector models.
- HERBERT F. THOMPSON, JR., Ph.D. Colorado 1964. The emergence of political economy from the moral philosophy course in the Scottish universities in the eighteenth century.
- WILLIAM C. WHITTEN, JR., Ph.D. Alabama 1964. The economic and social ideas of Thomas Cooper.
- SUZANNE E. WIGGINS, Ph.D. Indiana 1963. A time series analysis of the demand for capital goods by U.S. manufacturing industries 1948-1960.
- GORDON C. WINSTON, Ph.D. Stanford 1964. Income and the allocation of effort.
- MURRAY WOLFSON, Ph.D. Wisconsin 1964. A critique of Marxian economics.

*Theses in Preparation*

- ANDREW R. BLAIR, B.S. Fordham 1959; M.A. 1961. Structural elements in the Brazilian inflation 1940-1960. *Fordham*.
- MICHAEL W. DAVENPORT, M.A. Edinburgh 1961. The corporate propensity to save. *Pennsylvania*.
- A. RODNEY DOBELL, B.A. British Columbia 1959; M.A. 1961. Heterogeneous labor and capital theory. *Mass. Inst. Technology*.
- ROBERT G. FABIAN, B.A. Notre Dame 1960; M.A. Illinois 1963. The development of a theory of economic policy. *Florida*.
- HAROLD N. FRIEDMAN, B.A. City (New York); M.A. Miami. The impact of John Kenneth Galbraith on selected areas of contemporary economic thought. *Alabama*.
- KENNETH GOLDIN, B.A. Princeton 1961; M.A. Stanford 1963. The optimum rate of savings. *Stanford*.
- STEVEN M. GOLDMAN, B.A. Harvard 1962; M.A. Stanford 1963. Technical change and economic growth. *Stanford*.
- SURAJ GUPTA, B.A. Panjab 1948; M.A. 1950. Changes in price level expectations and rates of interest. *Chicago*.
- GIORA HANOCH, B.A. Hebrew 1960. Cross section analysis of personal earnings. *Chicago*.
- LAWRENCE HEXTER, B.A. Minnesota 1954; M.B.A. Cornell 1958; M.A. Wisconsin 1964. A reinvestigation and evaluation of several capital good investment models. *Wisconsin*.
- JOHN L. IRELAND, B.S. Columbia 1957. The German historical school and American economic development. *Columbia*.
- THOMAS A. JENNINGS, B.A. Rice 1961. Productivity measurement in criteria of economic efficiency. *Tulane*.
- PETER J. KALMAN, B.A. City (New York) 1961; M.S. Purdue 1964. The theory of consumer behavior when prices enter the utility function. *Purdue*.
- THOMAS H. MAYOR, B.A. Rice. Strategic factors in the declining capital-output ratio. *Maryland*.
- JOHN L. McENTAFFER, B.A. Iowa (Ames) 1961; M.A. Boston College 1964. A theoretical and statistical analysis of the effects of changes in income distribution on U.S. consumption functions. *Boston College*.
- RODNEY MORRISON, B.S. Boston College 1959; M.A. 1961; M.S. Wisconsin 1964. Economic thought in the 19th century. The American school. *Wisconsin*.
- JAN MOSSIN, Dipl. Norwegian School of Econ. & Bus. Admin. 1959; M.S. Carnegie Inst. Technology 1964. Capital accumulation and general equilibrium. *Carnegie Inst. Technology*.
- DONALD MURRY, B.S. Missouri 1959; M.A. 1961. Indivisibilities in the spatial pattern of research. *Missouri*.
- ROBERT F. NEILL, S.T.B. St. Michael's 1959; M.A. Toronto 1960. The content and context of the writings of Harold Adams Innis. *Duke*.
- DAVID M. NOWLAN, B.S. Queen's (Canada) 1958; B.A. Oxford 1960; M.A. Toronto 1963. Innovation, growth, and employment. *Toronto*.
- SEONG Y. PARK, B.S. Illinois 1958; M.A. Yale 1961. Bounded substitution, fixed proportions, and economic growth. *Yale*.
- EDWARD J. POWERS, B.S. Boston College 1958. John Maurice Clark and the role of the state in economic affairs. *Boston College*.
- KARL RADOV, B.A. Oberlin 1959. The demand for gasoline. *Chicago*.
- RICHARD W. RUPPERT, B.A. Pomona 1959. Allocating an indivisible resources subject to interdependence among users. *Purdue*.

- T. PAUL SCHULTZ, B.A. Swarthmore 1961. The distribution of personal income in the Netherlands. *Mass. Inst. Technology*.
- RUSSELL D. SHANNON, B.A. Duke 1960. On the problems related to the concept and representation of the Keynesian aggregate supply function. *Tulane*.
- KARL SHELL, B.A. Princeton 1960. Invention and technical change. *Stanford*.
- CHANDLER H. STEVENS, JR., B.E.E. Georgia Inst. Technology 1956. Information and the division of labor—towards a general information theory of economic structure. *Mass. Inst. Technology*.
- PAUL J. TAUBMAN, B.S. Pennsylvania 1961. A synthesis of saving theory with special reference to the components of saving. *Pennsylvania*.
- ADOLF VANDENDORPE, Lic. Sc. Ec. Louvain 1961; Act. Sc. 1961. Dynamic growth theory in open economies. *Mass. Inst. Technology*.
- ANDREW G. VERZILI, B.A. St. Vincent 1961; M.A. Boston College 1964. Explanation of net new appropriations for expenditures on new plant and equipment for total manufacturing. *Boston College*.
- ALEXANDER VICAS, B.Comm. McGill 1960. The lag of unrestricted imitation behind product innovation. *Princeton*.
- JEAN-PIERRE M. WAMPACH, Agr. Eng. Louvain 1953; M.A. 1957. Measurement of technological change: a theoretical and practical investigation. *Cornell*.
- RICHARD C. WILES, B.A. Boston College 1957; M.A. 1958. Theories of economic development of the English mercantilists 1675-1775. *Clark*.
- BORIS YAVITZ, B.A. Cambridge 1943; M.S. Columbia 1948. The dynamics of technological change—some economic and organizational impacts. *Columbia*.

### Economic History; Economic Development; National Economies

#### *Degrees Conferred*

- RALPH ANSPACH, Ph.D. California (Berkeley) 1963. The problem of a plural economy and its effects on Indonesia's economic structure: a study in economic policy.
- ANTONIO AYALA, Ph.D. Georgetown 1964. The use of a policy model for analyzing some Philippine growth alternatives: 1963-1966.
- AARON BENDER, Ph.D. New York 1964. An analysis of the idea of progress and other interpretations of history evidenced in selected books on philosophy of history published in English, 1933-1957.
- LYMAN D. BOTHWELL, Ph.D. George Washington 1964. Capital formation in Puerto Rico, 1950-1960.
- RICHARD P. BRIEF, Ph.D. Columbia 1964. Nineteenth century capital accounting and business investment.
- ARMANDO DIAZ-ROJAS, Ph.D. New York 1964. Financing economic development in Puerto Rico 1942 to date.
- ROBERT W. EISENMENGER, Ph.D. Harvard 1964. The dynamics of economic growth in New England, 1870-1960.
- LLOYD J. ELLIOTT, Ph.D. Texas 1964. Economic maturity and regional unemployment: the cases of Pennsylvania and Texas.
- HENRY E. FINLEY, Ph.D. Indiana 1963. Some aspects of Indian economic planning.
- EDITH H. FLOYD, Ph.D. Harvard 1964. The Swedish full employment policy and economic development, 1945-52.
- EDWARD L. HOYT, Ph.D. Fletcher School 1964. The dilemma of Argentine development: Frondizi's economic policy, 1958-62.
- YAOUB KARKAR, Ph.D. Indiana 1964. Railway development in the Ottoman Empire 1856-1914: an economic interpretation.

- NORMAN H. LITHWICK, Ph.D. Harvard 1964. Economic growth in Canada—a quantitative analysis.
- FLOYD B. MCFARLAND, Ph.D. Texas 1964. An analysis of relationships between foreign economic policy and economic development in Mexico.
- KHAN A. MOHABBAT, Ph.D. New York (Buffalo) 1964. Disguised unemployment, its significance for economic development, and its effects on international trade theory.
- LLEWELLYN M. MULLINGS, Ph.D. Clark 1964. An analysis of economic implications of political independence for Jamaica.
- HONG-LAN OEI, Ph.D. Texas 1964. Petroleum resources and economic development: a comparative study of Indonesia and Mexico.
- JUAN P. PEREZ-CASTILLO, Ph.D. Tulane 1963. Some aspects of Venezuela's economic development: 1945-1960.
- BRAHAMANAND PRASAD, Ph.D. Tulane 1963. The Keynesian apparatus in Indian planning for capital formation.
- ENRIQUE A. ROLDAN, Ph.D. Minnesota 1963. A resource allocation problem in the economic development of Ecuador.
- WILLIAM F. RYAN, Ph.D. Harvard 1964. Economic development and the Church in French Canada, 1896-1914.
- LARS. G. SANDBERG, Ph.D. Harvard 1964. Swedish economic policy in theory and practice, 1950-1961.
- SAMUEL N. SEIDMAN, Ph.D. New York 1964. Enterprise and entrepreneurship in the Philippine Republic: 1949-1959.
- PRITAM SINGH, Ph.D. Michigan 1963. Types of entrepreneurship in Indian economic development.
- CHINNAWOOT SOONTHORNSEMA, Ph.D. Michigan 1963. A macroeconomic model for economic development of Thailand.
- LIONEL G. STOLERU, Ph.D. Stanford 1964. A quantitative model of growth of the Algerian economy.
- THANKAMMA C. VARKI, Ph.D. American 1964. Development banks and industrial financing in India since World War II.
- HARRISON G. WEHNER, Ph.D. Michigan 1964. The cocoa marketing board and economic development in Ghana: a case study.
- RICHARD B. WIRTHLIN, Ph.D. California (Berkeley) 1964. International trade and the economic development of the underdeveloped country: the Brazilian case.
- KUNG C. YEH, Ph.D. Columbia 1964. Capital formation in Mainland China: 1931-36 and 1952-57.

### *Theses in Preparation*

- JOHN Q. ADAMS, III, B.A. Oberlin 1960. International trade and India's changing economy, 1900-1960. *Texas*.
- JOSEPH ADEKUNLE, B.A. Ohio Wesleyan 1961; M.A. Wisconsin 1964. Structural change and private foreign investment: a study of the United Africa group in Ghana and Nigeria, 1948-1962. *Wisconsin*.
- NORMAN D. AITKEN, B.A. Cincinnati 1961. An analysis of potential entrepreneurs in underdeveloped countries. *Tennessee*.
- LOUIS P. BELEKY, B.A. New York (WSC) 1959. The organization state—a study in organization and economic development. *New York*.
- KENNETH BROWN, B.A. Notre Dame 1961. Regional economic growth. *Johns Hopkins*.
- MELVIN BURKE, B.A. Wayne State 1960; M.A. 1962. Land reform and economic development: a case study in the Andes. *Pittsburgh*.
- WILLIAM CASSELS, B.A. Oberlin 1953. National income aggregates in underdeveloped countries. *Princeton*.

- JOHN K. CHANG, B.A. Illinois 1958; M.A. Maryland 1960. An index of industrial production of mainland China, 1912-1949. *Michigan*.
- PAUL L. CHEN-YOUNG, B.A. Howard 1961; M.A. Pittsburgh 1962. Tax incentives and economic development: the Jamaican experience. *Pittsburgh*.
- ROBERT E. COLE, M.A. Connecticut 1956. The economics of privileges. *Northwestern*.
- EL SAYED A. H. EL DALY, B.Com. Cairo. 1940; M.Com. 1951; M.S. Rutgers 1963. Economic development—comparison of Egypt and a Latin American country. *Rutgers*.
- WILLIAM O. FREITHALER, B.A. Texas 1961; M.A. Michigan 1962. The balance of payments and economic developments in Mexico. *Michigan*.
- DAVID E. GOODMAN, B.Sc. London School of Economics 1959. Economic growth and inflation in Latin America since 1945. *California (Berkeley)*.
- LOUIS G. GUADAGNOLI, B.S. Holy Cross 1951; M.S. Wisconsin 1955. Effects of export credit guarantees on developing countries, Argentina as a case study. *American*.
- RAYNAL L. HAMMELTON, B.S. California 1953; M.A. Southern California 1961. New port cities in Western America and their effect on economic development. *Southern California*.
- SAMUEL P. S. HO, B.S.E. Princeton 1959; M.A. Yale 1961. Development alternatives—the case of Taiwan. *Yale*.
- YOUNG C. KIM, B.A. Seoul 1960; M.A. Boston College 1962. Economic criteria for a choice among different methods of production in underdeveloped countries. *Columbia*.
- CHARLES A. LAVE, B.A. Reed 1960. Econometric model of the United Kingdom 1870-1914. *Stanford*.
- YOUNGIL LIM, B.A. Harvard 1958; M.A. California (Los Angeles) 1963. Impact of export industries on pattern of economic growth: a study of indirect effect in Ceylon. *California (Los Angeles)*.
- JOHN H. LORANT, B.S. Wisconsin 1953; M.B.A. Harvard 1957. The surge in American productivity during the 1920's. *Columbia*.
- RAYMOND MAGLOIRE, B.S. Columbia 1960; M.B.A. 1960. Development problems of a small country: the case of Gabon. *Columbia*.
- ANDREA MANESCHI, B.A. Oxford 1958; Dip. Econ. 1959. Linear and dynamic programming approaches to economic development. *Johns Hopkins*.
- PETER D. MCCLELLAND, B.Com. Queen's (Canada) 1957; M.A. 1961. Development problems in the Canadian maritime provinces, 1867-1911. *Harvard*.
- THOMAS A. MCCOWEN, B.S. Illinois. Non-economic factors that inhibit development. *Illinois*.
- ALBERTO O. PETRECOLLA, Pub. Accountant Buenos Aires 1957. The delay of industrialization in Argentina, 1915-1935. *Columbia*.
- MARY I. PETT, B.A. Brown 1956. Critique of economic development models using the Latin American experience. *Brown*.
- MARTIN PFAFF, M.B.A. Pennsylvania 1963. Marketing as a factor in economic development—India, a case study. *Pennsylvania*.
- MIGUEL RAMIREZ-PEREZ, B.A. Puerto Rico 1957; M.A. Syracuse 1958. Economic development of the Mississippi. *Rutgers*.
- IDRIAN RESNICK, B.A. Clark 1958; M.A. Boston 1960. Human capital and the classical theory of economic development. *Boston*.
- LAHOMA RIEDERER, B.B.A. Baylor 1955; M.B.A. Southern Methodist 1958. The corporation in Florida's economic development, 1870-1963. *Florida*.
- EDWARD STARR, B.B.A. City (New York) 1959. Inflation and economic development. *Minnesota*.
- ROBERT P. THOMAS, M.A. Northwestern 1964. The automobile in America: an historical analysis. *Northwestern*.
- MANUEL VELES-MONTES, B.S. Columbia 1960; M.A. 1956. Strategic factors governing

- the economic growth of underdeveloped regions (a theoretical examination). *Rutgers*.
- BRENDAN M. WALSH, B.A. Dublin; M.A. Tennessee 1962. Efficient programs of capital accumulation in the Irish economy. *Boston College*.
- CALMAN R. WINEGARDEN, B.S. American 1948; M.A. Western Reserve 1963. Balanced and imbalanced approaches to the theory of economic development. *Western Reserve*.
- KAN-HUA YOUNG, B.A. National Taiwan 1959; M.A. Columbia 1963. Optimal growth in a planned economy: an appraisal of the economic policy of Communist China. *Columbia*.
- ALBERT ZUCKER, B.S. City (New York) 1952. Productivity growth in manufacturing: 1910-1929. *Columbia*.

### Statistical Methods; Econometrics; Social Accounting

#### *Degrees Conferred*

- TAKESHI AMEMIYA, Ph.D. Johns Hopkins 1964. Specification analysis in econometrics.
- WILLIAM A. CHANCE, Ph.D. Kansas 1964. Two computer models of economic activity, with a test of model II against the performance of the United States economy in the period 1951-1959.
- GERALD L. CHILDS, Ph.D. Mass. Inst. Technology 1963. Linear decision rules for explaining finished goods inventories and unfilled orders.
- WILFORD L'ESPERANCE, Ph.D. Michigan 1963. The predictive ability of various estimators.
- WILLIAM J. LEININGER, Ph.D. Purdue 1963. An empirical production function for barge towing operations on the Ohio River.
- ABDUL H. MADJID, Ph.D. Harvard 1964. A dynamic input-output model incorporating technical change.
- YVES NANOT, Ph.D. California (Los Angeles) 1964. An experimental investigation and comparative evaluation of priority disciplines in job shop-like queueing networks.
- DONALD S. ORKAND, Ph.D. New York 1963. Some techniques for the statistical management of inventive contracts.
- MATTHEW J. STEPHENS, JR., Ph.D. Pennsylvania 1964. Proposals for supplementary financial statements, their purpose and their relationship to underlying accounting concepts and standards and income and capital theory.
- RAYMOND SWENSON, Ph.D. Chicago 1963. Economic models for design tolerance.
- PAUL VAN MOESEKE, Ph.D. Yale 1964. Stochastic linear programming.
- LEON L. F. WEGGE, Ph.D. Mass. Inst. Technology 1963. On binomial lag distributions.

#### *Theses in Preparation*

- HENRY J. BOISSEAU, JR., B.S. Tulane 1958; M.B.A. 1960. Dynamic programming and Markovian decision processes. *Purdue*.
- MARIO S. BRODESOHN, B.A. Buenos Aires 1961. Multiregional input-output analysis of Argentina. *Harvard*.
- WILLIAM J. CARROLL, B.A. Catholic 1954; M.B.A. Detroit 1957. Application of an inspection scheme for attributes. *New York*.
- MICHAEL A. H. DEMPSTER, B.A. Toronto 1961; M.S. Carnegie Inst. Technology 1963. On stochastic programming. *Carnegie Inst. Technology*.
- WILLIAM HOEHN, JR., B.S. Bethany 1960; M.A. Northwestern 1963. Simultaneous interpolation and estimation in simultaneous equation systems, and comparison with alternative estimation procedures. *Northwestern*.
- ROBERT E. JENSEN, B.S. Denver 1960; M.B.A. 1961. Pattern classification in an adoptive prediction model for forecasting in business and preparation of budgets. *Stanford*.
- ROBERT C. LIND, B.A. Yale 1960. Selected topics in time series. *Stanford*.

- ASATOSHI MAESHIRO, B.A. Hitotsubashi 1958. Comparison of the forecasting properties of a large econometric model fitted by alternative statistical techniques. *Michigan*.
- ROBERT R. MCKENZIE, B.B.A. Minnesota 1955; M.B.A. California (Los Angeles) 1959. A computer simulation to test alternative financial hypotheses. *California (Los Angeles)*.
- JOSEPH MIDLER, B.A. Chicago 1956; M.A. 1959. A dynamic portfolio selection model. *Wisconsin*.
- JOHN R. NORSWORTHY, B.A. Virginia 1961. Error analysis of Leontief input-output system. *Virginia*.
- ROBERT W. PRATT, JR., B.B.A. Texas 1953; M.B.A. Michigan 1959. Mail questionnaires in marketing research. *Michigan*.
- DONALD B. RICE, JR., B.S.Ch.E. Notre Dame 1961; M.S.I.M. Purdue 1962. Discrete optimizing solutions to integer programming problems. *Purdue*.
- HUGO F. SONNENSCHNEIN, B.S. Rochester 1961; M.S. Purdue 1963. Transitive preferences and the structure of the choice space. *Purdue*.
- HELEN B. STONE, B.A. Randolph-Macon 1957; M.A. Yale 1958. Alternative perpetual inventory estimates of the capital stock of the United States. *Yale*.
- ATSUSHI SUZUKI, B.A. Knox 1960; M.S. Purdue 1962. An econometric model of fluctuations in economic time series. *Purdue*.
- THAYER H. WATKINS, B.S. Colorado 1961. Principal component analysis of national income accounts. *Colorado*.

### Economic Systems; Planning and Reform: Cooperation

#### *Degrees Conferred*

- MICHAEL BORETSKY, Ph.D. Columbia 1964. The Soviet challenge to U. S. machine building: a study in production and technological policy.
- RONALD K. CALGAARD, Ph.D. Iowa 1963. Economic planning in underdeveloped countries.
- AHMED A. M. GOUELI, Ph.D. California (Berkeley) 1964. Economic planning of production and marketing facilities.
- ABDELAZIZ HAMZAOUTI, Ph.D. Fletcher School 1964. Independent Tunisia: economic planning and growth, analysis and appraisal.
- I. S. KOROPECKYJ, Ph.D. Columbia 1964. The economics of investment in Ukrainian industry, 1928-1937.
- NEAL B. LONG, JR., Ph.D. Indiana 1964. An input-output comparison of the United States and Soviet industrial structure.
- GORTI V. K. NARASIMHAM, Ph.D. Pittsburgh 1964. Economic models of growth and planning.
- JAMES R. O'CONNOR, Ph.D. Columbia 1964. The political economy of pre-revolutionary Cuba.
- DWIGHT H. PERKINS, Ph.D. Harvard 1964. Price formation in Communist China.
- NARASIMHA SRINIVASARAOHAVAN, Ph.D. Pittsburgh 1964. Stochastic linear programming applied to development planning in India.
- E. C. H. VEENDORP, Ph.D. Rice 1963. Decentralization in resource allocation.
- JOHN A. WEIR, Ph.D. Notre Dame 1964. Rural reconstruction in Prince Edward Island: an evaluation.

#### *Theses in Preparation*

- M. C. CHALLAH, B.A. Manchester 1958; M.B.A. Oklahoma 1961. Economic development and planning in Syria. *Oklahoma*.
- ARUN K. DATTA, B.A. Calcutta 1947; M.A. 1950. Growth models and economic planning: a case study of India. *New York (Buffalo)*.
- LORAINNE DONALDSON, B.S.B.A. Florida 1960; M.A. 1961. Government control and direction of investment in Irish development planning. *Indiana*.

- NICHOLAS HERNANDEZ, B.A. Puerto Rico 1957; M.A. Rutgers 1963. The entrepreneurial function of the government of Puerto Rico. *Rutgers*.
- NOKYOON KWAK, B.A. California Western 1956; M.A. California 1958. A comparative appraisal of development planning in India and Communist China under the second five year plans. *Southern California*.
- EMANUEL LANDAU, B.S. City (New York) 1939. The public control of air pollution: a case study. *American*.
- WAYTIN MALLIKAMAS, B.A. Chulalongkorn 1952; B.Econ. Thammasat 1956; M.S. Wisconsin 1961. An economic analysis and application for planning. *Wisconsin*.
- JEFFREY NUGENT, B.A. Amherst 1957; M.A. New School 1961. Programming the optimal development of the Greek economy, 1954-61 and 1961-75. *New School*.
- DAVID A. OLSON, B.S. Northwestern 1959; M.A. Michigan 1962. Investment criteria in the Soviet coal industry. *Michigan*.
- V. G. PATEL, B.A. Gujarat 1959; M.S. Wisconsin 1960. Problems of plan implementation—concentration on Indian five year plans. *Wisconsin*.
- MARVIN M. PHAUP, JR., B.A. Roanoke 1962. Constraints on planning in an open economy (Norway). *Virginia*.
- RALPH G. M. SULTAN, B.A. British Columbia 1956; M.B.A. Harvard 1960. Sales forecasting of consumer durables at the level of the firm. *Harvard*.
- UWE JAN WOLTEMADE, B.A. Kansas City 1961; M.A. Texas 1964. Economic change and the emergence of a market economy in India. *Texas*.

### Business Fluctuations and Forecasting

#### *Degrees Conferred*

- SHIRLEY A. ALMON, Ph.D. Harvard 1964. The distributed lag between capital appropriations and expenditures.
- NICHOLAS BALOFF, Ph.D. Stanford 1963. Manufacturing startup: a model.
- MEGHANAD J. DESAI, Ph.D. Pennsylvania 1964. An econometric model of the world tin market 1948-1961.
- PETER EILBOTT, Ph.D. Columbia 1964. Effectiveness of automatic stabilizers.
- SAUL H. HYMANS, Ph.D. California (Berkeley) 1964. American business cycles: interwar and postwar.
- ALLEN C. KELLEY, Ph.D. Stanford 1964. The existence and nature of long swings in Australian output, investment and related variables, 1860-1930.
- KURUGANTI KRISHNAMURTY, Ph.D. Pennsylvania 1964. An econometric model of India, 1948-1961.
- HARVEY A. KROW, Ph.D. Pittsburgh 1964. Forecasting stock market fluctuations by technical methods.
- KANTA K. MARWAH, Ph.D. Pennsylvania 1964. An econometric model of price behavior in India.
- LERAY R. MCGEE, Ph.D. Tulane 1963. The cyclical behavior of income and employment in the Southeast: 1945-1961.
- GEORGE VIKSNINS, Ph.D. Georgetown 1964. Seasonal movements in the flow of funds.
- RAYMOND E. WILLIS, JR., Ph.D. Mass. Inst. Technology 1964. Study of the relationships between fluctuations in the steel and related industries.
- HENRY L. WOJTYLA, Ph.D. Chicago 1963. Cyclical interrelationships in the postwar period.

#### *Theses in Preparation*

- DAVID A. BELSLEY, B.A. Haverford 1961. Inventory and production decision. *Mass. Inst. Technology*.

- DOUGLAS A. CAMPBELL, B.A. McGill 1951; M.B.A. Harvard 1953. The diffusion index as a tool for predicting stock market fluctuations. *Columbia*.
- PETER C. DOOLEY, B.A. Grinnell 1959. The upper turning points of 1953, 1957, and 1960. *Cornell*.
- SONIA T. MEJIA, B.A. Regis (no year given). A comparison of pre and post World War II business cycles in the United States. *Boston College*.
- JANET R. PACK, B.B.A. City (New York) 1959. Economic prerequisites to disarmament. *California (Berkeley)*.
- WILLIAM C. PERKINS, B.S. Rose Polytechnic Inst. 1960; M.B.A. Indiana 1962. A simulation analysis of the automatic stabilizers. *Indiana*.
- JAMES A. REFFNER, B.S. Wisconsin State (Stevens Point) 1961; M.A. Wisconsin 1964. Forecasting market demands in underdeveloped countries with applications to East Africa. *Wisconsin*.
- BONG J. SHIN, B.A. Pusan 1956; M.A. Ohio State 1963. A study of cyclical fluctuations and sensitivity in the Ohio economy, 1947-1963. *Ohio State*.
- RONALD SIMON, B.A. Harvard 1959; M.A. Columbia 1962. Uncertainty and business growth: a financial analysis. *Columbia*.

### Money, Credit and Banking; Monetary Policy; Consumer Finance and Mortgage Credit

#### *Degrees Conferred*

- STANLEY M. BESEN, Ph.D. Yale 1964. An empirical analysis of commercial bank lending behavior.
- LAWRENCE F. BRAUN, JR., Ph.D. Northwestern 1964. Implications of problems of monetary stabilization in the United Kingdom in 1961 for regulation of borrowing by British sales finance companies.
- HAROLD L. BRIDE, Ph.D. Oregon 1963. Money and credit expansion, inflation and economic development: an application of monetarist and structural doctrines to India and Pakistan.
- ROBERT J. B. BROWN, Ph.D. New York 1964. Currency revaluation.
- HANG S. CHENG, Ph.D. Princeton 1963. A theory of the optimal amount of foreign reserves of a central bank.
- JOSEPH L. CRAYCRAFT, Ph.D. Cincinnati 1964. The effect of Federal Reserve policy on the cyclical movement of the money supply.
- FRANKLIN R. EDWARDS, Ph.D. Harvard 1964. Concentration and competition in commercial banking: a statistical study.
- EDUARDO GARCIA D'ACUNA, Ph.D. Mass. Inst. Technology 1964. Inflation in Chile, a quantitative analysis.
- WILLIAM D. GEER, D.B.A. Indiana 1964. Problems in the development of instalment lending by Alabama commercial banks.
- STEPHEN M. GOLDFELD, Ph.D. Mass. Inst. Technology 1963. Commercial bank behavior and the level of economic activity: an econometric study.
- STUART GREENBAUM, Ph.D. Johns Hopkins 1964. Banking structure and costs: a statistical study of the cost-output relationship in commercial banking.
- MICHAEL J. HAMBURGER, Ph.D. Carnegie Inst. Technology 1964. The demand for money by households.
- ERNEST KOHN, Ph.D. Columbia 1964. Branch banking, bank mergers and the public interest.
- CHARLES P. LEON, Ph.D. New York 1964. The National Bank of Cuba: a study in institutional change.
- ABDUL MALIK, Ph.D. American 1964. Inflation and monetary policy in Turkey: 1950-1961.

- GEORGE S. MEDAWAR, Ph.D. Cornell 1963. Monetary policy in Lebanon.
- SAID NABULSI, Ph.D. Georgetown 1964. Problems of monetary integration in underdeveloped countries: a case study of the Syrian-Egyptian Union, 1958-1961.
- KHIN NYO-NYO, Ph.D. Toronto 1963. Central banking in South-East Asia.
- SATISECHANDRA PAREKH, Ph.D. New York 1963. Problems of agricultural credit in India.
- JOHN R. PIKE, Ph.D. Wisconsin 1964. The structure and adequacy of Wisconsin commercial banking.
- JONAS PRAGER, Ph.D. Columbia 1964. Decision-making in British monetary policy, with implications for the United States.
- VICENTE U. QUINTANA, Ph.D. Ohio State 1964. An economic analysis of straight agricultural loans granted by the branches of the Development Bank of the Philippines in Nueva Ecija and Isabela, 1962.
- CHARLES L. THIEMANN, D.B.A. Indiana 1964. The Bank Merger Act of 1960.
- RICHARD TILLY, Ph.D. Wisconsin 1964. Financial intermediaries and the development of the Rhineland, 1815-1870.
- BASILIOS TSAGRIS, Ph.D. Southern California 1964. The economic impact of savings and loan associations on residential construction.
- GERALD I. WEBER, Ph.D. California (Los Angeles) 1964. An analysis of the Canadian money supply system.
- EDWARD L. WHALEN, Ph.D. Princeton 1963. A statistical study of the inventory approach to the transactions and precautionary demand for cash.
- JEAN WILBURN, Ph.D. Columbia 1964. Support of the Second U.S. Bank.

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- BERNARD E. ANDERSON, B.S. Arizona State 1960; M.A. Arizona 1961. An investigation into the effect of banking structures on aspects of bank behavior. *Ohio State*.
- JOHN W. ANDERSON, B.A. Amherst 1956. Influence of the balance of payments on Federal Reserve policy. *Pennsylvania*.
- PHILOCTES D. ASSIMAKIS, Dip. Law, Athens 1957. Long-term finance in an underdeveloped economy (emphasis on Greece). *Cornell*.
- JOSEPH H. AUGUSTA, B.A. California 1959; M.A. Michigan State 1960. A quarterly analysis of the corporate demand for money. *Maryland*.
- WILLIAM BEAZER, M.S. Johns Hopkins 1961. Optimization of bank portfolios and response to monetary policy. *Northwestern*.
- GEORGE J. BEIER, B.A. Kansas 1960. The cash balance position of firms. *Washington*.
- ALLAN E. BERGLAND, B.S. Concordia 1957; M.B.A. Arizona 1959. The role of the Dallas Federal Reserve Bank in the Eleventh Federal Reserve District 1950-1960. *Arizona*.
- JOHN O. BORNHOFFEN, B.A. Illinois 1961; M.S. 1963. The effect of banklines of credit on corporate holdings of secondary reserves. *Illinois*.
- PATRICIA F. BOWERS, B.A. Cornell 1950. The influence of the Federal Reserve Bank of New York upon decision-making in the Federal Reserve system. *New York*.
- ROBERT D. BOWERS, B.A. Albright 1956; M.A. Pennsylvania State 1960. Criteria for new commercial bank charters. *Western Reserve*.
- WILLIAM J. BREEN, B.A. Ripon 1959. The demand for cash and other assets: an econometric study. *Cornell*.
- JAMES A. CACY, B.S. Oklahoma State 1959; M.S. 1960. A study in the theory of banking—an application of portfolio investment theory to banks. *California (Berkeley)*.
- JAMES W. CHRISTIAN, B.A. Texas 1962; M.A. 1964. Monetary policy for growth without inflation. *Texas*.
- BRUCE C. COHEN, B.S. Tufts 1960; M.S. Purdue 1962. Bank deposit growth by ownership type: 1951-1961. *Purdue*.

- DAVID W. COLE, B.A. Capital 1960; M.B.A. Ohio 1962. Senior securities in the capital structures of commercial banks. *Indiana*.
- DWIGHT B. CRANE, B.S. Mass. Inst. Technology 1959; M.B.A. Michigan 1961; M.S. Carnegie Inst. Technology 1964. Banking competition and the deposits of business firms. *Carnegie Inst. Technology*.
- GEOFFREY CROFTS, B.Comm. Manitoba 1946. Problems of non-proportional reinsurance. *California (Los Angeles)*.
- GEORGE E. DICKEY, B.A. Johns Hopkins 1961; M.A. Northwestern 1964. Money, prices and growth: the American experience 1869-1896. *Northwestern*.
- JOHN A. DOUKAS, B.A. Dartmouth 1949; M.S. Purdue 1962. The relationship of interest rate differentials to commercial bank investment portfolios. *Purdue*.
- DANIEL M. DRISCOLL, JR., B.A. Rutgers 1959; M.A. 1961. The determinants of the Canadian money stock 1867-1960. *Brown*.
- RODNEY ELDRIDGE, B.A. Vermont 1949; M.A. 1959. Determinants of the volume of banker's acceptance financing. *Columbia*.
- PETER A. FROST, B.A. Occidental 1959; M.A. California (Los Angeles) 1961. The banks' demand for cash assets. *California (Los Angeles)*.
- DAVID T. GEITHMAN, B.S.B.A. Florida 1960; M.A. Florida 1962. Money and prices in Colombia, 1950-1960. *Florida*.
- VICTOR B. GESTRIN, B.A. Toronto 1960; M.A. 1963. Canadian banking development 1900-1920. *Toronto*.
- EMILE GHATTAS, B.B.A. American (Beirut) 1956; M.B.A. 1959. The functioning of the banking system of Lebanon. *Columbia*.
- GEORGE D. GREEN, B.A. Stanford 1961; M.A. 1963. Banking and finance in ante-bellum Louisiana (1812-1861): their influence on the course of economic development. *Stanford*.
- HERSCHEL I. GROSSEMAN, B.A. Virginia 1960; B. Phil. Oxford 1962. Public assets and liabilities, private saving, and commercial banking, in a macro-economic model. *Johns Hopkins*.
- AL GUTOWSKY, B.A. Denver 1959; M.A. Oregon 1961. Structure, performance, and competition of Oregon commercial banking. *Oregon*.
- PHILIP J. HAHN, B.S. Juniata 1938; M.B.A. Harvard 1940. Factors determining capital adequacy of commercial banks. *Western Reserve*.
- BEULA W. HAMM, B.B.A. Houston 1958; M.B.A. 1960. History of classical monetary theory. *Houston*.
- ALBERT G. HEEBNER, B.A. Denver 1948. Time and business deposits. *Pennsylvania*.
- PATRIC H. HENDERSHOTT, B.A. St. Mary's 1961; M.S. Purdue 1963. Monetary policy 1951-63. *Purdue*.
- RICHARD J. HERDER, B.S. Minnesota 1958; M.S. 1959. An analysis of country banking in the Ninth Federal Reserve District. *Minnesota*.
- RICHARD HIGGINS, B.A. Tufts 1956. Differentiation of function vs. standardization of operations: a case study in branch banking. *Columbia*.
- WILLIAM H. JEAN, B.S. Kansas 1957; M.A. 1960. A quantitative study of Federal Reserve management of banking system reserve capacity. *Purdue*.
- DAVID H. KAIN, B.A. Haverford 1959. A study of commercial bank behavior in the municipal bond market: 1962-1963. *Michigan*.
- MICHAEL W. KERAN, B.B.A. Minnesota 1955; M.A. 1958. An appraisal of post-war Japanese monetary policy. *Minnesota*.
- HARLAN N. KILMER, B.A. Kansas 1949; M.B.A. Northwestern 1958. An analysis of concentration of residential mortgage foreclosures in Cook County, Illinois. *Northwestern*.
- ROBERT W. KING, B.A. Indiana 1959. The cyclical and secular impact of financial intermediaries. *Purdue*.

- MICHAEL A. LEBOWITZ, B.S. New York 1960. Sound banking and developing regions: New York State in the 1830's. *Wisconsin*.
- CHARLES M. LINKE, B.S. Indiana 1960; M.B.A. 1961. A study of time and savings deposits in Indiana commercial banks, 1957-1963. *Indiana*.
- FREDERICK V. LOUD, B.A. Harvard 1929; M.A. Columbia 1957. Devaluation of the Israeli pound. *Columbia*.
- JOSEPH L. LUCIA, B.A. Temple 1954. Commercial bank earnings. *Pennsylvania*.
- PATRICK J. LYNCH, B.S. St. Joseph 1958. Behavior of commercial bank portfolios: an inventory model. *Purdue*.
- EARL L. MARTINSON, B.A. San Diego State 1951; M.B.A. California (Los Angeles) 1952. Management organization in savings and loan associations: a case study. *California (Los Angeles)*.
- CARLOS MASSAD, Grad. Chile 1954; M.A. Chicago 1958. The implicit monetary policy rules in the Chilean economy: a study of the supply of money. *Chicago*.
- RICHARD L. MOORE, B.A. Claremont 1955; M.B.A. California (Berkeley) 1956. The California savings and loan associations: the implications for public policy and managerial decision. *Claremont*.
- JOHN R. MORRIS, JR., B.A. Cornell 1961; M.S. Purdue 1963. Monetary policy and economic growth: a comparative study of six countries. *Purdue*.
- WILLIAM G. NELSON, B.A. Swarthmore 1956; M.B.A. Pennsylvania 1958. Impact of Federal Reserve policies on financial intermediaries. *Rice*.
- ANTON NISSEN, B.A. Michigan State 1960; M.A. 1961. Measuring economies of scale in commercial banking. *Princeton*.
- RONALD L. OLSON, B.S. Shippensburg State 1960; M.B.A. Indiana 1962. Profit planning in commercial banks. *Indiana*.
- ALFRED L. PARKER, B.S. Oklahoma State 1957; M.S. 1960. An analysis of the determinants of the cyclical and secular velocity of money. *Ohio State*.
- SAM PELTZMAN, B.B.A. City (New York) 1960. Entry in commercial banking. *Chicago*.
- MARJORIE P. PERSON, B.S. N. W. Missouri State 1940; M.B.A. Indiana 1956. Marketing and commercial banks. *Indiana*.
- CHAKRAVARTHI RANGARAJAN, M.A. Banaras 1957. Deposit change. *Pennsylvania*.
- CHARLES SCHOTTA, JR., B.A. Texas Christian 1957; M.A. Brown 1959. Money and income in an export economy: Mexico 1938-62. *Brown*.
- MARTHA R. SEGER, B.B.A. Michigan 1954; M.B.A. Michigan 1955. Postwar bank mergers in Michigan. *Michigan*.
- HAROLD SHAPIRO, B.Comm. McGill 1956. Canadian monetary sector, econometric analysis. *Princeton*.
- GLORIA SHATTO, B.A. Rice 1954. Corporate demand for liquid assets. *Rice*.
- LARRY R. SHOTWELL, B.A. Bowling Green State 1960; M.A. Ohio State 1963. An analysis of velocity and liquidity in the post war period: 1951-1963. *Ohio State*.
- GORDON R. SPARKS, B.A. Toronto 1961; M.A. Michigan 1962. Policy multipliers and the monetary sector of the U.S. economy. *Michigan*.
- EDWARD SYRING, B.S. Oregon 1960; M.A. 1962. Demand for money in Norway. *Oregon*.
- H. ANTON TUCHER, B.A. Colorado 1958; M.B.A. Harvard 1960. Commercial banks and public financial institutions. *Harvard*.
- THOMAS VELK, B.S. Wisconsin 1960. Bank merger structure—the policy problem. *Wisconsin*.
- ROBERT C. VOGEL, B.A. Amherst 1960; M.A. Stanford 1963. Financial activity of non-financial corporations and its implications for monetary economics. *Stanford*.
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- KHAMIS A. ABDUL-MAGID, Ph.D. Pennsylvania 1963. The role of monetary and fiscal policy in the economic development of Egypt (U.A.R.).
- ZIAUDDIN AHMAD, Ph.D. Harvard 1964. Deficit financing supply response and inflation in underdeveloped countries.
- WILLIAM H. ANDERSON, Ph.D. Florida 1964. Tax-burden implications of some tax-law deviations from the accretion concept of income.
- GORDON E. BELL, Ph.D. Florida 1964. Identification and evaluation of standards for administration of local government financial affairs—with case studies of the states of Georgia and North Carolina.
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- LATTEE A. FAHM, Ph.D. Mass. Inst. Technology 1963. Study in economic-functional analysis of government spending: Nigeria 1951-1960.
- RICHARD W. FORTNER, D.B.A. Indiana 1964. The tax treatment of capital gains and losses under the federal statute.
- ANN F. FRIEDLAENDER, Ph.D. Mass. Inst. Technology 1964. Interstate highway system: a study in public investment.
- W. IRWIN GILLESPIE, Ph.D. Johns Hopkins 1963. The effect of public expenditures on the distribution of income: an empirical investigation.
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- WILLIAM HENDON, Ph.D. Oklahoma 1964. Property assessment in Fort Worth.
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- DAVID E. W. LAIDLER, Ph.D. Chicago 1964. The income tax incentive to owner-occupation of housing.
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- ROBERT W. RAFUSE, JR., Ph.D. Princeton 1963. State and local fiscal behavior over the postwar cycles.
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- JOHN TREACY, Ph.D. Tulane 1963. The flexibility of federal expenditures over the 1957-1958 recession.
- ROBERT C. VOWELS, Ph.D. American 1964. An evaluation of equity and economic effects in capital gains taxation.

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- JANE A. LEAN, B.A. Brown 1959; M.A. Michigan 1962. The economic effects of the property tax on areas of substandard housing in major metropolitan areas of the United States. *Michigan*.
- HENRY M. LEVIN, B.S. New York 1960; M.A. Rutgers 1962. Estimating the demand for municipal goods: open space—a case study. *Rutgers*.
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- NOLIN MASIH, B.A. Southwestern 1950; M.A. Kansas 1953. Industrial development bond financing. *Kansas*.
- JEROLD J. MORGAN, B.S. Mississippi Southern 1951; M.B.A. Houston 1960. Post auditing in state and local government in Oklahoma and Kansas. *Alabama*.
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- JACK W. OSMAN, B.S. Rutgers 1961; M.A. 1963. An analysis of state and local combined government expenditures: 1957-63. *Rutgers*.
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- LON POLK, B.A. San Jose State 1954. Effects on the marginal rate structure of reforms in the federal individual income tax. *Michigan*.
- ALLAN G. PULSIPHER, B.A. Colorado 1961. An evaluation and extension of some models of public budget determination. *Tulane*.
- JAMES A. SAWYER, B.A. Arizona 1960; M.A. Illinois 1962. The economic effects of the excise tax. *Illinois*.
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- LEE J. SEIDLER, B.A. Columbia 1956; M.S. 1957. European selective taxation devices: prototypes for U.S. legislation? *Columbia*.
- HIROFUMI SHIBATA, B.A. Kobe 1953; M.A. McGill 1962. Public finance harmonization in a free trade area. *Columbia*.
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- GARY W. SORENSON, B.A. Fresno State 1961. An alternative personal income tax for California. *Claremont*.
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- JOE A. THORNE, B.S. Tennessee Polytechnic Inst. 1956; M.B.A. Alabama 1962. The influence of federal income taxation on accounting practices and reports. *Alabama*.
- PETER TINSLEY, B.A. Hobart 1960. Impact of the federal budget: stabilization. *Princeton*.
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- MERLE I. YAHR, B.A. New York 1960. Financial policy and the U.S. balance of payments: an exercise in computer stimulation. *Columbia*.
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- ALAN P. CARLIN, Ph.D. Mass. Inst. Technology 1964. Evaluation of U.S. government aid to India.
- BENJAMIN I. COHEN, Ph.D. Harvard 1964. A study of the export policies of the Indian Government, 1951-52 to 1965-66.
- EVANGELOS A. DEVLETGLOU, Ph.D. Columbia 1963. Devaluation and the trade balance.
- OSMAN A. EL-KHOLEI, Ph.D. California (Berkeley) 1964. An econometric analysis of the impact of the European Common Market on the economy of the United Arab Republic.
- PETER W. FREVERT, Ph.D. Purdue 1964. A model of the spot and forward markets for foreign exchange.
- FRED R. GLAHEE, Ph.D. Purdue 1964. The behavior of professional speculators during a period of flexible exchange rates.
- GLENN C. HIMES, Ph.D. Ohio State 1964. International competition in the feed grain economy.
- JAN KMENTA, Ph.D. Stanford 1964. Economic effects of post-war immigration in Australia.
- DIONYSIOS S. KOTSONIS, Ph.D. Columbia 1964. The devaluation of the Greek drachma in 1953.
- JOHN D. LAFKY, Ph.D. Texas 1964. Silver: national and international problems.
- JAMES W. LAND, Ph.D. Princeton 1963. Factor-price equalization: an extension of a theorem in the pure theory of international trade.
- JULES N. LA ROCQUE, Ph.D. Iowa 1964. The Alliance for Progress.
- JAN B. LUTJES, Ph.D. Pennsylvania 1963. The economic effects of Indonesia's export regulations upon its domestic export industries.
- NABILA A. MALIK, Ph.D. American 1964. Impact of terms of trade on the balance of payments—the case of Egypt: 1920-1963.
- RICHARD D. MALLON, Ph.D. Harvard 1964. Economic development and foreign trade of Pakistan.
- BERNARD L. MARTIN, Ph.D. Cincinnati 1963. Problems of United States direct private investment in Latin America.
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- AVRAHAM MICHAEL, Ph.D. New School 1964. International trade in citrus fruits.
- SAMI K. MOHAMED, Ph.D. Ohio State 1963. The United Arab Republic's balance of payments, commercial policy, and economic development, 1952-1962.
- VICTOR MOK, Ph.D. Michigan State 1964. The foreign trade of Communist China 1949-1960, a regional and commodity analysis.
- ROBERT J. MURPHY, Ph.D. Fordham 1964. Employment coefficients of output in escape clause industries.
- JOHN A. PINCUS, Ph.D. Harvard 1964. Economic aid and international cost sharing.
- ERNEST H. PREEG, Ph.D. New School 1964. The elasticity of demand for manufactured exports: United States and United Kingdom, 1946-1962.

- SRIPRINYA RAMAKOMUD, Ph.D. Indiana 1963. Thailand's foreign trade: structure and policies, 1951-1960.
- ROBERT RENSHAW, Ph.D. Michigan State 1964. United State foreign economic policy with special reference to lead and zinc.
- MAHAMED A. H. SAKR, Ph.D. Harvard 1964. Economic integration and the growth of less-developed countries.
- ANTHONY E. SCAPERLANDA, Ph.D. Texas 1964. The contribution of the Latin American Free Trade Association to Latin American welfare: a preliminary survey applying the theory of economic integration.
- LEO TANSKY, Ph.D. American 1964. Comparative impact of U.S. and U.S.S.R. economic aid to underdeveloped countries with special reference to India, Turkey, and the United Arab Republic.
- GEORGE J. TZANNETAKIS, Ph.D. New York 1964. Postwar monetary developments and the balance of payments of Greece, 1950-61.
- RAY WARE, Ph.D. Kentucky 1963. The development of the concept of the balance of payments in the United States.
- REGINALD T. WEBER, Ph.D. New York 1963. An appraisal of the International Monetary Fund.
- ELLIOT WETZLER, Ph.D. Johns Hopkins 1964. Trade and economic development in a non-linear general equilibrium context.

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- RICHARD D. BARTELL, B.A. Ripon 1958; M.A. Columbia 1961. Methods for measurement of Common Market discrimination. *Columbia*.
- GIORGIO BASEVI, Laurea, Genova 1961; M.A. Chicago 1962. Trade restrictions and resource allocation in the United States. *Chicago*.
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- ARIE BEENHAKKER, Doctorandus, Netherlands Economic 1961. Capital movements in the European Economic Community. *Purdue*.
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- ELIZABETH P. BOND, B.A. Wellesley 1961; M.A. Yale 1962. The history of the international liquidity problem: a survey and analysis of international monetary conferences 1865-1933. *Yale*.
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- EHSAN CHOUDHRI, B.A. Panjab 1959. Unofficial markets in foreign exchange. *Chicago*.
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- EDWARD L. CLAIBORN, B.S. Idaho 1955. Forecasts of the balance of payments and their realizations. *Princeton*.
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- JOSEPH T. WHITE, B.A. Merrimack 1960. Direct foreign investments and national incomes. *Boston College*.
- H. DAVID WILLEY, B.A. Colgate 1952; M.A. Fletcher School 1956. Balance of payments and economic growth in postwar Germany, Italy and France. *Columbia*.
- TOHRU YAMANAKA, M.A. Northwestern 1956. Shifts in trade areas—their impact on the foreign trade of Japan. *Northwestern*.
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### Business Finance; Investment and Security Markets; Insurance

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- KAARLO P. AHTIALA, Ph.D. Harvard 1964. The short-term adjustment mechanism on the bond market.

- DOUGLAS V. AUSTIN, Ph.D. Indiana 1964. The causes of proxy contests: an empirical study, 1956-60.
- NORMAN BAILEY, D.B.A. Indiana 1964. Real estate investment trusts since 1960.
- ÁRPÁD BANDA, Ph.D. New York 1964. Financing the public utility industry in the postwar era.
- JOSEPH R. BROWN, Ph.D. Southern California 1964. An analysis of competition in the title insurance industry.
- LINDA P. FLETCHER, Ph.D. Pennsylvania 1964. Mutualization of stock life insurance companies.
- HARRY L. GRAY, Ph.D. American 1964. On maximizing returns from investments in Treasury securities. (Principles and implications of general application developed from a case study of the investment portfolios of the Farm Credit Banks.)
- MYRON A. GROVE, Ph.D. Northwestern 1964. A model of the maturity profile of life insurance companies.
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- RICHARD E. KISTLER, Ph.D. Stanford 1964. Parametric analysis of present value problems under conditions of uncertainty.
- CHE LIN, Ph.D. Pennsylvania 1963. The compensation structure in the marketing of group insurance.
- BURTON G. MALKIEL, Ph.D. Princeton 1963. Problems in the structure of financial markets.
- JAMES L. MURPHY, Ph.D. Purdue 1964. A model of investment behavior theory: an econometric predictive test.
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- LLOYD D. ORR, Ph.D. Northwestern 1964. A cross sectional analysis of inventory investment.
- ALFRED N. PAGE, Ph.D. Chicago 1964. The variation of mortgage interest rates.
- GORDON B. PYE, Ph.D. Mass. Inst. Technology 1963. Investment rules for corporations.
- R. BRUCE RICKS, Ph.D. California (Berkeley) 1963. Trend to equity real estate investment by institutions in the post-war period.
- MICHAEL RIEBER, Ph.D. Mass. Inst. Technology 1963. Primary market for United States Treasury bills.
- JOHN C. RITCHIE, JR., Ph.D. Pennsylvania 1963. Trends in internal financing; selected large corporations 1915 to 1955.
- PETER ROSKO, Ph.D. Michigan 1964. Investment aspects of Michigan land contracts.
- LEOPOLD SCHACHNER, Ph.D. New York 1964. Corporate diversification policies.
- PALMER C. SZE, Ph.D. New York 1964. The increase in the corporate investment in trade receivables.
- GRANT B. TAPLIN, Ph.D. New School 1964. Corporate liquidity preference; a re-examination.
- JAMES C. VAN HORNE, Ph.D. Northwestern 1964. Liquidity preference, interest-rate risk, and the term structure of interest rates.
- JAMES K. WEEKLY, D.B.A. Indiana 1963. Equity financing in a less-developed economy. Colombia—a case study.
- JAMES H. WIDDOWSON, Ph.D. Pennsylvania 1963. Life insurance policy changes of underwriting or actuarial significance.
- GLENN A. WILT, Ph.D. Michigan 1963. Nonconvertible preferred stock as a financing instrument 1950-1961.

- GLENN L. WOOD, Ph.D. Pennsylvania 1963. Life insurance policy loans, nature, uses, practices and regulations.
- ALAN ZAKON, Ph.D. California (Los Angeles) 1964. Common stocks as long-term investments in the nineteen-sixties.

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- JAMES C. BAKER, B.S. Indiana 1961; M.B.A. 1962. The German securities exchange: a definitive study with evaluation. *Indiana*.
- EARL S. BEECHER, B.A. Utah 1949; M.B.A. California (Los Angeles) 1958. A study of the state and local indebtedness of California. *California (Los Angeles)*.
- CHARLES H. BELL, B.S.B.A. Memphis State 1958; M.B.A. Arkansas 1959. An investigation of the practicality of the use of the electronic computer in selectivity and timing in portfolio construction and management. *Arkansas*.
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**Business Organization; Managerial Economics; Industrial  
Management; Marketing; Accounting**

*Degrees Conferred*

- ALBERT L. ARCUS, Ph.D. California (Berkeley) 1963. An analysis of a computer method of sequencing operations for assembly lines.
- EDWARD H. BARKER, Ph.D. Southern California 1963. An economic analysis of the changes in retail store sales and location in the central business districts of one hundred and nine central cities in the United States, 1948-1958.
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- GLENN T. WILSON, B.S. Sydney 1960; M.S. Carnegie Inst. Technology 1963. Behavioral theory of production planning in the firm. *Carnegie Inst. Technology*.
- KENNETH E. WING, B.S. Cornell 1958; M.Ed. 1960. Alternative systems for establishing egg market prices. *Cornell*.
- DWAYNE WRIGHTSMAN, B.S. Manchester 1958; M.B.A. Indiana 1959. An analysis of the extent of corporate ownership and control by private pension funds. *Michigan State*.
- J. W. WYPKEMA, M.A. Groningen State (Holland) 1961. The production-smoothing model with variable holding cost. *Rice*.

## Industrial Organization; Government and Business; Industry Studies

### *Degrees Conferred*

- BERNARD S. BECKLER, Ph.D. American 1964. The pricing process in defense procurement.
- JOEL BERGSMAN, Ph.D. Stanford 1963. Economic problems in electric power systems planning.
- DWIGHT BLOOD, Ph.D. Michigan 1963. A cross-section analysis of the domestic intercity travel market.
- BERNARD BOGAR, Ph.D. Indiana 1964. Mergers for diversification and the amended Clayton Act.
- ROGER E. BOLTON, Ph.D. Harvard 1964. Defense purchases and regional growth in the United States.
- WILLIAM D. BRINCKLOE, Ph.D. Pennsylvania 1964. The prediction of military worth.
- MAUREEN D. BRUNT, Ph.D. Harvard 1964. Concentration in the Australian economy.
- VERNON M. BUEHLER, Ph.D. George Washington 1964. Aircraft industry profits, 1952-1955: guidelines for renegotiations.
- BILLY J. COLWELL, Ph.D. Ohio State 1963. Amended Section Seven of the Clayton Act, conglomerate mergers, and preventive antitrust policy.

- WILLIAM S. COMANOR, Ph.D. Harvard 1964. The economics of research and development in the pharmaceutical industry.
- PETER H. CRAWFORD, Ph.D. Columbia 1963. Business proposals for government regulation of monopoly: 1887-1914.
- MARCEL G. DAGEHAIS, Ph.D. Yale 1964. The supply of newsprint paper in North America.
- GORDON K. DOUGLASS, Ph.D. Mass. Inst. Technology 1963. Product variation and international trade in motion pictures.
- LEO M. ELISON, Ph.D. Columbia 1964. Output, employment and labor productivity in the coal industry of the USSR.
- MURRAY FRANKLIN, Ph.D. Michigan 1963. Full-line development, related mergers, and competition in the major appliance industry during the 1950's.
- RICHARD C. GERHAN, Ph.D. Michigan 1963. An analysis of the first and second modernization programs of the Japanese iron and steel industry.
- DAVID G. GREENE, Ph.D. Michigan State 1963. Steel and economic development, capital—output ratios in three Latin American steel plants.
- WILLIAM W. HARNED, Ph.D. California (Los Angeles) 1964. A study of the pricing of inventories of manufacturing firms at market value.
- JOSEPH W. HARRISON, Ph.D. Virginia 1964. Relocation costs and resource allocation.
- RICHARD B. HARSHBARGER, Indiana 1964. TVA coal buying policies: effects on price and method of mining in supplying states.
- EDWARD L. HAUSWALD, Ph.D. Indiana 1963. The transport factor and plant-site selection in the factory fabricated home manufacturing industry.
- DONALD W. HILL, Ph.D. American 1964. The impact of advertising associations on public policy toward advertising.
- PAUL M. HOHENBERG, Ph.D. Mass. Inst. Technology 1963. Chemical industry and economic development in France, Germany, and Switzerland, 1850-1914.
- JAMES M. HOWELL, Ph.D. Tulane 1963. A statistical study of the secondary effects of structural unemployment in the textile industry of Lawrence, Massachusetts.
- RICHARD J. HRUBY, Ph.D. Clark 1964. Real capital investment in railroad transportation property, 1950-62.
- MANLEY IRWIN, Ph.D. Michigan State 1963. Entry restraints in the communication equipment industry.
- ANDREW B. JACK, Ph.D. Harvard 1964. The marketing function of the innovating entrepreneur: the sewing machine and garment industry in the United States.
- CLINTON E. JENCKS, Ph.D. California (Berkeley) 1964. The impact of nationalization on working conditions in British coal mining.
- WILLIAM A. JOHNSTON, Ph.D. Harvard 1964. India's iron and steel industry: a study of planned industrial growth.
- HIROMITSU KANEDA, Ph.D. Stanford 1964. Technical change and returns to scale: an econometric study of Japanese agricultural development in the postwar years.
- KENNETH E. KNIGHT, Ph.D. Carnegie Inst. Technology 1964. A study of technological innovation—the evolution of digital computers.
- RICHARD F. KOSOBUD, Ph.D. Pennsylvania 1964. Forecasting relationships for selected government expenditures and revenues.
- EDWARD R. LEHMAN, Ph.D. New York 1964. Profits, profitability and the oil industry.
- RICHARD L. LESHER, D.B.A. Indiana 1963. Independent research institutes and industrial application of aerospace research.
- OMEDA F. LIVINGSTON, Ed.D. New York 1964. A study of women executives in life insurance companies owned and operated by Negroes with implications for business education.
- G. S. MADDALA, Ph.D. Chicago 1963. Technical change in the bituminous coal industry, 1919-54.

- GEORGE C. MANIATIS, Ph.D. California (Berkeley) 1964. State control over public enterprise: the case of Italy.
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- DALE K. OSBORNE, Ph.D. Kentucky 1963. The theory of oligopoly.
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- HAROLD STROM, Ph.D. California (Los Angeles) 1964. The North Atlantic air transport market—a study of the American competitive position.
- FREDERICK D. STURDIVANT, Ph.D. Northwestern 1964. An historical analysis of the American Hospital Supply Corporation with special attention given to the backward vertical integration of a channel of distribution, 1922 to 1956.
- FEBS B. TAN, Ph.D. New York 1964. Liberalized depreciation—concepts, techniques, and impact upon privately-owned electric utility companies, 1954-61.
- PETER TEMIN, Ph.D. Mass. Inst. Technology 1964. History of the American iron and steel industry from 1830 to 1900.
- SAMSON G. TUCHMAN, Ph.D. New York 1963. The economics of the waste paper industry.
- GEORGE M. WATTLES, Ph.D. Virginia 1964. United States postal rates and costs.
- HENDERIKUS G. WERKEMA, Ph.D. Rice 1963. Profit and related objectives in the theory of the firm.
- GEORGE A. WING, D.B.A. Indiana 1964. A study of the origin and growth of the machine-tool industry in Cincinnati.
- KOZO YAMAMURA, Ph.D. Northwestern 1964. Competition and monopoly in Japan, 1945-1961.
- MAN H. YOU, Ph.D. Oregon 1963. A study of the natural rubber industry with special emphasis on its future prospects.
- EDWARD A. ZANE, Ph.D. Massachusetts 1964. Trailer-on flatcar: an economic analysis of piggyback transportation in New England.

### *Theses in Preparation*

- HUSSEIN A. ABDEL BARR, B.Com. Cairo 1954; LL.B. 1954. The cost structure and field price of natural gas in the USA. *Wisconsin*.
- CARLOS W. ALBERNI, B.B.A. Loyola 1961; M.B.A. California (Los Angeles) 1960. A study of companies with substantial acquisition histories compared with other companies in selected industries. *California (Los Angeles)*.
- MATSUKICHI AMANO, B.A. Meiji 1959; M.B.A. California (Los Angeles) 1962. Effects of social change on industrial relations. *California (Los Angeles)*.
- JUNE AXINN, B.A. Queens 1945. Government trust funds. *Pennsylvania*.
- CHARLES BARNGROVER, M.B.A. Cincinnati 1959. The problem and background on the growth of service industries. *Cincinnati*.

- JAMES F. BATEMAN, B.A. Tulane 1958; M.A. North Carolina 1961. Government participation in economic innovation: the case of American petroleum pipelines. *Tulane*.
- CLAUDE J. BOVÉ, B.A. Harvard 1935; M.A. Columbia 1950. Influence of natural rubber industry upon the economic development of Malaya. *Columbia*.
- GUNTHER BRINK, B.Comm. Sir George Williams 1956; M.B.A. Western Ontario 1959. Impact of antitrust on Canadian economy. *Columbia*.
- ERNEST W. CARLSON, B.S. Mass. Inst. Technology 1956. An econometric model of the automobile industry. *Boston College*.
- GASTONE CHINGARI, B.S. California (Los Angeles) 1955; M.S. 1959. Design and operation of a numerically controlled production facility. *California (Los Angeles)*.
- WILLIAM E. COLE, B.A. Texas 1952. Government monopoly and mixed enterprise and the development of Mexico's economy. *Texas*.
- DAVID W. CONRATH, B.A. Stanford 1956; M.S. Carnegie Inst. Technology 1961; M.A. California (Berkeley) 1964. The economics and politics of the budgeting behavior within N.A.S.A. *California (Berkeley)*.
- MURIEL J. CONVERSE, B.A. Michigan 1960. An econometric study of residential construction expenditures in the United States. *Michigan*.
- ARTHUR J. CORDELL, B.A. McGill 1960; M.A. Cornell 1963. Imperfect and monopolistic competition in historical perspective. *Cornell*.
- DANE J. COX, B.A. Harpur 1958. The reasons why firms become conglomerate. *Cornell*.
- JOHN P. CRECINE, B.S. Carnegie Inst. Technology 1961; M.A. 1963. The political decision maker as a responsive mechanism: a model of political choice. *Carnegie Inst. Technology*.
- NORMAL E. DANIEL, B.S. Tennessee 1954; M.S. 1960. Airport development in Indiana: the role of state government. *Indiana*.
- ROBERT G. DOYLE, JR., B.A. Boston College. Economic aspects and effects of advertising in the ethical drug industry. *Boston College*.
- MORTON EHRLICH, B.B.A. City (New York) 1960. A regional analysis of technological change: a study of the manufacturing sector of the U.S. economy for the years 1947-1957. *Brown*.
- BERT E. ELWERT, Ph.B. Illinois Wesleyan 1951; M.B.A. Indiana 1955. An economic and political study of public policy in broadcasting. *Indiana*.
- LOUIS ENOS, B.A. California 1960; M.B.A. Columbia 1962. A method for scheduling new product development and for allocating resources to the new product development process. *Columbia*.
- JAMES F. FILGAS, B.A. Michigan 1956; M.B.A. Indiana 1959. History and analysis of Yellow Transit Freight Lines, Inc. *Indiana*.
- HORACE H. FISHER, B.A. Utah 1929; M.A. Southern California 1964. Concentration and competition in the brewing industry. *Southern California*.
- OLIVER GALBRAITH, III, B.S. Northwestern 1949; M.B.A. 1951. An analysis of cargo handling communication networks. *California (Los Angeles)*.
- PETER J. GEORGE, B.A. Toronto 1962; M.A. 1963. A benefit cost analysis of the Canadian Pacific Railway. *Toronto*.
- JAISINGH GHORPADE, B.A. Poona 1957; M.B.A. California (Los Angeles) 1960. Enterprise effectiveness as a function of enterprise ownership: a case study of private and state sponsored enterprises in India. *California (Los Angeles)*.
- LEE GLICK, B.A. Pittsburgh 1948; M.A. 1950. The effect of existing regulatory policies on the market structure and behavior dimensions of leading food chains. *Pittsburgh*.
- JOSEPH GOLDMAN, B.A. Brooklyn 1947; M.A. Columbia 1950. Development of the southern iron and steel industry: 1865-1907. *Columbia*.
- EDMUND GRAY, B.S. Hofstra 1956; M.B.A. California (Los Angeles) 1960. The subcontracting process in the aerospace industry. *California (Los Angeles)*.

- IRWIN GRAY, B.E.E. Pratt Inst. 1956; M.S. Columbia 1958; M.B.A. City (New York) 1962. Employment considerations in the award of public contracts. *Columbia*.
- LESLIE GRAYSON, B.A. Oberlin 1951; M.A. Michigan 1952. The economics of a Western European energy policy. *Michigan*.
- RICHARD E. HAMILTON, B.A. Toronto 1961. An economic study of the Damodar Valley Corporation, India. *Duke*.
- SIDNEY S. HANDEL, B.S. Mass. Inst. Technology 1959. An econometric study of investment in the regulated industries. *California (Berkeley)*.
- ROY D. HARRIS, B.S. Texas 1958; M.S. 1960. Empirical study of a job shop like queueing system. *California (Los Angeles)*.
- CLARK A. HAWKINS, B.S. Pennsylvania State 1954; M.S.I.M. Purdue 1960. Calculating the cost of natural gas for purposes of FPC regulation. *Purdue*.
- JACK HAYYA, B.S. Illinois 1952; M.B.A. San Fernando Valley State 1961. Development of explicit principles to aid the government in use of PERT. *California (Los Angeles)*.
- JARED E. HAZLETON, B.B.A. Oklahoma. An economic analysis of the sulphur industry. *Rice*.
- RICHARD HELLMAN, B.A. Columbia 1934. Government competition in the electric power industry. *Columbia*.
- WON T. HONG, B.A. Seoul National 1962; M.A. Columbia 1964. Problems of improving productivity of the small and medium scale manufacturing industries. *Columbia*.
- HUI-MIN HWANG, B.S. National Taiwan 1956; M.S. Minnesota 1960. An econometric investigation of fertilizer industry in the U.S.—with special emphasis on North Central States. *Minnesota*.
- JOHN B. JAEGER, B.S. Southern California 1959; M.B.A. California (Los Angeles) 1960. A comparative management study: organization patterns and processes of hotels in four countries. *California (Los Angeles)*.
- CHARLES JOHNSON, B.S. California (Los Angeles) 1957; M.B.A. 1962. Trends in industrial research management and the effects of government regulation. *California (Los Angeles)*.
- ELBERT B. JOHNSON, B.A. Kansas City 1951; M.A. 1959. Implications of changes in the size of establishment for the location of industrial activity. *Iowa*.
- CARL R. JONES, B.S. Carnegie Inst. Technology 1956; M.B.A. Southern California 1963. Growth, prices, and governmental control processes and policies. *Claremont*.
- JOHN M. JORDAN, B.S. Loyola 1961; M.S. Purdue 1964. Transportation and industrial structure. *Purdue*.
- DAVID R. KAMERSCHEN, B.S. Miami 1959; M.A. 1960. Monopoly and resource allocation: a reappraisal. *Michigan State*.
- NORMAN N. KANGUN, B.B.A. Oklahoma 1959; M.B.A. Indiana 1960. A model of the processes which lead to active enforcement of the Sunday closing laws: a selected community study. *Indiana*.
- PETER KARES, B.S. Ohio 1958; M.S. Purdue 1961. Economic aspects of equipment leasing. *Purdue*.
- WADE R. KILBRIDE, B.S. Regis 1950; M.A. Texas 1958. The economics of stockpiling. *Texas*.
- JONGBIN KIM, B.A. Hitotsubashi 1958; M.A. California 1960. The Korean cotton manufacturing industry. *California (Berkeley)*.
- KEY W. KIM, B.A. Grinnell 1957; M.A. Yale 1959. The economic impact of public enterprises in postwar Japan: 1953-1963. *California (Berkeley)*.
- WESLEY KRIEBEL, B.S. Pennsylvania State College 1953; M.S. 1957. Economic and managerial problems of motor carriers of refrigerated transports. *Missouri*.
- ROBERT L. LAWSON, B.S. Ball State Teachers 1955; M.A. 1957. Bilateral monopoly and the theory of games. *Iowa*.
- GEORGE F. LEAMING, B.S. Lafayette 1953; M.B.A. Arizona 1961. Changes in the composition of the labor force in the primary copper industry. *Arizona*.

- KENNETH MACCRIMMON, B.S. California (Los Angeles) 1959; M.B.A. 1960. Normative decision theory: an experimental study with business executives. *California (Los Angeles)*.
- JOSEPH R. MASON, B.A. Providence. Government financing of research projects contracted to private firms: an economic study of the patent policies of government agencies. *Boston College*.
- JOHN J. MCGOWAN, B.A.Sc. Toronto 1958; M.A. Yale 1963. The effect of alternative merger laws on the size distribution of firms. *Yale*.
- TORLEIF MELOE, B.A. Columbia 1947; M.A. 1948. Oil imports: the economics of control. *Columbia*.
- JOHN H. MERRIAM, B.A. Claremont 1954. The influence of property tax differentials on manufacturing plant location. *Claremont*.
- CHARLES MILLER, B.S. U.S. Naval Academy 1945; M.B.A. California (Los Angeles) 1957. Patterns of growth in industries and companies. *California (Los Angeles)*.
- DOUGLAS NEEDHAM, B.Sc. London School of Economics 1961. Patent protection and the pharmaceutical industry. *Princeton*.
- HENRY ORION, B.A. Brooklyn 1960; M.A. Columbia 1962. Employment and income from Port of Buffalo before and after the opening of the St. Lawrence Seaway. *Columbia*.
- HARRY R. PAGE, B.A. Michigan State 1941; M.B.A. Harvard 1950. Procurement decision-making in the Department of Defense. *American*.
- JOSEPHUS O. PARR, III, B.A. Rice 1961. Risk, imperfect competition and inventories. *Tulane*.
- ROBERT F. ROONEY, B.A. California (Los Angeles) 1959; M.A. Stanford 1961. Taxation and regulation of the domestic crude oil producing industry. *Stanford*.
- JAMES N. ROSSE, B.S. Minnesota 1961; M.A. 1963. Determinants of industrial structure: cost, production, and technological change in the American daily newspaper industry. *Minnesota*.
- CHARLES M. SACKREY, JR., B.A. Texas 1961; M.A. 1963. A case study of the social control of industry: local service airlines. *Texas*.
- GIAN SAHOTA, B.A., M.A., B.T. Panjab 1946-53; M.A. Leeds 1957. Sources of productivity growth in the U.S. fertilizer industry. *Chicago*.
- NANCY L. SCHWARTZ, B.A. Oberlin College 1960; M.S. Purdue 1962. Economic scheduling of transportation equipment. *Purdue*.
- NATHANIEL E. SHECHTER, B.A. Minnesota 1954; M.A. 1956. The generation and transmission program of REA. *Maryland*.
- WERNER SICHEL, B.S. New York 1956. Fire insurance: imperfectly regulated collusion. *Northwestern*.
- EUGENE SILBERBERG, B.S. City (New York) 1960. The demand for inland waterway transportation. *Purdue*.
- J. H. STAFFORD, B.S. Purdue 1961; M.S. 1962. Procurement and inventory control system for feed manufacturers. *Purdue*.
- CHARLES G. STALON, B.A. Butler 1959. Product quality changes and the index of prices. *Purdue*.
- GEORGE C. STEINKE, B.S. Georgetown 1948. Economic evaluation of urban transportation investment proposals. *California (Berkeley)*.
- HENRY L. STETTLER, III, B.A. Notre Dame 1959. Tariffs and economic growth: the American textile industry. *Purdue*.
- JUDITH D. TENDLER, B.A. Michigan 1960; M.A. Columbia 1963. Argentine and Brazilian hydro power experiences. *Columbia*.
- CESAR F. TOSCANO, B.S. California (Los Angeles) 1955; M.S. 1959. Methodology for simulation of a transportation interchange. *California (Los Angeles)*.
- MICHAEL S. TURNER, B.S. Ohio State 1959; M.S. 1960. Inter and intra firm adjustment of the grain, feed, and farm supply industry of Ohio since 1954. *Ohio State*.

- THOMAS E. VOLLMAN, B.S. California (Los Angeles) 1961; M.B.A. 1962. An analysis of the criteria affecting facilities layout. *California (Los Angeles)*.
- JOHN WALGREEN, B.A. Boston College. The economics of ocean liner freight conferences. *Boston College*.
- CHARLES M. WHEAT, B.S.B.A. Arkansas 1961; M.B.A. 1961. The New York, New Haven and Hartford Railroad as a case study of the changing patterns of public policy and capital formation. *Arkansas*.
- JOHN T. WHOLIHAN, B.S. Notre Dame 1959; M.B.A. Indiana 1960. An evaluation of the relative roles of government and private activity in the development of public utilities in an expanding economy, as exemplified by Brazil. *American*.
- RONALD WIGGINS, B.I.E. Syracuse 1938; M.S. 1940. Arbitration of industrial engineering cases. *California (Los Angeles)*.
- FRANCIS WOEHRLING, Lic. Droit, Strasbourg 1961; M.B.A. Michigan State 1962. The North-Atlantic air transport market. *Michigan State*.
- STEWART W. YOST, B.A. California (Berkeley) 1953; M.B.A. California (Los Angeles) 1963. Application of a general simulation model to job shop operations. *California (Los Angeles)*.

### Land Economics; Agricultural Economics, Economic Geography; Housing *Degrees Conferred*

- J. RICHARD ARONSON, Ph.D. Clark 1964. The soybean crushing margin, 1963-62: an economic analysis of the futures markets for soybeans, soybean oil and soybean meal.
- PAUL W. BARKLEY, Ph.D. Kansas State 1963. The economic effects of reservoir development on individual farms lying partially within the site.
- DENNIS L. BAWDEN, Ph.D. California (Berkeley) 1964. Interegional models of the United States turkey industry.
- JAMES T. BONNEN, Ph.D. Harvard 1964. United States agricultural capacity.
- DAVID BRADWELL, Ph.D. Oregon 1964. The timing of development of the Saskatchewan River system.
- J. B. BRITNEY, Ph.D. Purdue 1964. Time series analysis of factors affecting the value of farm real estate, 1911-1959.
- GARDNER M. BROWN, Ph.D. California (Berkeley) 1964. Distribution of benefits and costs from water development: a case study of the San Joaquin Valley—Southern California aqueduct.
- WILLIAM F. CHAPMAN, JR., Ph.D. Florida 1963. Demand and substitution relationships for Florida and California Valencia oranges produced for fresh market.
- JAE H. CHO, Ph.D. Indiana 1964. Post-1945 land reforms and their consequences in South Korea.
- MARTIN K. CHRISTIANSEN, Ph.D. Minnesota 1964. An analysis of labor costs on home delivery milk routes in the Twin Cities market.
- RUSSELL CONNETT, Ph.D. Chicago 1963. Market imperfections in the resale of residential real estate in Park Forest, Illinois.
- HARLAN J. DIRKS, Ph.D. Minnesota 1963. Technological and market forces influencing vertical integration in the swine industry.
- S. R. ECKSTEIN, Ph.D. Harvard 1964. Collective farming in Mexico.
- JOHN A. EDWARDS, Ph.H. Chicago 1963. An analysis of weather-crop yield relationships: a production function.
- D. B. ERICKSON, Ph.D. Purdue 1964. Changes in the regional cattle and calf commercial slaughter production in the United States.
- JOHN E. FLOYD, Ph.D. Chicago 1964. The distribution effects of farm policy: a comparison of the experience of Canada and the United States in the Northern Great Plains.

- R. E. FREEMAN, Ph.D. Purdue 1964. Marketing areas and related issues in federal milk orders.
- GENE A. FUTRELL, Ph.D. Ohio State 1964. Interregional competition in beef and pork: a multiproduct spatial equilibrium investigation.
- VED P. GANDEHI, Ph.D. Harvard 1964. Tax burden on Indian agriculture.
- BIKRAMJIT S. GARCHA, Ph.D. Ohio State 1963. Demand and prices for Ohio greenhouse tomatoes and projection of prices to 1975.
- MILTON L. GLICK, Ph.D. Chicago 1963. The impact of economic development on the returns to labor in agriculture in Mexico.
- CLAUDE GRUEN, Ph.D. Cincinnati 1964. The socio-economic determinants of urban residential housing quality.
- JOHN C. HAGGART, Ph.D. California (Los Angeles) 1963. Authority in municipal organizations for the integration of urban renewal plans.
- ROBERT W. HARTMAN, Ph.D. Harvard 1964. Demand for the stock of non-farm housing.
- V. STEVENS HASTINGS, Ph.D. Chicago 1963. Water rationing: the North Platte River.
- JAMES HEILBRUN, Ph.D. Columbia 1964. Effects of alternative real estate taxes on the maintenance and rehabilitation of urban rental housing.
- DONALD C. HUFFMAN, Ph.D. Ohio State 1963. A technique for classifying farm managers according to managerial ability.
- AHMAD KAMALI-NAFAR, Ph.D. Minnesota 1963. The economic and social impact of credit institutions on agricultural development in Iran.
- THIPPALASSERY N. KRISHNAN, Ph.D. Mass. Inst. Technology 1964. Role of agriculture in economic development: an econometric study of price relationships in India, 1950-1962.
- R. G. LATIMER, Ph.D. Purdue 1964. Some economic aspects of agricultural research and education in the United States.
- ROBERT L. LEONARD, Ph.D. California (Berkeley) 1964. Economic analysis of integrated management of ground and surface water in relation to water importation.
- GEE S. LIN, Ph.D. Kansas State 1964. Organization of farms in Western Kansas (a statistical and programming approach).
- G. A. MACEACHERN, Ph.D. Purdue 1964. Regional projections of technological change in American agriculture to 1980.
- JOHN R. MALONE, Ph.D. Chicago 1963. A statistical comparison of recent new and used house buyers.
- ESSAM H. MONEEB, Ph.D. Ohio State 1964. Economic and statistical analysis of the impact of two federal agricultural policy programs on agricultural resource reallocation in Ohio.
- GENE E. MURRA, Ph.D. Ohio State 1963. Improving the quality and marketing of domestic fleece wool.
- JAMES I. NAKAMURA, Ph.D. Columbia 1964. The place of agricultural production in Japan's economic development.
- AMUDI PASARIBU, Ph.D. Purdue 1963. A study of the competition between natural and synthetic rubber.
- DAVID H. PENNY, Ph.D. Cornell 1964. The transition from subsistence to commercial family farming in North Sumatra.
- JAMES R. PRESCOTT, Ph.D. Harvard 1964. The economics of public housing: a normative analysis.
- PERETZ RAM, Ph.D. Minnesota 1964. A productivity analysis of South Central and Red River Valley farms in Minnesota 1960.
- S. ANANDA RAO, Ph.D. California 1964. Analysis of land values and uses in the context of local economic growth: an empirical approach with special reference to San Benito County, California.
- DEAN RICKENBACK, Ph.D. Indiana 1963. Some economic reconsiderations of zoning—remedial, emergent, and utopian.

- WILLIAM N. ROSS, Ph.D. Kansas State 1964. Effects of tenure on adjustment to changing farm conditions.
- RICHARD RUTH, Ph.D. Wisconsin 1964. The cotton and sugar industries of Mexico and Peru.
- AZIZ A. F. SAEKI, Ph.D. California (Berkeley) 1964. An economic study of the California canteloupe industry.
- R. S. SAVALE, Ph.D. Kansas State 1964. Farm planning and possibilities of capital accumulation on selected farms in Nasik District of Maharashtra State, India: an application of multi-period programming procedures.
- ROBERT D. SCHULTZ, Ph.D. California (Berkeley) 1964. Decision making in a wildland fire game.
- LAWRENCE H. SEAW, Ph.D. Harvard 1964. A measure of the effects of weather on agricultural output.
- JEROME B. SIEBERT, Ph.D. California (Berkeley) 1964. Long range adjustment of orange packing houses in Central California: a consideration of the optimum number, size, and location of packing facilities.
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- LOUIS H. STERN, Ph.D. California (Los Angeles) 1964. Interstate trade and the distribution economic activity in Texas, 1940-1956.
- DONALD L. STERNITZKE, Ph.D. Iowa 1963. The raw-materials problem in Latin America.
- GARY C. TAYLOR, Ph.D. California (Berkeley) 1964. The economic planning of water supply systems with particular reference to water conveyance.
- R. W. TAYLOR, Ph.D. Purdue 1963. Analysis of federal farm programs for Central Indiana.
- DENNIS E. TEEGUARDEN, Ph.D. California (Berkeley) 1964. The market for private timber in the Central Sierra Nevada region of California: an economic analysis of market structure conduct and stability.
- W. ALLEN TINSLEY, Ph.D. Minnesota 1963. An economic analysis of the use of corn silage in beef cattle rations.
- ALFRED J. VAN TASSEL, Ph.D. Columbia 1964. The influence of changing technology and resources on employment in the lumber industry of the Pacific Northwest.
- ROBERT S. VILLANUEVA, Ph.D. Pittsburgh 1964. An econometric estimation of user's demand for natural gas.
- GLEN J. VOLLMAR, Ph.D. Ohio State 1963. An investigation of the economic use of farm equipment, Northwestern Ohio cash grain farms.
- WILLIAM WALDORF, Ph.D. Chicago 1964. Statistical demand functions for manufactured farm foods, food manufacturers' services, and farm products used in manufacturing foods.
- THOMAS D. WALLACE, Ph.D. Chicago 1963. An analysis of recent shifts in the location of hog slaughtering: special emphasis on the corn belt.
- NORMAN V. WHITEHAIR, Ph.D. Purdue 1964. Measuring financial performance of cooperative grain and supply firms in Indiana.
- ROBERT YOST, Ph.D. California (Los Angeles) 1963. A systematic means of predicting the individual locational effects of changes in major urban commercial land-use variables.

### *Theses in Preparation*

- MOHAMMED AFZAL, B.S. Panjab 1955; M.S. 1957. Application and evaluation of inventory models to determine optimum feed reserves under varied pasture conditions and stocking rates. *Kansas State*.
- HASIM A. AL-SAMARRAI, B.S. Florida 1962. Methods of assessing beneficiaries for a share of the cost of water control projects that enhance land values. *Florida*.
- JAMES B. BELL, B.S. Virginia Polytechnic Inst. 1954; M.S. 1958. Market structure and economic analysis of the Florida sweet corn industry. *Florida*.

- SHAUL BEN-DAVID, M.Sc. Hebrew 1959. An analysis of the seasonality of demand for apples. *Cornell*.
- BRIAN BENTICK, B.Comm. Melbourne 1956; M.Comm. 1961. The land market and the trade cycle: a refinement and application of the theory of portfolio choice. *Yale*.
- WILLIAM R. BENTLEY, B.S. California 1960; M.F. Michigan 1961. An economic study of the timber allocation policies of the United States Forest Service. *California (Berkeley)*.
- JAY BERGER, B.A. California (Los Angeles) 1956; M.B.A. 1960. Determination of the economic height of high-rise structures. *California (Los Angeles)*.
- ANNE M. BROWN, B.A. Texas 1958. Resource development in East Texas. *Texas*.
- KRISHNA K. S. CHAUHAN, B.Sc. Ag. Agra (India) 1954; M.Sc. Ag. 1956. The effect of general economic conditions on the development of agricultural marketing. *Ohio State*.
- CHAO-CHEN CHEN, B.S. National Taiwan 1954; M.S. Pennsylvania State 1960. A statistical analysis of late summer onion prices in the United States. *Cornell*.
- DEAN T. CHEN, B.S. Taiwan National 1956; M.S. Minnesota 1960. The economies to scale in the provision of public services to rural communities in Southwest Minnesota. *Minnesota*.
- D. W. COBIA, B.S. Brigham Young 1960; M.S. Purdue 1963. Capital markets and market structure in the grain industry. *Purdue*.
- JAMES V. CORNEHLS, B.A. Mexico City 1961. Mexican agrarian reform and agricultural development. *Texas*.
- VIRGIL E. CROWLEY, B.S. Missouri 1943; M.S. 1955. Adjustments needed to raise levels of farm income in the corn belt area. *Missouri*.
- THOMAS D. CURTIS, B.S. Ball State Teachers 1957; M.A. 1959. Study of land tenure in the U.S. and Puerto Rico. *Indiana*.
- FRANK J. DALIA, B.S. Tulane 1949; M.S. 1952. An economic examination of water resource development in the Ouachita and Black River Basin. *Tulane*.
- LAWRENCE S. DAVIS, B.S. Michigan 1956; M.F. 1960. The economic possibilities of large scale fuel modification as a partial solution to California's wildfire problem. *California (Berkeley)*.
- D. R. DESAI, B.S. Bombay 1952; M.S. Karnatak 1954. The effects of rainfall and temperature on the yields of wheat and grain sorghum under varied management practices. *Kansas State*.
- SCOTT M. EDDIE, B.S. Minnesota 1960. Agriculture in the economy of the Kingdom of Hungary, 1867-1913. *Mass. Inst. Technology*.
- VERNON R. EIDMAN, B.S. Illinois 1958; M.S. 1961. A decision model for California turkey meat producers. *California (Berkeley)*.
- CLAUDE E. ELIAS, JR., B.A. Wyoming 1949; M.A. 1950. Effect of mass subdividing on local government revenues and expenditures. *California (Los Angeles)*.
- ROBERT J. T. EMOND, B.A. State University of New York 1951; B.A. Chattanooga 1962. Recreational expenditures by large industrial forest land owners. *Virginia*.
- GORDON W. ERLANDSON, B.S. Minnesota 1959; M.S. 1962. A market structure study of the Minnesota fluid milk industry. *Minnesota*.
- BARRY C. FIELD, B.S. Cornell 1956; M.S. 1959. The impact of the changing economic structure of farming on the strategy of agricultural policy. *California (Berkeley)*.
- DAVID F. FISHER, B.A. Trinity (Australia) 1954; M.Rel.Educ. Union Theological Seminary 1957. The agrarian reform in the Mexican sugar industry. *Columbia*.
- ALBERTO B. FRANCO, Ing. Agr. School of Agriculture of Medellin; M.S. Cornell 1962. Nature and conditions associated with the existence of Latifundia in southern Brazil. *Cornell*.
- HARALD B. GIAEVER, Agric. degree, Agricultural College of Norway 1954; M.S. North Carolina State 1959. Optimal dairy cow replacement policies. *California (Berkeley)*.
- GLENN GILLESPIE, B.S. Missouri 1959. An evaluation of the social and economic factors affecting water oriented outdoor recreation. *Missouri*.

- B. L. GREEN, B.S. Arkansas 1958; M.S. 1961. Resource adjustments to provide income from outdoor recreation. *Purdue*.
- JAMES W. GRUEBELE, B.S. North Dakota State 1958; M.S. Iowa (Ames) 1961. Competitive price structure of Minnesota creameries. *Minnesota*.
- B. S. H. GUSTAFSSON, B.S. Royal Agricultural College of Sweden 1956; M.S. Illinois 1964. Analysis and measurement of cattle production management. *Purdue*.
- CARL W. HALE, B.A. Texas A. & M. 1959; M.A. 1961. Metropolitan areas as economic regions. *Texas*.
- RAYMOND W. HAMILTON, B.A. American 1959. A comparative analysis of public housing in the United States, Britain, and Sweden. *Maryland*.
- MD. SHAMSUL HAQUE, B.C. Decca (E. Pakistan) 1953; M.C. 1955; M.B.A. American 1961. Jute—the biggest industry in Pakistan. *American*.
- KENNETH R. HENDERSON, B.S.A. Florida 1961. The demand for frozen orange concentrate produced in Florida. *Florida*.
- UPTON HENDERSON, B.S. Humboldt State 1958; M.S. Washington State 1960. Economic impact of the Swan Lake wildlife refuge on rural communities in Sheridan County, Missouri. *Missouri*.
- THEOR A. HERTSGAARD, B.S. North Dakota State 1953; M.S. 1958. Optimum patterns of production location and product distribution for livestock and poultry. *Minnesota*.
- GLENN C. HIMES, B.S. Purdue 1951; M.S. 1957. World equilibrium analysis of the feed grain economy. *Ohio State*.
- YU-CHU HSU, B.S. Taiwan Provincial College of Agriculture 1954; M.S. 1960. Income variability and resource productivity of rice farms in Changhwa and Nantou Hsien, Taiwan. *Cornell*.
- THAMBAPILLAI JOGARATNAM, B.S. Ceylon 1953; M.S.A. Ontario Agricultural College 1956. Food in the economy of Ceylon, 1840-1960. *Cornell*.
- GERALD L. KARR, B.S. Kansas State 1959; M.S. Southern Illinois 1962. Marketing and pricing of feeder pigs in Illinois: with implications of the effect of specialization within the swine industry upon buyers' decisions. *Southern Illinois*.
- ORVAL G. KERCHNER, B.S. Illinois 1954; M.S. 1958. A study of the capacity and flexibility of facilities in milk manufacturing plants. *Minnesota*.
- DONALD A. KING, B.S. Holy Cross 1957; M.A. Clark 1959. An analysis of federal participation in urban renewal. *Clark*.
- YOAV KISLEV, B.S.C. Hebrew 1960; M.S.C. 1961. Estimating a production function from the 1959 U.S. census agricultural data. *Chicago*.
- EDWARD E. LAITILA, B.S. California (Los Angeles) 1960; M.B.A. 1961. An economic appraisal of U.S. area redevelopment experience. *Indiana*.
- CHARLES LAMPHREAR, B.S. Kansas State 1962. The use of analytical models in determining crop allocations for Kansas. *Kansas State*.
- JAMES L. LEIBFRIED, B.S. South Dakota State 1958; M.S. 1959. A study of the negotiation and operation of the International Wheat Agreement. *Kansas State*.
- CHARLES H. LITTLE, B.S. North Carolina State 1961. The derivation of a predictive model for evaluating a timber stand employing multivariate statistical techniques. *California (Berkeley)*.
- DOUGALD A. MACFARLANE, B.A. St. Francis Xavier 1948; M.A. McGill 1957. Economic aspects of fisheries resources. *Washington*.
- JIMMY L. MATTHEWS, B.S. Missouri 1959; M.S. 1960. Statistical analysis of demand relationships for fresh and processed asparagus. *California (Berkeley)*.
- WILLIAM L. M. MCKILLOP, B.Sc. Aberdeen 1957; M.Sc. New Brunswick 1959. A statistical estimation of the long-run supply and demand equations for major forest products in the United States of America. *California (Berkeley)*.
- ALI I. MEDANI, B.S. London 1956; M.S. Khartoum 1958. Specification and estimation of

- production functions (with specific application to the California dairy industry). *California (Berkeley)*.
- JOSEPH J. MENDEL, B.S. Michigan 1941; M.S. Ohio State 1963. Motivation of the small woodland owners in Southeastern Ohio. *Ohio State*.
- MIAN A. H. NAZ, B.A. Panjab 1950; M.A. 1952; M.A. Maryland 1960. An investigation of the role of the state in the development of agriculture in West Pakistan. *American*.
- DONALD R. NICHOLSON, B.S. Missouri 1958; M.S. 1961. Optimum milk supplies in federal milk order markets. *Cornell*.
- GERALD P. OWENS, B.S. Phillips 1957; M.S. Kansas State 1958. Outdoor recreation—characteristics of participants and effective demand. *Ohio State*.
- OSMO PASANEN, B.A. California 1960. An analysis of the agricultural base and potential of economic areas 2b in Northeast Missouri. *Missouri*.
- RAM K. PATEL, B.Sc. Allahabad Agricultural Inst. 1952; M.Sc. Government Agricultural College, Kanpur 1954. Management of state agricultural experiment station research lands and livestock facilities. *Ohio State*.
- EDWARD L. PHILLIPS, B.A. Washington 1959. Internal pricing and the allocation of joint production costs in a vertically integrated company in the forest products industry. *Washington*.
- CHARLES R. PLOTT, B.S. Oklahoma State 1961. Influences of decision processes on decisions to undertake urban renewal. *Virginia*.
- MALCOLM J. PURVIS, B.Sc. Wye 1962; M.S. Cornell 1964. The food and agricultural economy of Malaysia. *Cornell*.
- VICENTE U. QUINTANA, B.S. Philippines 1957; M.S. 1959. An analysis of agricultural loans granted by the Development Bank of the Philippines. *Ohio State*.
- ARVO RAY, B.A. Western Ontario 1960; M.A. Toronto 1963. The operations of the Canadian Wheat Board. *Toronto*.
- W. R. REILLY, B.S. Minnesota 1954; M.B.A. 1956. Structural changes in the soybean processing industry. *Purdue*.
- D. C. ROCKE, B.S. Illinois 1941; M.S. 1949. Farm behavior and decision-making in purchasing supplies. *Purdue*.
- JOHN A. ROGALLA, B.S. California State Polytechnic 1956; M.S. Cornell 1958. Farm business expansion in an economically disadvantaged region of the Northeastern United States. *Cornell*.
- REYNOLD M. SACHS, B.A. Oberlin 1961; M.A. Northwestern 1962. Technological innovation in agriculture and economic development. *Columbia*.
- PAUL L. SCHMIDBAUER, B.S. Mass. Inst. Technology 1959; M.B.A. California (Berkeley) 1961; M.A. 1963. Information and communication requirements of the wheat market: an example of a competitive system. *California (Berkeley)*.
- SARASWATI P. SINGH, M.A. Allahabad 1958. Mobilization of agricultural resources for development in Shahabad District, India. *Kansas State*.
- DONALD C. TAYLOR, B.S. Cornell 1959; M.S. Minnesota 1964. Income improving adjustments and normative supply responses for hogs and beef in Southwest Minnesota. *Minnesota*.
- AZRIEL TELLER, B.B.A. City (New York) 1962. Air pollution abatement: an economic study into the costs of control. *Johns Hopkins*.
- NANDIAL P. TOLANI, B.S. Bombay 1945; M.S. Cornell 1947. Social investment decisions: an operational model for choosing between an irrigation project and a fertilizer plant in underdeveloped countries. *Cornell*.
- E. W. TYRCHNIEWICZ, B.S. Manitoba 1962; M.S. Purdue 1963. Econometric study of operator and unpaid family labor in agriculture. *Purdue*.
- MILLAN VUCHICH, B.A. Pittsburgh 1960; M.A. 1961. Economic determinants of residential rehabilitation. *Pittsburgh*.

- GEORGE C. WANG, M.A. St. John's (Shanghai) 1945; M.A. Columbia 1963. The contribution of agriculture to capital accumulation in Socialist China, 1950-1962. *Columbia*.
- JOHN WEICHER, B.A. Michigan 1959. The effect of urban renewal on the cost of municipal services. *Chicago*.
- ROBERT C. WELLS, B.S. Connecticut 1956; M.S. Cornell 1963. An economic study of changes in dairy farming in the Central Plain Region of New York, 1954-1964. *Cornell*.
- KELSO L. WESSEL, B.S. Purdue 1962; M.S. Connecticut 1964. An economic assessment of pioneer settlement in the Bolivian tropics. *Cornell*.
- FRANK H. WEYMAR, B.S. Mass. Inst. Technology 1958. The dynamics of the world cocoa market. *Mass. Inst. Technology*.
- JAMES WHITTAKER, B.S. Purdue 1958; M.S. 1960. Economics of the hardwood lumber producing industry of Southeast Ohio-Eastern Kentucky. *Ohio State*.
- WILLIAM W. WOOD, JR., B.A. Occidental 1951; M.S. California 1963. The Agricultural Marketing Agreement act of 1937, as amended, with special reference to California. *California (Berkeley)*.
- C.-J. YEH, B.S. Taiwan Chung-hsing 1957; M.S. Kansas State 1962. Trends and cycles in agricultural commodity marketing costs. *Purdue*.

### Labor Economics

#### *Degrees Conferred*

- STUART H. ALTMAN, Ph.D. California (Los Angeles) 1964. Factors affecting the unemployment of married women.
- KENTON L. ANDERSON, D.B.A. Indiana 1963. Long-term unemployed blue-collar workers in the Evansville, Indiana labor market.
- MAHABIR BAJAJ, Ph.D. California (Los Angeles) 1963. The evolution of the concept of property rights in jobs in Indian industrial relations.
- HOWARD R. BLOCH, Ph.D. Princeton 1963. An analysis of an attempt to cut labor costs despite strong union opposition.
- MICHAEL E. BORUS, Ph.D. Yale 1964. The economic effectiveness of retraining the unemployed.
- PAUL D. BUSH, Ph.D. Claremont 1964. Marginalism and institutionalism in labor economics: a study in methodology with special reference to labor economics.
- GLEN CAIN, Ph.D. Chicago 1964. Labor force participation of married women.
- THOMAS G. CARPENTER, Ph.D. Florida 1963. A preventive approach to the migrant labor problem in Florida agriculture.
- JANET G. CHAPMAN, Ph.D. Columbia 1963. Real wages in Soviet Russia since 1928.
- HERBERT A. CHESTER, Ph.D. Mass. Inst. Technology 1964. Worker retraining under the Area Redevelopment Act: a Massachusetts study.
- TEK-CHEUNG CHEUNG, Ph.D. Georgetown 1964. The growth of technical manpower in Communist China, 1949-1962; training, employment and significance to economic growth.
- ARBEN O. CLARK, D.B.A. Indiana 1964. Employee perception of and attitude toward the pension plans of employing organizations.
- LAWRENCE DONNELLY, Ph.D. Cincinnati 1964. A study in wage dispersion and wage skewness within occupations.
- JOHN W. DORSEY, JR., Ph.D. Harvard 1964. The Mack case: a study in unemployment.
- LOUIS F. EBERSOLE, JR., Ph.D. Alabama 1964. Democratizing training relations in business and industry.
- HERMANN ENZER, Ph.D. Harvard 1964. Learning on-the-job: a process analysis.
- PETER F. FREUND, Ph.D. New York 1964. Labor relations in the New York City rapid transit industry, 1945-1960.

- STANLEY L. FRIEDLANDER, Ph.D. Mass. Inst. Technology 1964. Labor migration and economic growth: a case study of Puerto Rico.
- HARRY GILMAN, Ph.D. Chicago 1963. Discrimination and the white-nonwhite unemployment differentials.
- RICHARD B. HALLEY, Ph.D. Stanford 1964. Pay levels, wage structures, and earnings-commuting relationships in Portland, Oregon: 1920-1960.
- IRWIN L. HERNSTADT, Ph.D. Mass. Inst. Technology 1964. Supply and demand in a clerical labor market.
- BRUCE H. HERRICK, Ph.D. Mass. Inst. Technology 1964. Internal migration, unemployment, and economic growth in postwar Chile.
- DALE L. HJESTAND, Ph.D. Columbia 1963. Economic growth and the opportunities of minorities: an analysis of changes in the employment of Negroes and women.
- ROBERT S. JOHNSON, Ph.D. Virginia 1964. Employment service as government policy.
- EDWARD D. KALACHEK, Ph.D. Mass. Inst. Technology 1963. Determinants of higher unemployment rates, 1958-60.
- MORTON I. KAMIEN, Ph.D. Purdue 1964. An econometric study of structural changes in the composition of the labor force with special reference to the railroads.
- DAVID E. KAUN, Ph.D. Stanford 1964. Economics of the minimum wage: the effects of the Fair Labor Standards Act, 1945-1960.
- KYE C. KIM, Ph.D. New York (Buffalo) 1964. Wage structure and wage change.
- TRULY E. KINCEY, Ph.D. Ohio State 1963. A study of the relationship between firm size and some of the factors important in establishing supplemental unemployment benefits programs in the Ohio rubber industry.
- GUNDAR J. KING, Ph.D. Stanford 1963. Manpower management in Soviet Latvia: a case study.
- YOUNG W. KOO, Ph.D. Ohio State 1963. An analysis of selected factors influencing labor efficiency in supermarkets.
- MUKHTAR A. MALIK, Ph.D. Michigan 1963. The economic effects of minimum wage legislation in the United States and Britain in the postwar period.
- BRUCE W. MARION, Ph.D. Ohio State 1963. Labor utilization and scheduling in four Ohio meat departments.
- FRANCIS M. McLAUGHLIN, Ph.D. Mass. Inst. Technology 1964. Industrial relations in the Boston longshore industry.
- NOAH M. MELTZ, Ph.D. Princeton 1963. Changes in the occupational composition of the Canadian labour force 1931-1961.
- JACK NICKSON, Ph.D. Oklahoma 1964. An economic study of employment conditions for registered professional nurses in Oklahoma.
- FRANCES E. OXLEY, Ph.D. Indiana 1963. The concept of survival in the theory of American unions with special reference to the work of Arthur M. Ross.
- JOHN M. OWEN, JR., Ph.D. Columbia 1964. The supply of labor and the demand for recreation in the United States: 1900-1961.
- SAM F. PARISH, Ph.D. Texas 1964. A case study of Latin American unionization in Austin, Texas.
- JOHN J. PASCUCCI, Ph.D. Stanford 1963. The investment policies of collectively bargained pension funds.
- LAURA E. ROMMINE, Ph.D. Columbia 1963. Wages of railroad employees: 1926-1956.
- ANTONIO F. SAGRISTA, Ph.D. Cornell 1963. Social security in Japan—its evolution, present status and economic implications.
- GEORGINA M. SMITH, Ph.D. Rutgers 1964. An empirical investigation of demand and supply in a local job market for women.
- GORMAN C. SMITH, Ph.D. Columbia 1964. Occupational pay differentials for military technicians.

- JOHN M. STOCHAJ, Ph.D. New York 1963. The development of the Atlantic and Gulf Coast District of the Seafarers International Union of North America: The first twenty years, 1938-1958.
- ROBERT L. SWINTH, Ph.D. Stanford 1963. Certain effects of training goals on subsequent task performance.
- HEINZ VERGIN, Ph.D. Minnesota 1963. Determinants of money wage changes in German industry: an analysis of German wage behavior from 1953 to 1960.
- JAMES R. WASON, Ph.D. American 1964. Labor and politics in Washington in the early Jacksonian era.
- HARRY J. WATERS, Ph.D. New York 1964. An apprenticeship and training program for industrial distributor salesmen.
- HERBERT D. WERNER, Ph.D. California (Berkeley) 1964. A history of labor relations in the American petroleum industry.
- RHEA H. WEST, JR., Ph.D. Alabama 1964. Jurisdictional labor disputes in the atomic energy industry: a case study of the Oak Ridge operations.
- JAMES WILLIAMS, Ph.D. California (Los Angeles) 1964. Setting job standards and selecting or evaluating workers by physiological work measurement.

#### *Theses in Preparation*

- W. P. ALBRECHT, B.A. Princeton 1956; M.A. South Carolina 1961. Labor markets, unemployment and wages in the United States, 1948-1962. *Yale*.
- MAHMUD M. AL-HABIB, B. A. Texas 1953; M. A. 1955. The labor movements and socio-politico-economic institutions in the Middle East. *Wisconsin*.
- BRUCE T. ALLEN, B.A. DePauw 1960; M.B.A. Chicago 1961. The effect of product market concentration on wage behavior: American manufacturing, 1947-1960. *Cornell*.
- EILEEN M. BEYDA, B.A. Wyoming 1949; M.B.A. Indiana 1957. A study of employee economic education in industry. *Indiana*.
- JULIA G. BIEHN, B.A. Mt. Holyoke 1960. Experience rating in unemployment compensation: an effective means of taxation. *Duke*.
- WILLIAM C. BONIFIELD, B.S. Bradley 1958. The ministerial profession: an economic analysis. *Minnesota*.
- THOMAS E. BORCHERDING, B.A. Cincinnati 1961. The growth of civilian public employment in the United States, 1900-1962. *Duke*.
- EDWIN C. BOYD, B.A. Duke 1954; M.B.A. Pennsylvania 1958. An evaluation of company and union programs aimed at alleviating the adverse effects of worker displacement due to automation and technological change. *American*.
- VERNON BRIGGS, B.S. Maryland 1959; M.A. Michigan State 1960. A study of strike subsidies. *Michigan State*.
- BENJAMIN BURDETSKY, B.S.E. Temple 1950; M.E. 1958. An evaluation of policies and programs to assist the technologically obsolete worker. *American*.
- LAWRENCE CHIMERINE, B.S. Brooklyn 1961. An econometric analysis of regional wage differentials. *Brown*.
- SERGIO DE CASTRO (earlier degree not supplied). A study of wage differentials in the Santiago labor market. *Chicago*.
- JOHN C. DETTER, B.S. Akron 1955; M.B.A. Indiana 1958. Economic effects of short work week in the rubber tire industry. *Western Reserve*.
- STANLEY DILLER, B.A. Queens 1961. The paternalistic behavior of employers in underdeveloped countries. *Columbia*.
- FRANZ DOLP, B.S. Yale 1958. Organization of agricultural wage labor in California. *California (Berkeley)*.
- SHMUEL ERlichman, B.A. Hebrew 1955; M.A. City (New York) 1959. The impact of trade unions on productivity: the case of Israel, Norway and Ghana. *New School*.

- EDWIN F. ESTLE, B.S. Purdue 1956. A theory of on-the-job training. *Princeton*.
- ANTHONY C. FISHER, B.A. Columbia 1962. The supply of volunteers for military service. *Columbia*.
- H. NAYLOR FITZHUGH, B.S. Harvard 1930; M.B.A. 1933. A comparative study of the commission, salary and combination methods for compensating service station attendants. *American*.
- JOSEPH W. FORD, B.A. Fordham 1959; M.A. 1961. Manpower legislation and the problem of structural unemployment. *Fordham*.
- KEVIN A. FRENZEL, B.A. Pacific Lutheran 1958. The differential emphasis on certain provisions of the Employment Act of 1946. *Washington*.
- ELEANOR GILPATRICK, B.A. Brooklyn 1951; M.A. New School 1959. Structural unemployment and inadequate aggregate demand: U.S., 1947-1962. *Cornell*.
- WILLIAM D. HEIER, B.S. Maryland 1955; M.A. George Washington 1960. The component activities and functional parameters of the union steward's position, a regional inquiry. *American*.
- JAMES L. HESTER, B.S. Mississippi State 1961; M.B.A. 1961. Economics of labor union organizing: analysis of the decision-making process. *Arkansas*.
- JAMES K. HIGHTOWER, B.A. Kalamazoo 1958. The Kaiser Steel long-range sharing plan. *Claremont*.
- GEORGE IDEN, B.A. Ohio Wesleyan. A regional analysis of major high unemployment areas. *Harvard*.
- CASWELL L. JOHNSON, B.A. McGill 1960; M.A. Columbia 1963. Wage differentials in the European Economic Community, 1950-1963. *Columbia*.
- ERWIN L. KELLY, B.A. California 1954. The Negro in unions in the New Orleans area during the nineteenth century. *Tulane*.
- EDWARD C. KOZIARA, B.A. Blackburn 1957. Attitudes of professional training directors toward federal training programs. *Wisconsin*.
- WILLIAM M. LANDES, B.A. Columbia 1959. An economic analysis of fair employment practice laws. *Columbia*.
- DAVID B. LIPSKY, B.S. Cornell 1961. The labor market experience of workers displaced by plant shutdowns. *Mass. Inst. Technology*.
- PAUL J. McNULTY, B.A. Hobart 1959. Economics and the study of labor. *Cornell*.
- WILLIAM MAHER, B.A. and Ph.L. St. Louis 1951; S.T.L. Woodstock 1958; M.A. Fordham 1960. Economic and institutional factors influencing the shortage of general duty nurses in the metropolitan Boston nursing market. *Boston College*.
- STANLEY MASTERS, B.A. Amherst 1961. Changes in employed worker productivity relative to changes in employment, as output varies over the business cycle. *Princeton*.
- JEAN-LUC MIGUE, B.A. Montreal 1953; M.A. 1956. The relation of skilled to unskilled wages (with special reference to Canada). *American*.
- JOSEPH D. MOONEY, B.A. Harvard 1960. An analysis of occupational mobility and the process of reabsorption into the labor market for displaced engineers. *Mass. Inst. Technology*.
- HERBERT C. MORTON, B.A. Minnesota 1947; M.A. 1950. Public contracts and private jobs: experience under the Walsh-Healey Act. *Minnesota*.
- WILLIAM O'LOUGHLIN, B.A. New York 1938. Wage structure and economic development in Puerto Rico. *Columbia*.
- MARTIN M. PERLINE, B.A. Arizona State 1960; M.A. Ohio State 1962. Trade union organization of the retail trades: problems and potential. *Ohio State*.
- PIERRE-PAUL PROULX, B.Comm. Ottawa 1960; M.A. Toronto 1961. Structural unemployment in Canada—its definition, measurement, and policy implications. *Princeton*.
- ALVIN PURYEAR, B.A. Yale 1960; M.B.A. Columbia 1962. Youth unemployment in New York City. *Columbia*.

- BRIAN QUINN, M.A. Glasgow 1959; M.A. Manchester 1961. Some problems of producing an industrial labor force in an underdeveloped country. *Cornell*.
- IRA J. REES, B.A. Wichita 1934; L.L.B. Oklahoma 1939; L.L.M. George Washington 1951; M.B.A. Maryland 1956. The role of the attorney consultant in labor relations in a southern metropolitan area. *Alabama*.
- SHERWIN ROSEN, B.S. Purdue 1960. Cyclical variance of employment on U.S. railroads. *Chicago*.
- KAREN E. SHALLCROSS, B.A. Pennsylvania State 1961; M.S. Wisconsin 1963. Unemployment and the legislative process. *Wisconsin*.
- RICHARD J. SOLIE, B.S. Wisconsin State (Superior) 1955. An evaluation of job retraining as a partial solution to the problem of depressed area unemployment. *Tennessee*.
- JOHN B. SPERRY, B.S. New York 1947; M.B.A. Pennsylvania 1955. A formulation of a system for payment and accounting for accrued pay of uniformed personnel of the U.S. Army. *American*.
- JACK E. STEEN, B.S.M.E. Kentucky 1951; M.B.A. 1958. An analysis of mediation activity in strike situations in the metropolitan Birmingham, Alabama area. *Alabama*.
- JOANN P. STEWART, B.A. St. Lawrence 1949; M.A. Boston 1960. The changing wage structure of New England since World War II. *Boston*.
- ARTHUR A. THOMPSON, JR., B.S. Tennessee 1961. An analysis of employee-management attitudes in the Tennessee Valley Authority. *Tennessee*.
- BRINTON P. THOMPSON, B.S. Florida 1954; M.A. 1958. Monopsonistic competition in the labor market. *Virginia*.
- RICHARD S. UNDERHILL, B.A. Michigan 1950; M.A. 1951. The power structure of industrial relations in Sweden. *Indiana*.
- ROGER N. WAUD, B.A. Harvard 1960; M.A. California 1962. Structural unemployment in the United States. *California (Berkeley)*.
- ROBERT WELSH, B.S. Ohio State 1957; M.A. 1962. An analysis of management-labor relations in agricultural marketing firms. *Ohio State*.

### Population; Welfare Programs; Consumer Economics

#### *Degrees Conferred*

- HENRY AARON, Ph.D. Harvard 1964. Social security in an expanding economy.
- RICHARD M. BAILEY, D.B.A. Indiana 1963. An economic analysis of private medical practice organization.
- RONALD CONLEY, Ph.D. Johns Hopkins 1964. The economics of vocational rehabilitation.
- LARRY L. CUMMINGS, D.B.A. Indiana 1964. Correlates of selected dimensions of student performance in a graduate school of business.
- ALI HEZAREH, Ph.D. Southern California 1963. Financing of medical care for the aged: a comparative evaluation of compulsory versus voluntary health insurance.
- SOEGITO REKSODIHARDJO, Ph.D. California (Los Angeles) 1964. Skills investment in a developing country: an appraisal of management development for Indonesia.
- PAUL P. ROGERS, Ph.D. Kansas 1964. The significance of insurance in the command economy: a study of Soviet insurance.
- NASIM M. SADIQ, Ph.D. American 1964. The economic effects of postponement of marriage for Pakistan.
- PRAVINCHANDRA M. VISARIA, Ph.D. Princeton 1963. The sex ratio of the population of India.
- RICHARD E. VOSBURGH, D.B.A. Indiana 1964. A general theory of personal consumption.
- CHARLES B. WARDEN, JR., Ph.D. Harvard 1964. Unemployment insurance: a statistical study of Massachusetts experience.

*Theses in Preparation*

- JOSEPH BUCHWALD, B.S. California (Los Angeles) 1957; M.B.A. 1960. Life care contracts in California. *California (Los Angeles)*.
- RICHARD N. CARDOZO, B.A. Carleton 1956; M.B.A. Harvard 1959. An experimental study of consumer effort, expectation, and satisfaction. *Minnesota*.
- HAROLD A. COHEN, B.A. Harpur 1959. Hospital costs and pricing in a short term general hospital. *Cornell*.
- JUNE COHN, B.A. Sarah Lawrence 1955. The effect of education on migration. *Columbia*.
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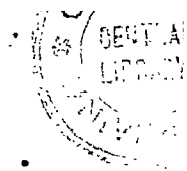
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## RESEARCH EXPENDITURES, EDUCATION, AND THE AGGREGATE AGRICULTURAL PRODUCTION FUNCTION

By ZVI GRILICHES\*

Two previous papers [5] [6] summarized the results of estimating an aggregate agricultural production function based on 1949 data for 68 regions of the United States. The main findings of that study were the importance and significance of education as a factor affecting output and an indication of the existence of substantial economies of scale in agriculture. The present paper extends this study to cover the years 1949, 1954, and 1959, using per-farm state averages as its units of observation, and introduces explicitly the level of public expenditures on agricultural research and extension (the dissemination of research results) as a variable in the aggregate production function. The results of the current study indicate that these expenditures affect the level of agricultural output "significantly" and that their social rate of return is quite high.

### *I. The Approach and the Data*

The basic approach of this paper consists of estimating an unrestricted production function of the Cobb-Douglas type, using separate variables for each of the five major input categories, and introducing, in addition, a measure of education per worker and a measure of public expenditures on research and extension (*R & E*) per farm into the estimating equation. The data used are derived mainly from various U.S. Department of Agriculture publications; they are expressed as per-farm averages for 39 "states,"<sup>1</sup> and deflated by various

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<sup>1</sup> Because of lack of data (mainly for the labor variable), some of the smaller states were aggregated into larger units. Thus all of New England was treated as one observation, and so also were Delaware and Maryland, New Mexico and Arizona, and Wyoming-Utah-Nevada.

national price indexes with 1949 (=100) as a base.<sup>2</sup>

This study differs from the previous one in that it investigates three cross sections of data simultaneously. This allows it to ask questions which could not be answered by the previous cross-section study. On the other hand, the 1949 study was based on data for 68 "productivity regions," while this study has to restrict itself to observing only 39 "states" at any one point of time. The lower number and the conglomerate nature of these states reduce sharply the observed range of variation in some of the "independent" variables and, hence, also the sharpness with which their individual influence on output can be estimated.

This study differs also from the previous one in the definition and measurement of some of the variables. It treats fertilizer as a separate variable (measuring it by weighted plant nutrients rather than expenditures), while combining other current expenditures with expenditures on purchased livestock and feed. It uses also a measure of land in constant prices which is free from contemporaneous influences of trends in product prices or urbanization and a measure of machinery services based on the aggregation of inventory data on individual machines, using prices of "new" machines as weights. The latter procedure results in a machinery-services measure based on "gross stock" concepts.<sup>3</sup>

## II. *A Digression on the Elasticity of Substitution*

Before we examine the main results of this study, it is worth while to investigate whether there is strong a priori evidence against the assumption of a unitary elasticity of substitution between labor and all other inputs implicit in the Cobb-Douglas form of the production function. This question can be investigated by fitting a log-linear relationship between output (or value-added) per man and the wage rate. Given the assumption of a constant elasticity of substitution (CES) production function [1], competitive product and factor markets and equilibrium, correct measurement of all the variables, and an exogenous wage rate, the slope coefficient in this equation is an estimate of the elasticity of substitution between labor and all other capital inputs (if value-added is used as the output variable).

The results of fitting such a relationship to various combinations of years and different forms of the equation and data are summarized in Table 1. The direct estimate of this equation (lines 1a, 2a, 3a) results in coefficients that are somewhat above unity. The other lines in Ta-

<sup>2</sup> See the data appendix for a more detailed discussion of definitions and sources.

<sup>3</sup> See [6] for a more detailed discussion of problems associated with the measurement of machinery services for productivity comparisons.

TABLE 1 — ESTIMATES OF THE ELASTICITY OF SUBSTITUTION IN U.S. AGRICULTURE  
(COL. 1) BASED ON DATA FOR 39 "STATES" AND THE YEARS 1949, 1954, AND 1959

Years and Method	Coefficients of					R <sup>2</sup>	Residual Stand- ard Error
	Wage Rate	Educa- tion	Time Dummies		Lagged Value- Added per Man		
			1954	1959			
1a. 1949-54-59 Combined	1.212 (.068)		.028 (.020)	.046 (.022)		.802	.087
1b.	.745 (.133)	1.476 (.369)	.038 (.019)	.070 (.022)		.827	.082
2a. 1949-54	1.324 (.075)		.020 (.019)			.824	.080
2b.	1.005 (.160)	.995 (.445)	.027 (.019)			.835	.078
3a. 1954-59	1.226 (.087)			.017 (.022)		.752	.092
3b.	.465* (.129)			.010 (.020)	.600 (.086)	.850	.072
4. First differences: 1959-54 and 1954- 49 combined	1.140 (.250)			-.010 (.020)		.221	.072
5a. 1954	1.131 (.240)	1.029 (.691)				.851	.078
5b. 1954 I.V.	1.041 (.299)	1.265 (.843)				.820	.086

The basic equation estimated is of the form  $\log (VA/L) = a + b \log W + Zc + u$ , where  $VA$  is "real" value-added equaling sales plus inventory change plus home consumption plus government payments minus all current purchases of goods and services (except labor) from both outside and inside the sector, all individually deflated (1949=100) before subtraction.

$L$  is a man-days-worked measure of labor.

$W$  is the "real" wage rate per hour: composite hourly wage rate for hired farm labor, deflated in each state separately by the implicit output deflator.

$Zc$  is a shorthand notation for a matrix of other variables and the associated coefficient vector;  $u$  is a disturbance, usually assumed to be random and uncorrelated with the "independent" variables.

"Education" is a measure of formal schooling per male in the rural farm population, constructed by weighting the distribution of males by school years completed within each state by all U.S. mean incomes in each of these categories. See the data appendix and [6] for more details on the derivation of this and other variables.

"Time dummies" are shift variables that take the value of one for all states in their "reference" year and zero in all the others. Their introduction allows each cross section to have a mean level (intercept) of its own, while preserving the equality of slope coefficients. They allow for common time trends (and measurement errors) in the mean level of all the variables. Under the assumption of the CES model and neutral technical change, the coefficients of the dummies are an estimate of  $(1-\sigma)\lambda_t$ , where  $\sigma$  is the elasticity of substitution and  $\lambda_t$  is the rate of technical advance relative to the base period (in this case 1949). If one assumes that  $\lambda > 0$ , the slightly positive coefficients imply a  $\sigma$  of somewhat less than unity.

Lagged value-added per man is the lagged value of the dependent variable from the preceding cross section. That is, the lag is five years.

I.V. are estimates based on the method of instrumental variables, using the lagged ( $t-5$ ) wage rate instead of the current one as an instrumental variable.

The number of observations equals 117 for set 1, 78 each for sets 2, 3, and 4, and 39 for set 5. The numbers in parentheses are the estimated standard errors of the respective coefficients.

ble 1 reflect various attempts to deal with possible sources of bias in this estimate. An attempt to take into account differences in the quality of labor in different areas (the neglect of which would bias the estimated coefficient towards unity) by introducing a measure of education per man into the equation does reduce the estimated elasticity of substitution, but not consistently or significantly below unity. The possibility that the farm labor market is not in instantaneous equilibrium is investigated by fitting a distributed lag model of the Koyck-Nerlove type (line 3b). The resulting "long run" estimate of the elasticity of substitution is again above unity (but not significantly so). The possibility that the relationship may be due to the effect of omitting some regionally stable variables is investigated by fitting the same equation in the form of first differences, relating *changes* between cross sections in output per man to changes in the wage rate (line 4). Finally, the possibility that the results may be due to simultaneous-equations bias, since the farm wage rate may in its turn be affected by the same forces that determine the level of output per man, is investigated by fitting the same equation, using the method of instrumental variables (and the lagged real wage rate as the instrumental variable) instead of the method of least squares (lines 5a and 5b). Neither of these alternative forms of the model results in estimates of the elasticity of substitution which are significantly different from unity. Thus, at this level of aggregation, there does not seem to be any strong *prima facie* evidence against the Cobb-Douglas form, which will therefore be used in the subsequent discussion.

### III. *The Main Results*

The main results of fitting a Cobb-Douglas-type production function to data on per-farm output and input averages for 39 states are summarized in Table 2. Preliminary investigations showed that estimates for different cross sections do not differ significantly and, hence, most of the discussion that follows will be based on results obtained by combining the data from several cross sections and imposing the assumption of constancy of coefficients throughout the 1949-59 period.<sup>4</sup>

<sup>4</sup>Equations comparable to those reported in columns 3-6 of Table 2, but without the time dummies, were computed for each of the three cross sections separately. Using the standard normality and independence assumptions, one could not reject the null hypothesis that the separate estimates for 1949 and 1954 did not differ "significantly" (at conventional significance levels) from the joint estimates for 1949-54. The same was also true for the 1954 and 1959 with 1954-59 comparison. (The computed F statistic is 1.1 for the 1949-54 comparison and 2.0 for the 1954-59 comparison. Given the usual assumptions the "critical" value of the F statistic, with 7 and 64 degrees of freedom, is about 2.3 at the .05 and 3.5 at the .01 significance levels.) The results are somewhat weaker but similar for the over-all comparison of all three separate equations with the pooled (but without time dummies) 1949-54-59

The major findings of the previous study are confirmed by the results of this study. The education of the farm labor force is again a "significant" variable, entering the equation with a coefficient which is not significantly different from that of the labor variable.<sup>5</sup> Thus one can combine both the quantity and quality dimensions of labor into one variable, which simplifies some of the subsequent analysis. Again, this study finds evidence of substantial economies of scale. The sum of the estimated coefficients (excluding those of education, research and extension, and time dummies) is consistently above unity with 1.2 being a reasonable point estimate.<sup>6</sup> Nor is this finding contradicted when an attempt is made to guard against the possibility of simultaneous-equations bias using the method of instrumental variables to estimate the coefficients of the production function (col. 7 of Table 2). In general, the results of using the instrumental variables approach for both the 1954 and 1959 cross sections (the latter results are not reproduced here) differ little from those based on single-equation least-squares procedures.<sup>7</sup>

The main novelty of this study is the introduction of a variable to reflect the contribution of public expenditures on agricultural research and on the dissemination of its results on the level of agricultural productivity in different states. The measure used is quite crude: it is the sum of all the relevant entries in the annual budget of the respective state agricultural experiment stations and extension services. To allow for some lags in the effect of these expenditures, this variable was defined as an average of the flow of expenditures in the previous year and the level six years previously. Thus, the average of 1958 and 1953 is used in the 1959 cross section, the average of 1953 and 1948 is used in the 1954 cross section, and the average of 1948 and

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equations. (The computed F statistic is now 2.1, while the "critical" value of this statistic, with 14 and 96 degrees of freedom, is about 2.0 at the .05 and 2.4 at the .01 level.) See [3, Ch. 6] for details of such tests.

<sup>5</sup> The computed F statistic for the test of equality of the two coefficients for the 1954-59 cross section is 2.3. The "critical" value of F with 1 and 70 degrees of freedom is at least 4.0, even at the .05 level. The results for other cross sections are similar.

<sup>6</sup> Taking the lower estimate of the sum of the coefficients in column 3 of Table 2, we can construct a test for its difference from unity by computing the variance of this sum. In this case, it is just the sum of the variances plus twice the sum of the covariances of all the relevant coefficients. The standard deviation of the sum of the coefficients is 0.0434. Under the null hypothesis and the standard normality and independence assumptions, the ratio of the difference of the sum from unity to its standard error  $.19715/.0434 = 4.54$  has the "t" distribution with 108 degrees of freedom. The probability of observing a ratio as high as 4.54 if the true sum were equal to unity would be less than one in ten thousand. Thus the excess over unity is "highly significant."

<sup>7</sup> Since lagged inputs are used as instrumental variables, the implicit reduced forms have very high  $R^2$ s, and thus the substitution of a "predicted" for the actual input series (in two-stage least-squares language) makes very little difference.

TABLE 2—ESTIMATES OF THE AGGREGATE AGRICULTURAL PRODUCTION FUNCTION,  
U.S., 1949-54-59

Coefficients of	1949-54-59			1949-54 1954-59		1954	
						I.V.	
"Other"	.393 (.024)	.367 (.028)	.342 (.027)	.312 (.035)	.371 (.034)	.345 (.056)	.349 (.082)
Land & Bldg.	.152 (.022)	.146 (.022)	.145 (.021)	.186 (.028)	.119 (.026)	.157 (.043)	.158 (.058)
Fertilizer	.107 (.012)	.100 (.013)	.095 (.013)	.101 (.016)	.120 (.019)	.140 (.030)	.144 (.047)
Machinery	.200 (.046)	.158 (.037)	.164 (.034)	.188 (.045)	.185 (.043)	.241 (.072)	.250 (.104)
Labor	.426 (.051)	.511 (.060)					
Education		.405 (.161)					
Lab.×Educ.			.448 (.063)	.415 (.084)	.398 (.077)	.364 (.123)	.311 (.189)
Res. & Ext.			.059 (.021)	.052 (.027)	.062 (.027)	.044 (.043)	.050 (.061)
T 54	-.011 (.011)	-.003 (.011)	-.017 (.012)	-.019 (.014)			
T 59	.005 (.014)	.019 (.015)	-.006 (.017)		-.002 (.020)		
R <sup>2</sup>	.980	.981	.983	.981	.980	.977	.959
S.E.	.036	.035	.034	.035	.035	.039	.052
Sum of Coeff.	1.278	1.282	1.197	1.202	1.193	1.247	1.212

The equation estimated is linear in the logarithms of the variables (except for the time dummies). The output and input variables are per-farm state averages. A total of 39 "states" is used in each cross section, some of these "states" being a combination of several smaller adjoining states (see footnote 1 for details).

Output equals value of sales, inventory change, home consumption, and government payments. The separate components are deflated in detail by U.S. price indexes with 1949=100.

Land and Buildings: The value of buildings is computed as the product of the percentage of value that is due to buildings and the total value of land and buildings per farm and deflated by an index of building materials prices with 1949=100. "Land" is a constant price volume measure constructed by David Boyne [2], using 1940 relative prices of different categories of land in each state and the changing number of acres in these categories in the subsequent years. It is inflated to add up in 1949 (for the U.S. total) to the total value of land and buildings. The resulting measure of land takes into account quality differences in land at 1940 relative state prices, but does not allow subsequent appreciation due to product price

1945 (since the data for 1943 were not strictly comparable) is used in the 1949 cross section. Given the crudeness of this measure, it is surprising that it is as significant as it is. Also, its coefficient remains remarkably stable when cross sections are added or subtracted, and when other variables are introduced or the measurement and definition of included variables are changed. While the estimated coefficient of R & E may appear to be rather small—a doubling in the level of public R & E expenditures per farm would lead to an increase in output of only about 5 per cent—the implied absolute effect is very large. The average (geometric) public expenditure on R & E during the whole 1949-54-59 period was only about \$32 per farm per year, while the average gross output per farm (in 1949 prices) was \$7,205 per year during the same period. Using 0.059 for the coefficient of

trends and other changes to be reflected in the “quantity” measure. The result is a measure of the *stock* of land and buildings per farm.

**Machinery:** A measure of the stock of machinery is constructed by valuing a relatively detailed list of machines on farms by the average U.S. price for such new machines in 1949, allowing for some regional differences in the average price of a machine, due to regional differences in the average size of machines, where data were available. The result is an estimate of the gross (undepreciated) stock of machines on farms in 1949 prices. This is converted into a flow measure by multiplying it by 0.15 (the fraction that flow is of gross stock under the assumption of an average length of life of 10 years and an 8 per cent rate of interest) and added to the deflated values of gasoline, oil, and other machinery operation and repair expenditures to arrive at an estimate of the total flow of services associated with the use of farm machinery.

**Fertilizer:** weighted plant nutrients. The sum of nitrogen, phosphoric acid, and potash consumption weighted by 1955 relative prices for these ingredients (1.62, .93, and .45 respectively).

**Other:** The sum of deflated expenditures on purchased feed and livestock, interest on the livestock inventory (at 8 per cent), and expenditures on seed and other current inputs not elsewhere classified.

**Labor:** A measure of labor-days-worked constructed by adding together total expenditures on hired workers divided by the average wage rate per day and the number of family workers times the average number of days worked on the farm (constructed by subtracting from 300 the estimated number of days worked off the farm by operators). Additional adjustments were made for operators over 65 years of age (they were entered with a weight of 0.6) and unpaid family workers (who were entered with a weight of 0.65).

**Education:** weighted school years per man (25 years and older in the rural farm population), using 1950 mean incomes for the respective education categories (for all U.S. males) as weights. See [6] for additional details on the construction of this variable.

**Research and Extension:** undeflated sum of total expenditures on research and extension by the respective agricultural experiment stations and extension services based on all sources of funds (including USDA appropriations). Averages for the previous year and five years previously, except for the 1949 cross section where these expenditures are averages for 1948 and 1945.

Time dummies take the value of unity for observations in the specified year and zero for all the other observations.

**I.V.:** coefficients estimated by using the method of instrumental variables, assuming that all the “current” input variables (“other,” fertilizer, and labor) are endogenous, and by using the current wage rate and the lagged values of these inputs from the previous cross section (i.e. at  $t-5$ ) as instrumental variables.

The “sum of the coefficients” excludes the coefficients of the education, research and extension, and time-dummies variables.

R & E (from column 3, Table 2) leads to an estimate of its marginal product of  $.059 \times 7,205/32$  or approximately 13 dollars of output per year for an additional dollar of R & E expenditures per year. This finding implies the fantastically high gross rate of return of about 1300 per cent for social investment in agricultural research and extension. Even if one allows that much of it is the result of research expenditures by private firms (mainly in the agricultural supplies industries),<sup>8</sup> and that, due to our inability to solve the agricultural problem, the social value of additional agricultural output is only about half of its market value, the gross social rate of return to R & E expenditures is still about 300 per cent. This last figure is of the same order of magnitude as a previous estimate (in [4]) of a 35 to 170 per cent *net* social rate of return to agricultural research based on entirely different data and a different approach.

The estimated average "marginal products" of all the inputs in 1949 and 1959 are presented in Table 3. In general, they do not appear to conflict with other information that we have on the economic conditions and economic history of this industry. For the inputs that are measured in units of dollars per year, one would expect marginal productivity coefficients on the order of 1.2, given the previous finding of economies of scale of about this order and the assumption that the marginal products, while not equal, are still proportional to marginal costs.<sup>9</sup> This is, in fact, approximately the order of magnitude of the numbers in columns 1, 4, and 6 of Table 3. The only two columns that seem to imply substantial disequilibrium at both the beginning and the end of the period are the fertilizer and the research and extension columns. A ton of weighted plant nutrients cost about \$250 throughout most of this period, implying a ratio of the marginal product to factor price between 3 and 5. Farmers have not remained idle in the face of such a disequilibrium. Between 1949 and 1959 fertilizer consumption grew at the tremendous rate of 7.4 per cent per year (more than doubling fertilizer consumption during this period) and continued to grow at the rate of 5.6 per cent per year between 1959 and 1962. The estimated equilibrium gap (VMP/factor price) has declined from about 5 in 1949 to 2.7 in 1959 and 2.4 in 1962 (using the estimated 1954-59 coefficient). But even if fertilizer consumption continues to grow at about the same rate, relative to output, as it did during the 1959-1962

<sup>8</sup> Assuming that private expenditures are perfectly correlated with public expenditures and are of about the same order of magnitude (see [4] for some support for the last statement).

<sup>9</sup> If there are economies of scale, the adding-up or product-exhaustion theorem doesn't hold. Factors cannot all be paid their marginal product, since this would overexhaust the available total.

TABLE 3—ESTIMATED AGGREGATE MARGINAL PRODUCTS IN U.S. AGRICULTURE (IN 1949 PRICES) IN 1949 AND 1959

Year	Marginal Product of						
	"Other"	Land & Bldgs.	Fertilizer	Machinery	Labor	Education	R & E
1949	1.22 (.14)	.12 (.02)	1267 (200)	1.26 (.30)	5.76 (1.17)	.85 (.17)	16 (8)
1959	1.25 (.11)	.10 (.02)	697 (110)	1.18 (.27)	10.43 (2.02)	1.30 (.25)	10 (4)

Computed by multiplying the appropriate geometric average output ratio by the respective input coefficient from Table 2 (column 4 for 1949 and column 5 for 1959). The numbers in brackets are a crude translation of the estimated standard errors from Table 2 into comparable units. The units are (from right to left): dollars per dollar (both per year) for columns 1, 4, 6, and 7; dollars per year per dollar or per cent per year for column 2; dollars per weighted plant nutrient ton for column 3; and dollars per day for column 5.

period (4 per cent per year), the disequilibrium is not likely to disappear before the early 1980's unless there are some new price and technological developments. At any rate, the regression estimates confirm the existence of this disequilibrium, while the observed market behavior reflects the producers' attempt to reduce it.

Public research and extension expenditures are, of course, outside the farmers' control (at least directly). Thus there is no obvious way for them to eliminate this disequilibrium except through individual attempts to acquire more of the products of this activity, which are largely free to them (except for the nonnegligible monetary and psychological cost of doing so). But even if we make the fourfold downward adjustment discussed above and also adjust for the decline in farm product prices since 1949 (by 4 per cent), we are still left with a social disequilibrium gap of over 2 (or a *net* social rate of return of more than 100 per cent). Thus, perhaps, we should not be so quick to cut the agricultural research appropriations in our attempt to solve the farm problem.

The education variable is measured in dollars per year that a comparable average education mix would fetch in the nonfarm sector. When detailed adjustments are made to bring the farm and nonfarm labor force measures into comparable units, as in [7], it is found that a ratio of about .7 of farm to urban per capita income is consistent with equal real returns for comparable labor. Thus, a marginal product of about .7 or so, which would be in units of farm dollars per urban dollars, would indicate an approximate equilibrium with respect to this variable. Since the estimated coefficients are higher than that,

though probably not significantly so, there is some indication that the marginal return to quality of labor in agriculture may actually exceed its opportunity cost in the nonagricultural sector.

One of the findings of the previous study was that the estimated labor coefficient appeared to be lower and the coefficient of machinery higher than the coefficients that would be implied by the factor-shares approach. This finding is not confirmed by this study. When the estimated factor shares are adjusted for the difference in the concept of output used in this study and the estimated production function coefficients are divided by their sum (to add up to unity), the two sets of coefficients are quite close to each other, and the differences that remain are not statistically significant. Thus, while the previous findings would have implied a marginal product of labor below the going wage rate, when the estimates in Table 2 are adjusted for economies of scale and for the minor decline in farm-product prices between 1949 and 1959, the resulting marginal product of labor estimates of \$4.80 and \$8.30 per day are not significantly different from the U.S. average wage rate of hired farm workers of \$4.40 and \$6.50 per day in 1949 and 1959 respectively.<sup>10</sup>

If all the variables were measured correctly, the coefficients of the time dummies would be estimates of the rate of "disembodied" technical change (relative to 1949). In practice, however, they also reflect errors of measurement in the average level of variables. Hence, the fact that they are almost never significantly different from zero may be the result of underdeflating some of the input series (or, for example, not deflating the R & E expenditures at all). On the other hand, an attempt has been made to incorporate as many adjustments for quality change as possible into the measurement of inputs and, hence, one might expect that only a very small role would be assigned to disembodied technical change. It would be beyond the scope of this paper to repeat the analysis of sources of growth presented in [6]. But if one uses the estimated coefficients from Table 2 and data on input and output change over time, it is possible to account for *all* of the observed growth in agricultural output without invoking the unexplained concept of (residual) technical change. Table 4 summarizes an analysis of the "residual," which parallels closely the results presented previously in Tables 4 and 7 of [6]. As against the previous study, the current results reduce somewhat the role of economies of scale, increase somewhat the role of education and other input quality change, and

<sup>10</sup> The differences that remain may be due to the lower-than-average quality of the workers (the hired ones) for whom the market price is measured. Actually, about two-thirds of the total work is done by family members, whose average education and also motivation are higher than those of the hired farm labor force.

TABLE 4—SOURCES OF THE "RESIDUAL": U.S. AGRICULTURE, 1949-1959

1. <i>Residual</i>		<i>Per cent change</i>
a. Official, adjusted		28
b. This study		-1
2. <i>Sources of difference:</i>		<i>Common logarithms</i>
a. Difference in output measure		.0096
b. Difference in weights		.0121
c. Difference in conventional input measures		.0105
d. Contribution of education		.0146
e. Contribution of R & E		.0297
f. Economies of scale in conventional inputs		.0320
Sum		.1085
Residual as in 1a		.1065
Difference (—)		.0020

1a. From [8]. Adjusted for difference in output concept, and inputs weighted geometrically.

1b. Computed using the weights from column 3 of Table 2 and the differences in the geometric means of all the variables between 1949 and 1959.

2b. Adjusting the regression weights to add up to unity. Logarithms are used in the second half of the table, since the items are not additive otherwise.

assign for the first time a substantial role to the previously unmeasured contribution of public investment in agricultural research and extension to the explanation of the growth in the aggregate output of agriculture. Taking items 2c through 2f as what one could have called the "residual" in this study, the total can be divided into three roughly equal parts: the contribution of input quality change (in labor and other inputs), of economies of scale, and of investments in research and extension.

#### IV. Conclusions

"Productivity," "technical change," or the "residual" are usually computed residually, as part of some accounting framework. Because of the nature of the beast, it is impossible to test *accounting* frameworks statistically. One may discuss the "usefulness" of various accounting schemes, as I have done in [6], where I suggest using a framework that *minimizes* the role of the unexplained residual, but the argument eventually reduces itself to aesthetics and research strategy. It is possible, however, to test some of the components of an accounting scheme separately if data which these concepts do not have to fit tautologically are used. The empirical work on aggregate production functions reported here has been motivated by a desire to gather statistical evidence for various theoretically justifiable adjustments to the conventional accounting framework. The study has been designed to investigate whether there is evidence for introducing a

measure of education and a measure of research investment into the accounting scheme for growth and whether there is evidence for relaxing the assumption of constant returns to scale. None of these questions can be answered unequivocally by looking only at one piece of data. They are too interdependent. This is why one must repeat the analysis on different sets of data, some of them being better suited for illuminating one or another aspect of the problem.

In this paper I have reproduced some of the previously reported results on a new set of data. "Education" does belong in the production function, and there do appear to be substantial economies of scale in agriculture. A new variable, public investment in research and extension, is introduced and found to be both "significant" and important as a source of aggregate output growth. Using the estimated coefficients as weights, and the expanded and adjusted list of inputs, one can again account for most of the observed growth without being left with a large unexplained "residual." None of these conclusions is very firmly established, and some may be subject to substantial bias,<sup>11</sup> but the only known way of either confirming or disproving them is the slow and expensive but cumulative process of conducting additional studies of this type on different bodies of data.

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<sup>11</sup> The finding of economies of scale is particularly susceptible to bias due to incomplete or incorrect accounting. If not all of the inputs are adjusted for quality differences, and they are not, and if larger size farms tend to use higher quality inputs, then some of what is measured as economies of scale are actually due to differences in the quality of inputs. Also, these economies may be the reflection of discontinuities in the range of available machine sizes with the resultant nonhomogeneity of the production function. In either case, the accounting is still correct, but the interpretation of a particular category is less firm.

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#### APPENDIX

##### *Data Sources*

A complete description of the data used in this study will be given in a publication presenting a more extended version of the results of this study. Only the most important adjustments are described below.

Most of the data on output and input by states are taken from USDA, *Farm Income: A Supplement to the Farm Income Situation for July 1961, State Estimates 1949-1960*, Washington, August 1961. Output is defined as the sum of cash receipts from farm marketings, government payments, value of home consumption, and net change in farm inventories. The first item was divided into 12 commodity groups (such as meat animals, dairy products, poultry and eggs, feed crops, food grains, etc.) and each was deflated separately by an appropriate U.S. price index (1949 = 100). In a few cases where one particular commodity accounted for a large fraction of total receipts in a state, the deflation was done on an even finer commodity level. The price indexes used are components of the Prices Received by Farmers Index and were taken from various issues of *Agricultural Prices*. The resulting procedure produced an implicit farm-marketings deflator for each state. This deflator was used in turn to deflate the government payments, value of home consumption, and net inventory change categories.

The following items were also taken from the *Farm Income* publication: Current expenses on (1) feed, (2) livestock, (3) seed, (4) repairs and operation of capital items (excluding buildings), (5) miscellaneous, and (6) hired labor. Unpublished data supplied by the U.S. Department of Agriculture were used to exclude the building component from (4). Each of these components was deflated separately by the appropriate U.S. subindex of the Prices Paid by Farmers Index taken from USDA, *Prices Paid by Farmers . . . , 1910-1960*, Stat. Bull. No. 319; except for (6)—hired labor—which was “deflated” separately by dividing it through by the average wage rate per day paid to hired farm labor in *each* state to arrive at a “days worked” measure. These wage rates and the hourly ones used explicitly as a variable in the regressions summarized in Table 1 were taken from various issues of *Farm Labor*.

The “other” inputs variable was constructed as the sum of items (1), (2), (3), (5) and interest (8 per cent) on the constant price value of livestock and crop inventories on farms. The latter item was estimated by multiplying the physical data on the inventory of different types of livestock and different crops stored on farms by their average 1949 U.S. prices.

The construction of the land and buildings, machinery, and fertilizer variables is described in the notes to Table 2. The data on the inventory of machines on farms come from USDA, *Numbers of Selected Machines and Equipment on Farms*, Stat. Bull. 258, 1960, and the respective volumes of the *Census of Agriculture* for 1949, 1954, and 1959. The prices used to value

this inventory were derived largely from USDA, *Farmers' Expenditures for Motor Vehicles and Machinery . . . 1955*, Stat. Bull. 243, 1959, and converted to 1949 levels by using the appropriate series from the previously cited *Prices Paid* publication. The data on plant-nutrient use by states were taken from various reports of the Soil and Water Conservation Branch of the USDA.

The labor measure, in days, equals

$$D\{N(1 - 0.4A) + 0.65(F - N)\} + HE/W$$

where  $D$  is the estimated average days worked on farms by operators (estimated by subtracting from 300 the calculated average number of days worked off the farm by operators),  $N$  is the number of farms, assumed also to equal the number of farm operators,  $A$  is the fraction of operators above age 65,  $F$  is the total number of family workers,  $N - F$  is thus the estimated number of unpaid family workers (set to zero in the few cases where  $F > N$ ), and  $HE/W$  is the estimated number of hired worker man-days (hired worker expense divided by the wage rate per day).  $N$  and  $F$  are taken from various issues of *Farm Labor*. " $A$ " and the data on off-farm work come from the *Census of Agriculture* volumes for the respective years. The implicit weighting of operators over 65 at 0.6 and of unpaid family workers at 0.65 is the same as in [6] and is based on E. G. Strand and E. O. Heady, *Productivity of Resources Used on Commercial Farms*, USDA Tech. Bull. 1128, November 1955.

The education variable was based on the distribution of the male rural farm population by school years completed taken from the 1950 and 1960 *Census of Population*. These distributions were weighted by the mean U.S. income for males (over 25) in 1950, based on 1950 Census of Population data. See [6] for additional details. 1954 was approximated by a simple interpolation between 1950 and 1960.

The data on research and extension expenditures are taken largely from Dana G. Dalrymple, *State Appropriations for Agricultural Research and Extension*, Mich. State Univ. Department of Ag. Econ., A.E. 852, Dec. 1961 and the annual reports on "Cooperative Extension Work in Agriculture . . ." and "Agricultural Experiment Stations" by the USDA Office of Agricultural Experiment Stations.

All variables, except education, are divided by  $N$  to arrive at per-farm units.

## SHIFTS IN THE COMPOSITION OF DEMAND AND THE INFLATION PROBLEM

*By* W. G. BOWEN AND S. H. MASTERS\*

In recent years a number of persons have suggested that shifts in the composition of demand play a role in the inflationary process. More specifically, the usual hypothesis is that, other things equal, the over-all price level is likely to rise more rapidly, the greater the extent of shifts in the composition of demand. This hypothesis is based on two separate propositions concerning price and wage behavior, one having to do with the shape of supply schedules and the other pertaining to collective bargaining and wage behavior. In brief, the supply-schedule proposition is that there is a ratchet effect, that prices of all kinds are more flexible upwards than downwards; the wage-determination proposition is that "large" increases in wages are likely to occur in rapidly expanding sectors and that these increases will be transmitted, at least in part, to other sectors.<sup>1</sup>

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<sup>1</sup> To elaborate just a bit, for the benefit of readers who have not followed the literature dealing with this topic, the shape-of-the-supply-schedule aspect of the argument asserts that in imperfectly competitive markets decreases in demand are unlikely to result in price reductions, whereas increases in demand may well entail increases in marginal costs and thus price increases. In short, the typical supply schedule is alleged to be horizontal over the range of output below the current output level and to slope upward as capacity output is approached. The greater the extent of shifts in the composition of demand, the greater the upward thrust to the over-all price level imparted by this ratchet-type mechanism. The wage-determination aspect of the argument postulates the following sequence of events: (1) rapid shifts in the composition of demand produce increases in profits in the expanding sectors, which in turn increase union wage demands, lower employer resistance (by raising the cost of strikes), and thus lead to relatively large wage increases in this sector; (2) because of the importance of wage comparisons to unions and companies, these "large" increases in wages will be transmitted (at least in part) to other sectors, with the result that cost schedules in both sectors are shifted upward, assuming that the average wage increase exceeds the average increase in output per man-hour. Finally, it is often assumed that the monetary-fiscal authorities take whatever expansionary actions are necessary to prevent total output and employment from declining as a consequence of these upward shifts in cost schedules. While much can be (and has been) said about the plausibility of these various propositions, this discussion falls outside the scope of our paper.

Charles Schultze, in his Joint Economic Committee paper on the 1955-57 inflation, has supplied what is probably the best-known statement of this general line of reasoning. In addition to developing the main lines of the demand-shift *hypothesis* (as sketched above), he attempted to use this hypothesis as a partial explanation for price behavior during 1955-57 by adding the *assertion* that in fact shifts in the composition of demand were unusually pronounced during this period.<sup>2</sup> To the best of our knowledge, no systematic effort has been made to test this empirical assertion by actually trying to estimate the extent of shifts in the composition of demand from one period to another, let alone to test the underlying demand-shift hypothesis.

It is to the limited task of measuring the extent of shifts in the composition of demand within different periods, and seeing if indeed shifts in the composition of demand were unusually pronounced during the 1955-57 period, that this paper is addressed. We wish to emphasize that we are not testing the validity of the demand-shift hypothesis itself. This hypothesis is, of course, a *ceteris paribus* hypothesis and can be tested properly only in the context of a complete model of the inflationary process. A rigorous test would need to employ multivariate analysis in order to isolate the influence of demand shifts from such other factors as the balance between aggregate demand and supply, the state of money markets, and the wage and price practices of particular groups of unions and firms. We hope that by calling attention to the need for more careful empirical work along this line, by suggesting some simple aggregative measures of demand shifts, and by presenting what we consider to be some interesting differences between periods in the extent of demand shifts, we shall encourage (or provoke) others to do the more extensive type of research that is necessary.<sup>3</sup>

The two main empirical conclusions of this paper can be summarized as follows. First, the 1955-57 period was characterized by shifts in the composition of demand which were well above average in magnitude, compared with other postwar periods, though not extraordinarily large in the sense of being separated from the results for all periods by a wide gulf. Second, we also discovered, as an admittedly accidental by-product of our work, that the period between 1957 and 1963 has been characterized by abnormally small shifts in the composition of demand. If one accepts the demand-shift hypothesis, this last finding has in-

<sup>2</sup> "Recent Inflation in the United States," Study Paper No. 1, Joint Economic Committee, *Study of Employment, Growth, and Price Levels*, September 1959, pp. 8-10 and 44 ff.

<sup>3</sup> One word of warning is in order: anyone attempting to use multiple regression techniques in a time series analysis is faced with a serious degrees-of-freedom problem if he defines his observation periods in terms of relatively long intervals such as 1955-57. (Our criterion for defining periods is discussed later in this paper.)

interesting implications for the way in which we view the relatively moderate increase in the general price level that has occurred in the more recent period. However, before saying anything more about the meaning of our findings, it is well to describe the measures used and the statistical results.

### *Concepts, Measures, and Methods*

Estimating *ex ante* shifts in the composition of demand on the basis of *ex post* data is not easy. We rely mainly on two simple measures, the first of which is an index of sales dispersion labeled  $I_t$ , which for any given period  $t$  is defined as:

$$(1) \quad I_t = \sum_{i=1}^n |\bar{S}_t - \dot{S}_{it}| W_{it}, \quad \text{where}$$

$\dot{S}_{it}$  = the rate of change of dollar sales in the  $i$ th industry from the beginning to the end of period  $t$ , expressed at an annual rate by the usual compounding formula;

$W_{it}$  = an income-originating weight for the  $i$ th industry, obtained by dividing the income originating in industry  $i$  during period  $t$  by the total income originating in all  $n$  industries during period  $t$ ;

$\bar{S}_t = \sum_{i=1}^n \dot{S}_{it} W_{it}$ , a weighted average of all the individual industry

rates of change of sales.

The reason for using interindustry differences in rates of change of sales to represent interindustry shifts in the composition of demand is that shifts in demand are likely to evoke relative price and relative output responses, both of which are taken into account by the sales measure.<sup>4</sup>

Individual industry deviations from the all-industry average rate of change of sales are multiplied by income-originating weights for two reasons. First, the usual indices of the general price level are, of course, weighted measures, and we can expect at least a rough correspondence between weights of income originating by industry and the weights

<sup>4</sup> We recognize that interindustry differences in sales behavior can result from supply-side changes (especially differing rates of productivity gain) as well as demand-side changes, but there is no simple way to separate the two effects. This is somewhat less troublesome than it might at first seem because interindustry differences in productivity gains can themselves lead to interindustry differences in profits, and thus to the same wage-determination process postulated in the demand-shift model. Edwin Dale has in fact suggested a model of the inflationary process much like the demand-shift model, except that it is differential rates of productivity gain which provide the impetus for upward pressure on the price level. (See Dale's articles in the *New York Times*, March 10, 1957, pp. 1, 54, and May 19, 1957, pp. 1, 48; see also the critique by Fellner, in the "Letters" column of the March 25, 1957 issue.)

attached to various product classes in the construction of these indices. Second, as a general rule, we would expect the indirect price effects that are likely to result from transmission of industry *A*'s wage and cost increases to other industries to be greater, the greater the income originating in industry *A*. Thus, wage increases in the auto and steel industries, for instance, can be expected to have more of a direct effect on the usual price indices and to be more important in influencing other negotiations than wage increases in smaller industries. Ideally, we would have liked to include in our weighting scheme factors which reflect differences among industries in their own wage and price flexibilities in response to demand shifts; but no practical way of devising a suitable set of flexibility factors occurred to us.

Our second measure is nothing more than the weighted standard deviation of the rates-of-change-of-sales set of data, and is defined as:

$$(2) \quad I^* = \sqrt{\sum_{i=1}^n (\bar{S} - S_i)^2 W_{ii}}$$

The squaring operation used in the calculation of  $I^*$ , in effect assigns a more than proportionate weight to extreme deviations from the all-industry average rate of change of sales, and whether one prefers  $I^*$  or  $I_s$  depends on whether or not one's theory of wage-setting and pricing leads him to favor this greater emphasis on extreme values. We do not tend to prefer  $I^*$ , because we are inclined to think that downward rigidities and upward flexibilities are likely to have a more than proportionate impact within industries growing or declining at an unusual pace. Also, transmission effects may well bear a nonlinear relationship to the absolute deviations from the all-industry average rate of change of sales. We do not wish, however, to put much emphasis on this nonlinearity premise, and so we present both the  $I_s$  and the  $I^*$  measures.

Let us turn now from the measures themselves to the details of our computational procedures. Department of Commerce data for twenty-one two-digit industries are used as the basic sales data.<sup>5</sup> Indices of interindustry shifts are calculated for the six time periods shown in Table 1 (p. 979). As the reader can confirm by looking at a chart of the Federal Reserve Production Index, all of the terminal dates represent relative production peaks—no months of pronounced expansion or contraction in aggregate industrial production are used to mark off periods.<sup>6</sup> This approach to the dating of the periods is necessary be-

<sup>5</sup> A list of the 21 industries and exact references to data sources are included in Appendix A.

<sup>6</sup> The one possible exception to this rule is the terminal month for the July 1957-

TABLE 1—MEASURES OF INTERINDUSTRY SHIFTS IN THE COMPOSITION OF DEMAND, SELECTED PERIODS, 1947-63

Periods Measures	(1) Jan. 47- July 48	(2) July 48- Mar. 51	(3) Mar. 51- July 53	(4) July 53- Sept. 55	(5) Sept. 55- July 57	(6) July 57- Mar. 63
A. Actual Values						
$I_s$	4.96	3.94	4.76	2.46	5.34	1.57
$I^*$	7.19	5.00	5.96	3.17	6.34	1.94
B. Ranks <sup>a</sup>						
$I_s$	5	3	4	2	6	1
$I^*$	6	3	4	2	5	1

<sup>a</sup> A rank of "1" was assigned to the period with the lowest degree of dispersion, . . . , "6" to the period with the highest degree of dispersion.

cause industries differ significantly in their degree of sensitivity to cyclical swings in business activity. To take an extreme example, a period which started at a business-cycle peak and ended at a trough would exhibit pronounced shifts in output and sales composition due to cyclical considerations alone. We have tried to choose our terminal dates in such a way as to prevent cyclical influences from dominating interperiod differences in the dispersion indices.

### *Results and Interpretations*

Results for both measures are presented in Table 1. Period 5 (September 1955-July 1957) is the Schultze period, and we see that the  $I_s$  measure does take on its highest value during this period and the  $I^*$  measure its second highest value. Thus, if we pool all our observations (implicitly assigning equal stature to both measures and assuming that comparisons among all six periods are equally meaningful), we conclude that the Schultze period was characterized by shifts in the composition of demand which were well above average in magnitude.

A closer inspection of Table 1 suggests, however, that it may be somewhat misleading to treat all six periods as if they were observations drawn from the same universe, and that the six periods should be divided into two broad categories on the basis of the timing of postwar recessions. We know that periods (1), (3), and (5) were plateau-like periods characterized by relatively low unemployment and the absence of sharp downturns in economic activity; periods (2), (4), and (6) spanned recessions. Looking at the results in Table 1 from this perspective, we see that both of our measures of dispersion take on lower

March 1963 period. March 1963 cannot be considered a true production peak in light of the steady expansion in output that has occurred since then.

values in each of the three periods spanning recessions than in each of the three nonrecession periods. We have no firm explanation for this persistent pattern,<sup>7</sup> but its existence does suggest that perhaps it is more meaningful to compare 1955-57 with only the two other non-recession periods. When we limit the comparison in this way, the Schultze period still appears to be somewhat above average in terms of degree of sales dispersion, but the range among periods is reduced considerably. (It should be noted that the nonrecession period which vies with September 1955-July 1957 for the top ranking is January 1947-July 1948, a period when reconversion from a wartime to a peacetime economy was still under way.)

A stronger case can be made for September 1955-July 1957 as a nonrecession period characterized by unusually rapid shifts in the composition of demand if we take account of differences among periods in the over-all rate of increase in economic activity and work with *relative* measures of dispersion (measures which in effect deflate the absolute degree of dispersion among industries by the all-industry average rate of change in economic activity). However, finding a relative measure which makes sense in terms of one's a priori notions of wage and price behavior is difficult, and we have deferred a discussion of this matter to Appendix B.

We come now to our findings concerning the more recent period (period [6], measured from July 1957-March 1963). Here the two absolute measures of dispersion (and the relative measures as well) give identical ordinal results—shifts in the composition of demand were much smaller during this period than during any of the other postwar periods. We have been asked by several people who read an earlier draft of this paper if breaking this rather long period at May 1960 (the peak month following the 1959 recession) would affect these results. Additional calculations have been made for the two subperiods July 1957-May 1960 and May 1960-March 1963, and the results are entirely consistent with the results for the recent period taken as a whole. For both measures, the values for *each* of these two subperiods are smaller than the values for all five of the pre-July 1957 periods.

<sup>7</sup> As explained above, we tried to date our periods so as to avoid reflecting direct cyclical influences on dispersion in our measures. And there is no reason to expect (unsystematic) errors in our dating decisions to cause the nonrecession periods to show either more or less dispersion than the recession periods. The only hypothesis we have been able to develop is that consumers and businesses are more likely to rearrange patterns of expenditures when times are relatively good and there is a (previously uncommitted) margin above old expenditures to "play with" than when there is substantial unemployment. This is analogous to the argument that the federal government is fairly well committed to maintain old expenditures and that it is new receipts which offer the real opportunity for changing the over-all balance among different types of expenditures.

As we admitted at the beginning of this paper, this result came as something of a surprise to us; and we suspect that it may surprise others as well, since we have found no statements calling attention to the rather remarkable degree of uniformity in interindustry sales trends that has characterized the period since mid-1957.

From the standpoint of the inflation issue, the interpretation of this finding depends, of course, on whether one accepts the demand-shift hypothesis. While we do not pretend to have tested the hypothesis itself, we are inclined, on the basis of the a priori plausibility of the hypothesis and on the basis of the lack of contradictory evidence, to assume that it has some validity. Given this viewpoint, the data suggest that one reason for the relatively good record of price stability in recent years may be the low magnitude of the secular shifts in the composition of demand over the course of this period. Persistent high unemployment and the size of the "gap" between actual and potential GNP were, we are confident, of much greater importance in holding prices down—but there is no reason that these usual macroeconomic variables need to be given exclusive billing.

Whether, in future years, shifts in the composition of demand will again become more pronounced is hard to say. However, there is no obvious reason for thinking that 1957 was a kind of watershed, marking off past periods of relatively large shifts in demand from new periods of relatively small shifts. The factors determining demand shifts comprise such a varied list—changes in tastes, demographic trends, and the level and distribution of income—that the presumption would seem to be in favor of some return to degrees of sales dispersion characteristic of past periods. And there is also the likelihood that further corrections in our balance-of-payments situation will necessitate resource reallocations among industries. If the measures of dispersion do again take on higher values, then the demand-shift inflation model implies (*ceteris paribus*, of course) that inflation may become more of a problem than the record of price behavior since 1957 suggests.

#### APPENDIX

##### *A. Data Sources and Computational Procedures*

1. The sales data were taken from the *Survey of Current Business*, U.S. Department of Commerce, Office of Business & Economics. The output data (discussed in Appendix B) were taken from *Industrial Production, 1959 Revision*, Board of Governors of the Federal Reserve System, Washington, pp. S-124–S-135.

2. Separate rates of change of sales and output were calculated for the following 21 industries:

Food & kindred products  
Textile-mill products

Tobacco manufacturers  
 Apparel & other finished fabric products  
 Paper & allied products  
 Printing, publishing & allied industries  
 Chemicals & allied products  
 Products of petroleum & coal  
 Rubber products  
 Leather & leather products  
 Stone, clay & glass products  
 Primary metal industries  
 Fabricated metal products, including ordnance  
 Instruments  
 Miscellaneous manufacturing  
 Machinery, except electrical  
 Electrical machinery  
 Transportation equipment, except automobiles  
 Automobiles & automobile equipment  
 Lumber & products  
 Furniture & fixtures.

3. Each rate of change of sales,  $\dot{S}_{it}$ , was expressed at an annual rate by use of the expressions  $S_T = S_I(1 + \dot{S}_{it})^T$  where  $S_T$  = sales of the  $i$ th industry at the end of period  $t$ ,  $S_I$  = sales at the start of the period, and  $T$  = length of period  $t$  (in years).

4. The income-originating data (used to construct the weights) were compiled from *U. S. Income and Output*, U.S. Department of Commerce, Office of Business Economics, pp. 130-131, and *Survey of Current Business*, July 1962 and 1963, U. S. Department of Commerce, Office of Business Economics. These income-originating data are annual averages, and so the periods of the weights do not correspond precisely to the periods of the sales and output data. The pairings are shown below:

Periods for the Measurement of Rates of Change of Sales and Output	Periods for the Measurement of Income-Originating Weights
January 1947-July 1948	1947-1948
July 1948-March 1951	1949-1950
March 1951-July 1953	1952
July 1953-September 1955	1954
January 1947-September 1955	1947-1954
September 1955-July 1957	1956
July 1957-March 1963	1958-1962

### B. The Use of Relative Measures

There are reasons for thinking that the rate of change of total sales might have an important effect on the demand-shift inflationary process. First, for a given degree of sales dispersion, more firms are likely to have experienced

absolute declines in sales, the smaller is the increase in the over-all sales level. We associate a sales decline with a downward shift in the firm's demand curve, and the demand-shift theory postulates especially strong wage and price rigidities in the face of absolute declines in demand. Therefore, the smaller is the increase in total sales,  $\bar{S}$ , the greater effect should a given degree of dispersion have on the price level.

Secondly, if a given degree of dispersion results in a given pattern of absolute changes in wage differentials among industries (before transmission effects), and if the change in the general wage level that would have occurred apart from dispersion considerations is positively related to the rate of increase of total sales, then the relative changes in wage differentials will be greater, the smaller is the increase in total sales. And changes in relative wage differentials may be what is most important in determining transmission wage effects.

On the basis of the above reasoning, we tried using "relative" measures of dispersion:  $I_s/\bar{S}$  and  $I^*/\bar{S}$ . We achieved no systematic results and, after further consideration of the full workings of the demand-shift mechanism, concluded that we were introducing perverse feedback effects. Interperiod differences in  $\bar{S}$  are themselves, in part, a result of interperiod differences in the rate of increase in the general price level and therefore of the strength of the inflationary process generated by the initial pattern of demand shifts. The nature of this difficulty is discussed more rigorously in a mimeographed paper which we shall be glad to send to anyone interested in pursuing this matter. There it is demonstrated that using scales in both the numerator and denominator is likely to result in a measure which will have the undesirable property of indicating a smaller degree of dispersion, the more "successful" was a given initial change in the composition of demand in producing a rise in the general price level.<sup>8</sup>

To cope with this feedback problem we divided  $I_s$  and  $I^*$  by the rate of change of output,  $\bar{O}$ , instead of by  $\bar{S}$ . This gives us relative measures which are free of serious feedback effects.<sup>9</sup>

Turning now to the data, the important point is that in the Schultze period  $\bar{O}$  was remarkably low. Between September 1955-July 1957,  $\bar{O}$  was 1.41, compared with values of 4.67 for January 1947-July 1948 and 5.91 for March 1951-July 1953. When we divide by  $\bar{O}$ , both of our measures  $I_s/\bar{O}$  and  $I^*/\bar{O}$  take on by far their highest values in the September 1955-July 1957 period.<sup>10</sup>

<sup>8</sup> Some feedback effects may be present even in our  $I_s$  and  $I^*$  measures. No such effect occurs, however, if we use output data instead of sales in our measures. Separate calculations were made using output data and the results were very similar to those using  $I_s$  and  $I^*$ , so we are not very concerned about feedback here. Also, it is encouraging to see that an independent set of data produces consistent results.

<sup>9</sup> Using sales in the numerator and output in the denominator does produce a kind of "bastard" measure, and so we also made calculations using output in both the numerator and denominator. Again, there were no significant differences in results.

<sup>10</sup> Using the  $I_s/\bar{O}$  measure, the actual value for the Schultze period is 3.79, compared with 1.06 (period 1), .62 (period 2), .81 (period 3), 1.08 (period 4), and .50 (period 6).

Our second main empirical result—that shifts in the composition of demand have been very small during the post-1957 period—is unaffected by the use of the relative measure. All of the various measures used in this paper take on their lowest values during this period.

There is one major difficulty with the relative measure, which accounts for our decision to relegate the discussion of these results to this appendix. We now suspect that dividing by  $\bar{\sigma}$  overcompensates for the “relative” as opposed to the “absolute” effects of dispersion. To illustrate the problem by an extreme example, if the average increase in output were 1.0 per cent in period  $X$  and .1 per cent in period  $Y$ , and if the absolute degree of dispersion were the same in the two periods (say 2.0 per cent), the relative measure would take on a value of 2.0 for period  $X$  and 20.0 for period  $Y$ . This is what happens, to a lesser extent, in the case of the Schultze period (which is analogous to period  $Y$ ) when we use the relative measure. Taking some root of  $\bar{\sigma}$  might help meet this difficulty, but how would one determine which root? Our conclusion is that, until this question is answered, the absolute measures should be given main emphasis (and are therefore presented in the body of this paper). At the same time, we think that the relative measures, and the general results they give, are of some help in the job of interpretation.

# LONG-TERM UNEMPLOYMENT, THE STRUCTURAL HYPOTHESIS, AND PUBLIC POLICY

By N. J. SIMLER\*

To submit the rise-in-structural-unemployment hypothesis to yet another test may strike some as pretty much like subjecting an apparently dead horse to one last thumping. But the fact is that not all versions of the argument have been given the attention they deserve. In particular, it appears that the rise in the rates of long-term and very-long-term unemployment since 1957 cannot be satisfactorily accounted for or explained by the rise in the over-all unemployment rate. If, upon analysis, this turns out to be the case, it may suggest to some the presence of some kind of underlying structural change. After briefly summarizing the main lines of attack on the structural hypothesis that have appeared so far, this paper seeks to identify the principal factors behind the increase in long-term unemployment rates, to set forth a theory of long-duration unemployment and, finally, to draw certain conclusions respecting public policy.

## I. *Criticisms of the Structural Hypothesis*

Attempts to explain the higher unemployment rates that have characterized the economy since the mid-1950's have centered around two alternative hypotheses. (The Council of Economic Advisers [8] [11, pp. 40-56] [18, pp. 1751-69], along with other proponents of the inadequate-aggregate-demand hypothesis, maintains that the rise in unemployment rates has been due simply to the failure of the rate of growth of final demand for goods and services to keep pace with the rate of growth in potential supply made possible by continuing increases in productivity and in the stocks of productive resources. In other words, a "gap" has emerged between potential and actual output as the annual increment in the number of job opportunities has been insufficient to absorb the yearly increase in the number of job seekers. Accordingly, higher unemployment rates are taken to mean that the economy has departed from a position of full employment.)

On the other hand, proponents of the rise-in-structural-unemployment hypothesis, such as Thomas Curtis [1], Walter Fackler [17, pp.

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44-56], and Charles Killingsworth [18, pp. 1461-83], appear to be saying that the rise in unemployment rates has taken place despite the presence of a generally adequate level of aggregate demand and a tolerably sufficient number of job opportunities. (Unlike the demand hypothesis, the structural hypothesis has taken several forms, but in each instance it seems to have been implicitly or explicitly assumed that the increase in the number of unemployed workers has been accompanied by an approximately equal increase in number of unfilled jobs. Accordingly, the economy is believed to have remained roughly in a full-employment position, although this is associated with a higher rate of unemployment than before. It is argued that what has happened is that structural changes have occurred which have increased labor market frictions, led to greater imbalances between labor demands and labor supplies, and thus have made markets for labor function less efficiently than before. Each of the variants of the structural hypothesis has come under attack. What follows is a brief summary of these critiques.

1. The structural hypothesis has failed the job-vacancies test, at least on the basis of the National Industrial Conference Board's index of the number of help-wanted advertisements. (If structural change has accounted for the rise in unemployment rates after 1955-57, then it would have to be true that the change in the number and composition of unfilled jobs has paralleled the change in the level and composition of unemployment.) Abstracting from compositional changes, a decrease in the number of job vacancies relative to the number of unemployed persons would imply a shortage of jobs and to this extent would convert the problem into one stemming from inadequate aggregate demand. It turns out that the NICB index, even if it is not adjusted to take account of the growth in the labor force, was actually lower in 1960 and 1962 (years of 5.6 per cent unemployment) than it was in 1955-57 (years of 4.2-4.4 per cent unemployment).<sup>1</sup> If this index were a comprehensive and complete measure of the number of job vacancies, the failure of the structural hypothesis to meet this test would have established a presumptive case against it. But the NICB index does not really measure the number of job vacancies; it only measures the number of help-wanted ads in the classified-ad sections of a selected leading newspaper in each of 33 major labor market areas, which in the mid-1950's accounted for 44 per cent of total nonfarm employment [10, pp. 2-3].

<sup>1</sup> The adjusted NICB index (1957 = 100) averaged about 107 in the 1955-57 period and fell to 91 and 95 in 1960 and 1962, respectively. If the structural hypothesis were correct, one would have expected the index to have risen to the neighborhood of 146 in 1960 and 149 in 1962, in order for it to have paralleled the increase in the number of unemployed persons. The unadjusted index averaged about 106 in 1955-57 and fell to 94 and 100 in 1960 and 1962, respectively; but one would have expected it to have risen to the vicinity of 144 in 1960 and 147 in 1962. Computed from data in [10, Appendix Table 1, p. 41] [15, Table 1, p. 24] [16 Table A-1, p. 195].

Hence, the results of any test based on it can only be considered indicative, not conclusive; and so other tests have had to be devised.

2. (If the structural hypothesis that relative declines in demand for blue-collar and goods-producing workers have been an independent source of higher unemployment rates were in fact correct, then observed unemployment rates in the 1958-63 period in specific, "technologically vulnerable" [18, p. 1758] blue-collar occupations and goods-producing industries should have been systematically higher than what one would have expected on the basis of the 1947-57 experience.) But the Kalachek regressions [8, pp. 97-101] [9, pp. 49-71] [14] [18, pp. 1756-59], in which unemployment rates sector by sector are regressed against the over-all unemployment rate in the earlier period, show no systematic tendency to underpredict the specific sector rates in the later period and, in fact, tend to overpredict these rates.<sup>2</sup>

3. Alternatively, if the structural hypothesis that shifts in the geographic composition of labor demands have been an independent source of higher unemployment rates were in fact correct, then the geographic distribution of unemployment should have been more concentrated in 1960 than it was in 1950. But the studies by Denison [2] [3] and Eckstein [5, pp. 119-23] show that, in fact, the opposite has occurred and that the distribution of unemployment by metropolitan areas, states, and regions was less concentrated in 1960 than it was in 1950. Using different methods, the Kalachek-Denison-Eckstein findings have recently been confirmed by Gallaway [6].

4. Finally, if the structural hypothesis that shifts in the composition of unemployment in the direction of those groups in the labor force with characteristically high unemployment rates (teenagers, nonwhites, and unskilled or uneducated or inexperienced workers) have been an independent source of higher unemployment rates were in fact correct, then the relative unemployment rates of these disadvantaged groups, when weighted by their contribution to the total labor force, should have been higher in 1962 or 1963 than they were in the mid-1950's. But the Gordon study [7] demonstrates that, in fact, the opposite has occurred and that the concentration of unemployment among the above-mentioned groups (except for first-job seekers) has decreased. Using different methods, conclusions similar to those of Gordon had been reached in an earlier study by the Council of Economic Advisers [13, pp. 378-92].

## II. Long-Term Unemployment

Since structural unemployment may be considered an extreme form of frictional unemployment, it should also be revealed by longer spells

<sup>2</sup> Kalachek's work was done while he was a member of the staff of the Joint Economic Committee and, later, the staff of the Council of Economic Advisers.

of unemployment in particular occupations, industries, and areas [11, p. 45]. A partial test of the hypothesis that the rise in unemployment rates since 1957 has been due to structural change can be devised using data relating to long-term unemployment in the economy as a whole. If the structural hypothesis is correct, then observed long-term unemployment rates in the 1958-63 period should be systematically higher than what one would expect on the basis of the 1947-57 experience. )

TABLE 1—DATA FOR REGRESSION EQUATIONS (1)–(10)

Year	$U_L$ (per cent)	$U_{LL}$ (per cent)	$U_t$ (per cent)	$U_{t-1}$ (per cent)	$L_{tt}$ (per cent)	$t$
1947	0.66	0.27	3.92	4.22*	34.19	-5
1948	0.50	0.19	3.78	3.92	34.41	-4
1949	1.10	0.41	5.93	3.78	34.69	-3
1950	1.24	0.57	5.31	5.93	35.04	-2
1951	0.48	0.22	3.34	5.31	35.87	-1
1952	0.37	0.13	3.07	3.34	36.44	0
1953	0.33	0.12	2.93	3.07	36.84	1
1954	1.26	0.49	5.55	2.93	37.16	2
1955	1.07	0.51	4.41	5.55	37.67	3
1956	0.79	0.34	4.18	4.41	38.00	4
1957	0.82	0.35	4.32	4.18	38.13	5
1958	2.12	0.97	6.82	4.32	38.47	6
1959	1.50	0.82	5.49	6.82	38.77	7
1960	1.35	0.64	5.57	5.49	38.85	8
1961	2.14	1.12	6.71	5.57	38.89	9
1962	1.56	0.81	5.58	6.71	38.97	10
1963	1.49	0.76	5.71	5.58	38.90	11

\* Estimated using data in [11, Table B-19, p. 230 and Table B-21, p. 233].

Source: [16, Statistical Appendix, pp. 195, ff.].

The Department of Labor defines long-term unemployment as a spell of unemployment lasting 15 weeks or longer. In any given year, the ratio of the number of long-term unemployed persons to the civilian labor force measures the long-term unemployment rate ( $U_L$ ) for that year.  $U_L$  averaged 0.78 per cent in the 1947-57 period and 1.69 per cent in the 1958-63 period, an increase of 117 per cent. At the same time, the over-all unemployment rate ( $U_t$ ) increased from an average of 4.25 per cent to an average of 5.98 per cent, or by only 41 per cent. Thus, the increase in  $U_L$  was almost three times as great as the increase in  $U_t$ .

A simple linear regression of  $U_L$  on  $U_t$  for the 1947-57 period yields the following equation:<sup>3</sup>

$$(1) \quad U_L = -0.589 + 0.323U_t, \\ (0.174) \quad (0.0399)$$

<sup>3</sup> The data for this and all subsequent regressions are contained in Table 1.

where  $\bar{R}^2=0.866$  and  $\bar{S}=0.126$ . When equation (1) is used to predict  $U_L$  in the 1958-63 period, the result is a systematic underprediction of the actual  $U_L$ 's. The deviations of the actual from the predicted  $U_L$ 's are all positive and are fairly large. (See Table 2.)

This test cannot be considered decisive for it is clear that those who comprise the long-term unemployed in any given year need not have become unemployed during that year but could have become so in the immediately preceding year, or even longer ago than that. That is, it is reasonable to suppose that this year's long-term unemployment rate

TABLE 2—DEVIATIONS OF THE ACTUAL FROM THE PREDICTED VALUES OF  $U_L$  AND  $U_{LL}$ 

Equation	$U_L$				$U_{LL}$		
	(1)	(2)	(3)	(5)	(6)	(7)	(8)
1947	-.02	-.02	+.08	+.08	-.01	-.01	+.04
1948	-.13	-.12	-.04	-.03	-.07	-.06	-.01
1949	-.23	-.18	-.13	-.12	-.14	-.10	-.07
1950	+.11	+.01	+.05	+.05	+.10	+.01	+.03
1951	-.01	-.09	-.06	-.07	+.01	-.06	-.05
1952	-.03	+.02	+.02	+.02	-.04	0	0
1953	-.03	+.04	+.02	+.02	-.03	+.03	+.02
1954	+.06	+.15	+.11	+.10	-.01	+.08	+.05
1955	+.23	+.15	+.09	+.08	+.16	+.09	+.04
1956	+.03	+.02	-.06	-.07	+.02	+.01	-.04
1957	+.01	+.02	-.08	-.07	+.01	+.02	-.03
1958	+.51	+.52	+.40	+.40	+.30	+.32	+.25
1959	+.32	+.16	+.02	+.02	+.33	+.19	+.11
1960	+.14	+.07	-.09	-.07	+.14	+.07	-.01
1961	+.56	+.50	+.32	+.35	+.47	+.41	+.32
1962	+.35	+.20	0	+.05	+.31	+.17	+.09
1963	+.23	+.16	-.06	+.02	+.24	+.17	+.09

depends not only on this year's over-all unemployment rate, but also on (at least) last year's. A multiple linear regression of  $U_L$  on  $U_t$  and  $U_{t-1}$  for the 1947-57 period yields the following equation:

$$(2) \quad U_L = -0.826 + 0.315U_t + 0.0641U_{t-1},$$

(0.206) (0.0362) (0.0362)

where  $\bar{R}^2=0.891$  and  $\bar{S}=0.113$ . However, when equation (2) is used to predict the 1958-63  $U_L$ 's, the result is again a systematic underprediction of the actual  $U_L$ 's, although the residuals are somewhat smaller. (See Table 2.)

Equations (1) and (2) give support to the structural hypothesis. But examination of the residuals from both regression lines in the 1947-57 period suggests that the rise in the long-term unemployment rate relative to the over-all unemployment rate may have taken place gradually

and cumulatively over time, rather than having occurred abruptly after 1957. A multiple linear regression of  $U_L$  on  $U_t$ ,  $U_{t-1}$ , and time ( $t$ ) for the 1947-57 period yields the following equation:

$$(3) \quad U_L = -0.841 + 0.316U_t + 0.0662U_{t-1} + 0.0193t, \\ (0.171) \quad (0.0301) \quad (0.0301) \quad (0.00899)$$

where  $\bar{R}^2=0.926$ ,  $\bar{S}=0.0942$ , and  $t_{1952}=0$ , and where each net regression coefficient is statistically significant. (Now when equation (3) is used to predict the 1958-63  $U_L$ 's, there is no longer a systematic tendency to underpredict the actual  $U_L$ 's. The latter are scattered above, below, and virtually on the regression line, and four of them are within one standard error of the regression line. (See Table 2.)

This analysis reveals that time has been a statistically significant factor underlying the rise in the long-term unemployment rate. The problem is to find a variable that might be related to the long-term unemployment rate and for which time might be a proxy. One such variable is the proportion of older workers (defined as those of age 45 and over) in the civilian labor force. This proportion has been increasing. For example, in 1947 older workers constituted 34.2 per cent of the civilian labor force; in 1952, 36.4 per cent; and in 1957, 38.1 per cent.) In fact, a simple linear regression of this proportion ( $L_{45}$ ) on time for the 1947-57 period yields the following equation:

$$(4) \quad L_{45} = 36.221 + 0.438t, \\ (0.0602) \quad (0.0190)$$

where  $\bar{R}^2=0.981$ ,  $\bar{S}=0.200$ , and  $t_{1952}=0$ . At the same time, there is a rather pronounced tendency for older workers to remain unemployed for longer periods of time than younger workers. For example, in 1957 the ratio of the long-term unemployment rate for persons who were 45 years of age and over to the over-all unemployment rate in that age group was 0.27; in the 25-44 age group the corresponding ratio was 0.18; and in the 18-24 age group it was 0.14. Similarly, in 1960 the ratios were 0.32, 0.24, and 0.20, respectively; and in 1962 they were 0.38, 0.28, and 0.22, respectively.<sup>4</sup> Hence, the observed values of  $L_{45}$  may be substituted for  $t$  in equation (3).

(A multiple linear regression of  $U_L$  on  $U_t$ ,  $U_{t-1}$ , and  $L_{45}$  for the 1947-57 period yields the following equation:

$$(5) \quad U_L = -2.575 + 0.322U_t + 0.0684U_{t-1} + 0.0470L_{45}, \\ (0.743) \quad (0.0288) \quad (0.0287) \quad (0.0195)$$

<sup>4</sup> The ratios in this paragraph were computed from data in [16, Tables A-1, A-5, A-8, A-13, pp. 195-203]. The tendency for older workers to remain unemployed for longer periods than younger workers is also borne out by data from other countries. See [12, pp. 60-64].

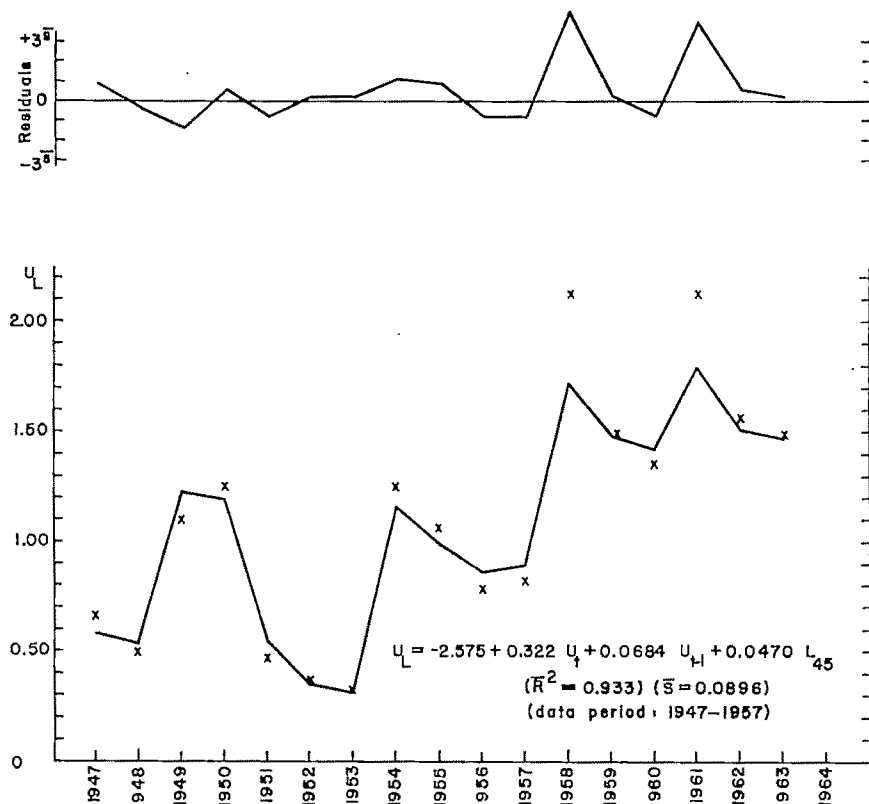


FIGURE 1

where  $\bar{R}^2 = 0.933$  and  $\bar{S} = 0.0896$ , and where each net regression coefficient is statistically significant. When equation (5) is used to predict the 1958-63  $U_L$ 's, there is again no systematic tendency to underpredict the actual  $U_L$ 's. The latter are found above, below, and virtually on the regression line. While four of the actual  $U_L$ 's lie well within one standard error of the regression line, those for the two recession years are outside three standard errors.<sup>5</sup> Figure 1 shows the actual and predicted  $U_L$ 's, together with the residuals, for the period since 1947.

<sup>5</sup> The fact that the regression equation underpredicts by a wide margin the actual  $U_L$ 's in 1958 and 1961 is probably due in some part to the fact that annual, rather than quarterly or monthly, data are used. When a recession trough occurs early in the year, as was the case in both 1958 and 1961, the (annual) average levels of both over-all unemployment and long-term unemployment will tend to be higher than they would have been had the trough occurred late in the year. The reason is that it takes less time to increase unemployment than it does to reduce it. While the ratio of the predicted  $U_L$  to the actual  $U_L$  will be unaffected by the timing of the trough, the ratio of the actual  $U_L$  to the actual  $U_t$  will tend to be the greater, the earlier the trough occurs in the year, and the smaller, the later it occurs. This means that, other things equal, years with early troughs will tend to have positive deviations of the actual from the predicted  $U_L$ , those with late troughs, negative deviations, and those with in-

III. *Very-Long-Term Unemployment*

The analysis thus far has isolated the factors underlying the secular increase in the percentage of the labor force unemployed for 15 weeks or longer. However, the data on long-term unemployment make it possible to compute not only the long-term unemployment rate ( $U_L$ ) but also the very-long-term unemployment rate ( $U_{LL}$ ), where very-long-term unemployment is defined as a spell of unemployment lasting 27 weeks or longer. In this section we shall examine the behavior of  $U_{LL}$  in the 1958-63 period to see whether it conforms to what one would have expected on the basis of the 1947-57 experience.

$U_{LL}$  has increased much faster between the two periods than  $U_L$ .  $U_{LL}$  averaged 0.33 per cent in the 1947-57 period and 0.85 per cent in the 1958-63 period, an increase of 158 per cent; whereas the corresponding increase in  $U_L$  was 117 per cent. It will be recalled that over the same span  $U_t$  increased by only 41 per cent, from an average of 4.25 per cent to an average of 5.98 per cent. Thus, the increase in  $U_{LL}$  was almost four times as great as the increase in  $U_t$ .

A simple linear regression of  $U_{LL}$  on  $U_t$  for the 1947-57 period yields the following equation

$$(6) \quad U_{LL} = -0.238 + 0.133U_t, \\ (0.118) \quad (0.0270)$$

where  $\bar{R}^2 = 0.699$  and  $\bar{S} = 0.0854$ . When equation (6) is used to predict  $U_{LL}$  in the 1958-63 period, the result is a systematic underprediction of the actual  $U_{LL}$ 's. The deviations of the actual from the predicted  $U_{LL}$ 's are all positive and are almost as large as those for  $U_L$  in equation (1). Since  $U_{LL}$  is by definition smaller than  $U_L$ , this means that the tendency toward underprediction in equation (6) is relatively more pronounced than that in equation (1). (See Table 2.)

Despite the stronger indications of the presence of structural change, the grounds for considering this test as decisive are weaker than before. If this year's long-term unemployment rate depends not only on this year's over-all unemployment rate but also on last year's (as has been shown), then surely this year's very-long-term unemployment rate depends even more importantly on last year's over-all unemployment rate. A multiple linear regression of  $U_{LL}$  on  $U_t$  and  $U_{t-1}$  for the 1947-57 period yields the following equation:

$$(7) \quad U_{LL} = -0.455 + 0.125U_t + 0.0589U_{t-1}, \\ (0.114) \quad (0.0201) \quad (0.0201)$$

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between troughs will tend to have residuals which are closer to zero than those of the others. Roughly speaking, this is actually the case. The residuals for the four recession years are: 1949 (October trough), -0.12; 1954 (August trough), +0.10; 1958 (April trough), +0.40; and 1961 (February trough), +0.35.

where  $\bar{R}^2=0.836$  and  $\bar{S}=0.0628$ . But when equation (7) is used to predict the 1958-63  $U_{LL}$ 's, the result is still a systematic underprediction of the actual  $U_{LL}$ 's. Again, the residuals are of about the same order of magnitude as those for  $U_L$  in equation (2), and so here, too, the tendency toward underprediction is relatively more pronounced than it was in equation (2). (See Table 2.)

Thus, equations (6) and (7) give stronger support to the structural hypothesis than do equations (1) and (2). To test the proposition that the rise in the very-long-term rate of unemployment relative to the over-all rate may have been a gradual and cumulative development related to the increasing proportion of older workers in the civilian labor force, we may employ a multiple linear regression of  $U_{LL}$  on  $U_t$ ,  $U_{t-1}$ , and  $L_{46}$ . For the period 1947-57 this yields the following equation:

$$(8) \quad U_{LL} = -1.454 + 0.129U_t + 0.0614U_{t-1} + 0.0268L_{46},$$

$$(0.402) \quad (0.0155) \quad (0.0155) \quad (0.0105)$$

where  $\bar{R}^2=0.903$  and  $\bar{S}=0.0484$ , and where each net regression coefficient is statistically significant. However, when equation (8) is used to predict the 1958-63  $U_{LL}$ 's, the results are somewhat different from those obtained when equation (5) was used to predict the 1958-63  $U_L$ 's. Whereas the actual  $U_L$ 's had a tendency to fall above, below, and virtually on the regression line, the actual  $U_{LL}$ 's tend (with one exception) to lie above the regression line. Moreover, the  $U_{LL}$  residuals tend to be larger than the  $U_L$  residuals. Only one of the actual  $U_{LL}$ 's lies within one standard error of the regression line and three others are either barely within or just outside two standard errors, while those for the two recession years lie well outside three standard errors.<sup>6</sup> Figure 2 shows the actual and predicted  $U_{LL}$ 's, together with the residuals, for the period since 1947.

#### IV. *A Theory of Long-Duration Unemployment*

The preceding analysis has identified the secular increase in the proportion of older workers in the civilian labor force as a "structural" change underlying the secular increase in long-term unemployment rates. But like the lagged unemployment rate, it is statistically significant but of relatively little quantitative importance. In addition, the influence of  $L_{46}$  can be seen to have waned since 1957. Equation (4) implies that between 1947 and 1957 the trend rate of increase of  $L_{46}$  was 1.1 per cent per year; but between 1958 and 1963 the data show that it grew at a trend rate of only 0.2 per cent per year. And on the basis of

<sup>6</sup> See the preceding footnote for an explanation of the large positive residuals in 1958 and 1961. Here, the residuals for the four recession years conform somewhat more closely to the expected pattern than the later trough, the smaller the positive deviation of the actual from the predicted  $U_{LL}$ : 1949, -0.07; 1954, +0.05; 1958, +0.25; and 1961, +0.32.

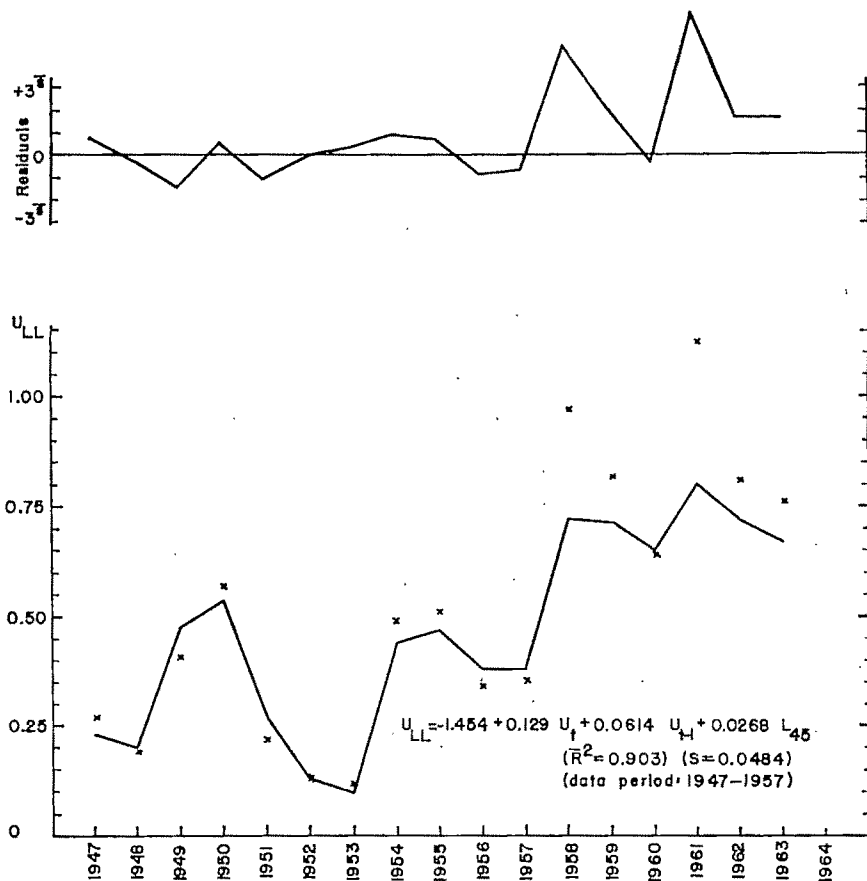


FIGURE 2

the interim revised projections of population, total labor force, and labor force participation rates, it seems reasonable to expect this leveling-off trend to continue through the decade of the 1960's, after which time  $L_{45}$  should begin to decrease and by 1975 approach its 1955-57 level.<sup>7</sup>

If we assume that, during the course of the next few years or so,  $\bar{U}(=U_t=U_{t-1})$  and  $\bar{L}(=L_{45})$  stabilize in the neighborhood of 4 per cent

<sup>7</sup> The proportion of older workers in the civilian labor force ( $L_{45}$ ) varies directly with (1) the ratio of the population 45 years of age and over to the total population 14 years of age and over ( $P_{45}$ ) and (2) the labor force participation rate of persons 45 years of age and over ( $R_{45}$ ), and inversely with (3) the labor force participation rate of persons 14 years of age and over ( $R_{14}$ ) and (4) the ratio of the civilian labor force to the total labor force ( $F$ ). That is,  $L_{45} = (P_{45})(R_{45})(1/R_{14})(1/F)$ . On the assumption that the armed forces continue to comprise the same percentage of the civilian labor force as they have in the past six years (3.725 per cent, on the average), so that  $F = 0.9641$ , the interim revised population and labor force

and 40 per cent, respectively, then equation (5) implies a  $U_L$  of 0.87 per cent, or about 695,000 persons out of an expected civilian labor force of about 80 million. Under the same conditions, equation (8) implies a  $U_{LL}$  of 0.38 per cent, or about 305,000 persons. Equation (5) also implies that a reduction in  $U_L$  of one percentage point requires a decrease of 2.6 percentage points in  $\bar{U}$  or 21.3 percentage points in  $\bar{L}$ , or some combination of the two. Similarly, equation (8) implies that a reduction of one percentage point in  $U_{LL}$  requires a decrease of 5.3 percentage points in  $\bar{U}$  or 37.3 percentage points in  $\bar{L}$ , or some combination of the two. For  $\bar{L}=40$  per cent, this means that  $U_L$  will approach zero as  $\bar{U}$  approaches 1.8 per cent and that  $U_{LL}$  will approach zero as  $\bar{U}$  approaches 2.0 per cent. If  $\bar{U}$  were stabilized in the neighborhood of 2 per cent, then unemployment would presumably be the result of normal labor turnover. Its duration would be relatively short, since it would only involve locating a new job at a time when job opportunities were relatively abundant. In other words, all unemployment would be frictional unemployment.<sup>8</sup>

The preceding analysis has also pointed up a shift in the relationship between  $U_L$  and  $U_{LL}$ , particularly since 1957. To be more specific, regressions of  $U_{LL}$  on  $U_L$  for the 1947-57 and 1958-63 periods yield the following results:

$$(9) \quad U_{LL} = -0.0183 + 0.441U_L, \\ (0.0281) \quad (0.0331)$$

projections contained in [16, Table E-2, p. 242] yield the following values of  $L_{45}$  for 1970 and 1975:

$$L_{45}(1970) = (0.4141)(0.5234) \left( \frac{1}{0.5696} \right) \left( \frac{1}{0.9641} \right) = 0.3946 = 39.46 \text{ per cent};$$

$$L_{45}(1975) = (0.4051)(0.5173) \left( \frac{1}{0.5698} \right) \left( \frac{1}{0.9641} \right) = 0.3815 = 38.15 \text{ per cent.}$$

<sup>8</sup> A better way of estimating the current frictional minimum rate of unemployment is to use data for the entire postwar period rather than just the period between 1947 and 1957. Multiple linear regressions of  $U_L$  and  $U_{LL}$  on  $U_t$ ,  $U_{t-1}$ , and  $L_{45}$  for the 1947-63 period yield the following equations:

$$(5a) \quad U_L = -3.159 + 0.385U_t + 0.0441U_{t-1} + 0.0588L_{45}, \\ (0.730) \quad (0.0305) \quad (0.0304) \quad (0.0218)$$

where  $\bar{R}^2=0.948$  and  $\bar{S}=0.127$ ;

$$(8a) \quad U_{LL} = -2.157 + 0.175U_t + 0.0596U_{t-1} + 0.0414L_{45}, \\ (0.473) \quad (0.0197) \quad (0.0197) \quad (0.0141)$$

where  $\bar{R}^2=0.926$  and  $\bar{S}=0.0821$ . For  $\bar{L}=39$  per cent, these equations imply that very-long-term unemployment would disappear ( $U_{LL}=0$ ) if  $\bar{U}$  were stabilized at 2.3 per cent, and that all long-term unemployment would vanish ( $U_L=0$ ) if  $\bar{U}$  were stabilized at 2.0 per cent. Hence, at  $\bar{U}=2.0$  per cent all unemployment would be temporary and short-term, or frictional, unemployment.

where  $\bar{R}^2 = 0.947$  and  $\bar{S} = 0.0360$ , for the period 1947-57;

$$(10) \quad U_{LL} = 0.0767 + 0.459U_L, \\ (0.144) \quad (0.0834)$$

where  $\bar{R}^2 = 0.854$  and  $\bar{S} = 0.0644$ , for the period 1958-63. Thus, contrasting the later period with the earlier, not only is there a greater change in  $U_{LL}$  per unit change in  $U_L$  (0.46 vs. 0.44), but for any given value of  $U_L$ ,  $U_{LL}$  is greater than before (e.g., for  $U_L = 1$ , 0.54 vs. 0.42). In other words, not only have long-term unemployment rates increased relative to the over-all unemployment rate, but the duration of long-term unemployment has become longer.

These results would seem to permit one to draw the conclusion that the relative increase in long-duration unemployment has been due to an increase in structural unemployment and, because there has been a disproportionate increase in long-term unemployment rates, it must be so that structural changes have been, in whole or in part, the cause of the higher unemployment rates of the past six years.<sup>9</sup> But do they? What if it were possible to explain both the rise in  $U_L$  and  $U_{LL}$  relative to  $U$ , and the rise in  $U_{LL}$  relative to  $U_L$  without introducing the concept of structural change?

Imagine an economy with no technological change, no change in the composition of demand for either products or factors, and no change in the age-sex-color-geographic-educational-and-skill composition of its employed labor force. Assume that it is operating at the frictional minimum rate of unemployment,  $U_0$ . All unemployment is due to normal labor turnover and is short term. The flow into and out of this category is such that the probability of an unemployed worker finding a job within some specified period of time—say, 14 weeks—is one. Now suppose that the unemployment rate starts to fluctuate between  $U_0$  and  $U_1$ , rising to  $U_1$  during downswings in activity and falling to  $U_0$  during upswings. If the structure of wages (meaning, all hourly employment costs) conforms to the structure of skills, then decisions to lay off and rehire workers will be made on a random basis, provided there are no institutional rules that preclude such behavior. All workers displaced during

<sup>9</sup> This point of view has perhaps been most clearly stated by D. E. Diamond in [4, p. 35]:

In part, the higher unemployment was caused by the slowdown in the nation's economic growth. But structural changes are also responsible for some of the increase. Between 1957 and 1962 long-term unemployment (15 weeks and over) rose by 100 per cent and very long-term unemployment (six months and over) rose by almost 150 per cent. In the same period overall unemployment increased only 40 per cent.

Structural or technological unemployment differs materially from other major types of unemployment. Workers who are laid off due to seasonal or cyclical factors are usually back at work as the normal pace of business activity resumes. In contrast, the technologically displaced worker must seek a new position. He is likely to be unemployed for an extended period of time and may often be forced to take a job at substantially lower pay.

the downswings will get re-employed during the upswings, but the probability of re-employment within 14 weeks for any individual unemployed worker will be less than one. Although both the mean over-all and long-term unemployment rates rise, there is no chance of a "hard core" of unemployed workers developing because each unemployed worker is certain to have become re-employed by the time  $U_0$  is reached.

Imagine now that the unemployment rate rises from  $U_0$  past  $U_1$  to  $U_2$  and then falls only to  $U_1$ . Some of the workers displaced during the downswing in activity will remain unemployed during the succeeding upswing. The mean over-all and long-term unemployment rates increase; and if in subsequent cycles the unemployment rate simply fluctuates between  $U_1$  and  $U_2$ , the probability of an unemployed worker getting re-employed remains less than one. If the probability of re-employment within any specified period of time falls as  $U_t$  rises, then the ratio of both  $U_L$  and  $U_{LL}$  to  $U_t$  will be greater than it was when the unemployment rate merely fluctuated between  $U_0$  and  $U_1$ , and so also will be the ratio of  $U_{LL}$  to  $U_L$ .

A hard core of long-duration unemployed workers will emerge if the probability of re-employment is a decreasing function of the duration of unemployment. Among other reasons, this will be the case if the skill level of unemployed workers is also a decreasing function of the duration of unemployment, and if the structure of wages fails to adapt to the changing structure of skills; or if unemployed workers' skills remain intact but do not advance with the increasing level of skills of the employed labor force, and if the wage structure fails to adjust to the changing skill structure. Either way, the gap between the potential productivity of the unemployed and the actual productivity of the employed widens with time; and, therefore, the probability of re-employment diminishes with time. Under these conditions, the chance of a hard core of unemployed workers developing becomes the greater, the longer the unemployment rate fluctuates between  $U_1$  and  $U_2$ , for with each succeeding cycle the flow into the category of very-long-term unemployment exceeds the flow out of it. Hence, the ratio of  $U_{LL}$  to  $U_L$  will rise with time.

Under the assumed conditions, the hard core will be comprised of those whose skills have deteriorated the most, relative to those of employed or newly unemployed workers. The labor force components that would appear to be most vulnerable to skill decay through disuse are older workers and those with relatively low levels of educational attainment. These will tend to be overlapping sets if years of schooling completed and age are inversely correlated. When aggregate demand contracts and unemployment rises, these workers have just as much chance of becoming unemployed as any other labor force component,

provided that layoffs among workers of equal skill are made in a random fashion and not on a last-in, first-out basis. But when aggregate demand expands and unemployment falls, they have less chance of becoming re-employed than other workers, and their prospects diminish the longer they remain unemployed. An employer with one job vacancy and two unemployed workers from which to choose will not make a random selection, unless the choice is between workers of equal skill. Rather, he will tend to choose that worker whose skills have deteriorated the least through lack of use, unless he is constrained to do otherwise. Other things equal, this will tend to be the worker who has been unemployed the shorter time, and this in turn will tend to be the younger and more highly educated of the two.

Thus it is that persistent and long-term, or structural, unemployment—as distinct from temporary and short-term, or frictional, unemployment—can emerge in the first place (i.e.,  $U_L > 0$  and  $U_{LL} > 0$ ), can increase relative to all other unemployment (i.e.,  $\Delta U_L > \Delta U$ , and  $\Delta U_{LL} > \Delta U_L$ ), and can become increasingly concentrated among older workers (i.e.,  $\Delta U_L / \Delta U$  for older workers  $> \Delta U_L / \Delta U$  for all workers, and  $\Delta U_{LL} / \Delta U_L$  for older workers  $> \Delta U_{LL} / \Delta U_L$  for all workers) without there having occurred a structural change of any kind whatsoever. Therefore, from the mere fact that persistent and long-term, or structural, unemployment has increased, one cannot logically infer the presence of underlying structural change. In other words, a rise in structural employment can simply be the result of an increase in over-all unemployment and not in any way the cause of it. It is, therefore, no contradiction to say that inadequate aggregate demand can be the cause of a rise in over-all unemployment and that this in turn can generate a rise in structural unemployment.

### V. *Some Policy Implications*

If the preceding analysis of the problem is correct, it is clear that fiscal-monetary policy, unaccompanied by labor market policies which effectively restore the skills of the unemployed, cannot succeed in reducing unemployment to the neighborhood of  $U_0$  without simultaneously driving up the level of prices. But it is also clear that labor market policies, unless accompanied by a net increase in total expenditures, cannot reduce unemployment at all.<sup>10</sup> So, even if the problem of high unemployment is a pure case of a deficiency in aggregate demand (apart from those consistent with  $U_0$ , no job vacancies exist at all), both fiscal-monetary policy and labor market policy are required to reduce unemployment to  $U_0$  without inflation. Only if the problem is

<sup>10</sup> Labor market policies, however, tend to equalize the probability of re-employment among unemployed workers and thereby tend to reduce  $U_{LL}$  relative to  $U_L$ .

purely structural in nature (the number of job vacancies equals the number of unemployed persons), can labor market policy alone, and unaccompanied by a net increase in total expenditures, be relied upon to regain  $U_0$  without inflation. Except for this one limiting case, therefore, fiscal-monetary policy and labor market policy appear as essentially complementary policies and not as alternative means of obtaining simultaneously the goals of full employment ( $U = U_0$ ) and price stability.

It may occur to some that, since  $\bar{L}$  depends on the labor force participation rate of older workers, both  $U_L$  and  $U_{LL}$  can be reduced by any policy which reduces this participation rate—for instance, some kind of compulsory retirement program. This is indeed the case; but a decrease in the participation rate of older workers would also induce a decrease in over-all unemployment—directly, because some of those who retired would be unemployed workers; indirectly, because any withdrawal of employed workers from the labor force would open up job opportunities for those now unemployed. But it is clear from equations (5) and (8) that the resulting decreases in  $U_L$  and  $U_{LL}$  would be due principally to the reduction in  $\bar{U}$  and only incidentally to the reduction in  $\bar{L}$ . There is, however, a more fundamental consideration.

The existence of unemployment implies the presence of a gap between actual and potential output. This gap can be closed, and unemployment reduced, either by raising actual output to its potential or by lowering potential output to the actual. If the latter alternative were the only one possible, then a persuasive case could be made for policies aimed at reducing the fraction of older workers in the labor force. For example, if in 1960 all workers 45 years of age or older who were or became unemployed had been persuaded to retire from the labor force, then using the data contained in [16, pp. 195–203],  $L_{45}$  would have been 37.90 per cent (instead of 38.95 per cent); and  $U_t$  would have been 4.03 per cent, or 2.80 million persons (instead of 5.57 per cent, or 3.93 million persons). Hence,  $U_L$  would have been 0.86 per cent, or almost 600,000 persons (instead of 1.35 per cent, or 956,000 persons); and  $U_{LL}$  would have been 0.38 per cent, or about 260,000 persons (instead of 0.64 per cent, or 454,000 persons).<sup>11</sup> If the only alternatives facing these 1.13 million unemployed older workers were continued unemployment or compulsory retirement, it would have

<sup>11</sup> To take another example: If in 1960 all workers 70 years of age or older (about 1.45 million in number) had been persuaded to withdraw from the labor force (without regard to whether they were currently employed or unemployed), then  $L_{45}$  would have been 37.68 per cent;  $U_t$  would have been 3.58 per cent, or 2.48 million persons; and, according to equations (5) and (8),  $U_L$  would have been 0.72 per cent, or almost 500,000 persons, and  $U_{LL}$  would have been 0.36 per cent, or about 250,000 persons.

been difficult indeed for society to have resisted choosing the latter for it would have only involved the spurious sacrifice of giving up some nonattainable potential output.

But if it is also possible to reduce unemployment by increasing employment and thus to raise actual output to its potential—for instance, by an appropriate combination of fiscal-monetary and labor market policies—then choosing to reduce unemployment by imposing earlier retirement (or, for that matter, shorter work-weeks or work-years) on a portion of the labor force involves the real sacrifice of relinquishing some attainable potential output. Here, the case for earlier retirement and similar programs is less persuasive and clear cut, for the choice between them and policies directed at increasing employment depends on society's preferences between work and leisure, about which we (or, at least, I) know precious little. But in the absence of such knowledge, the prudent thing to do would seem to be to provide an economic environment which would permit people to make free and voluntary choices between work and leisure. In an economy whose rate of involuntary leisure has averaged 6 per cent for the past six years, this would mean the adoption of policies aimed not at sharing involuntary leisure among a larger group of people or institutionalizing it among a given group of people, but at eliminating it by simultaneously expanding the opportunities for gainful employment and adjusting the skill and geographic composition of the work force to the evolving pattern of labor demands.

Successful fiscal-monetary and labor market policies would reduce unemployment and permit workers voluntarily to substitute leisure for work by choosing shorter work-weeks, work-years, or work-lives. Actual output would be raised to its potential, but the level and rate of growth of potential output itself (productivity considerations aside) would depend upon the free choice of people between work and leisure. In contrast, successful participation rate (and related) policies would reduce unemployment and compel workers involuntarily to substitute leisure for work by requiring earlier retirement from, or later entry into, the labor force (or by requiring shorter work-weeks or work-years).<sup>12</sup> When viewed in this light, the proper course of public policy is clear.

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<sup>12</sup> However, a participation rate policy aimed at delaying the entry of younger workers into the labor force by extending the number of years of required attendance in school would both reduce unemployment now and, if the additional years of education effectively raised labor productivity, increase actual and potential output later on.

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## THE DEMAND FOR GENERAL ASSISTANCE PAYMENTS

By C. T. BREHM AND T. R. SAVING\*

. . . the growth of enlightened views as to the duties of the State and of private persons toward the poor, is tending to make it every day more true that those who have helped themselves and endeavoured to provide for their own future will be cared for by society better than the idle and the thoughtless. But the progress in this direction is still slow, and there remains much to be done yet. (Alfred Marshall, *Principles of Economics*, Bk. IV, Ch. 7, p. 226.)

The scope of relief which society gives to the poor has been a topic much discussed by economists. A major issue of these discussions is the extent to which relief payments, depending as they do on the idleness or near idleness of the recipients, will diminish incentives to seek or hold a job.

Pigou [7, p. 728] makes the following comment concerning relief payments:

. . . those [relief payments] which differentiate in favour of idleness and thriftlessness by making the help that is given larger, the smaller is the provision the recipients have made for themselves. Some resort to this type of transference is involved in all Poor Law systems that fix a state of minimum fortune below which they will not allow any citizen to fall. For, in so far as they raise to this level the real income of all citizens whose provision for themselves falls below it, they implicitly promise that any reduction in private provision shall be made good by an equivalent addition to State provision. It is plain that the expectation of these differential transferences will greatly weaken the motive of many poor persons to make provision for themselves.

Pigou continues with the following statement concerning the level of relief payments to the poor [7, pp. 731-32]:

But differentiation in favour of small provision made through temporary work is a serious matter. If, for example, it is understood that everybody's income will, at need, be brought up by State aid to, say, £3 a week, it will, generally and roughly, be to the interest of everybody capable of earning by work any sum less than £3 a week to be idle and earn nothing. This *must* damage the national dividend. How much it damages it will,

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of course, depend on how large the sum fixed on as a minimum is, and how many people in the country would normally earn by work less than that sum. If the sum exceeds the normal earning power of a large part of the community, the damage done must necessarily be very great.<sup>1</sup>

In spite of this serious question, the level of welfare benefits relative to income levels tends to be dismissed as a factor influencing work incentives. Thus, in a report published by the U.S. Senate [10], the Advisory Council on Public Assistance urges a minimum standard of living without discussing the impact on work incentives. On the other hand, V. Carlson [3] and E. M. Burns [2] tend to emphasize the social status and psychological lift achieved through gainful employment, and Carlson [3, p. 200] seems to indicate that the relative level of public assistance to wages may have a short-run effect on work incentives. Burns [2, pp. 57-59], while asserting that the large majority of workers will always prefer work to relief payments, recognizes that there are "marginal groups of whom all this is not true."

These and other works suggest that there is a need for economic and statistical analysis concerning the effect of the magnitude of the public assistance payment on work incentives.

In Part I of this paper we present a theoretical exposition of those factors which might be expected to influence decisions to demand public assistance. Part II consists of a discussion of the data used in the estimation of the demand for public assistance. In Part III we present the estimation procedure, and Part IV contains a summary of our findings and policy implications.

### *I. A Theoretical Discussion of the Demand for Government Assistance Payments*

The demand for government assistance payments (hereafter referred to as G.A.P.) may be looked upon as a special case of the demand for leisure with the following qualifications:

1. If the consumer chooses to take G.A.P., he must specialize in leisure or, if he does work, he must earn below some minimum which society deems necessary for life.<sup>2</sup>
2. Society may place some stigma on the receipt of G.A.P. so that income from this source will be discounted by the recipient.

Consider an indifference map with real income ( $Y$ ) on the vertical axis and leisure ( $L$ ) on the horizontal axis (see Figure 1).

<sup>1</sup>In addition to this argument concerning the cost to society of the shift in the labor supply function due to the adverse effect of the existence of poor relief on work incentive, we have the effect on the incentives of the workers who must now be taxed to pay for the relief.

<sup>2</sup>If the G.A.P. recipient does work, his earnings are subtracted from his G.A.P.

Assume that the consumer's alternative to leisure yields wage rate ( $w_0$ ), the level of which is independent of the number of hours per day the consumer chooses to work, so that his income constraint consists of the line  $L^m Y^m$  in Figure 1. Given the indifference map represented by the solid indifference loci in Figure 1, the consumer will be in equilibrium at point  $A$  consuming ( $OL^0$ ) hours of leisure and receiving ( $OY^0$ ) income.

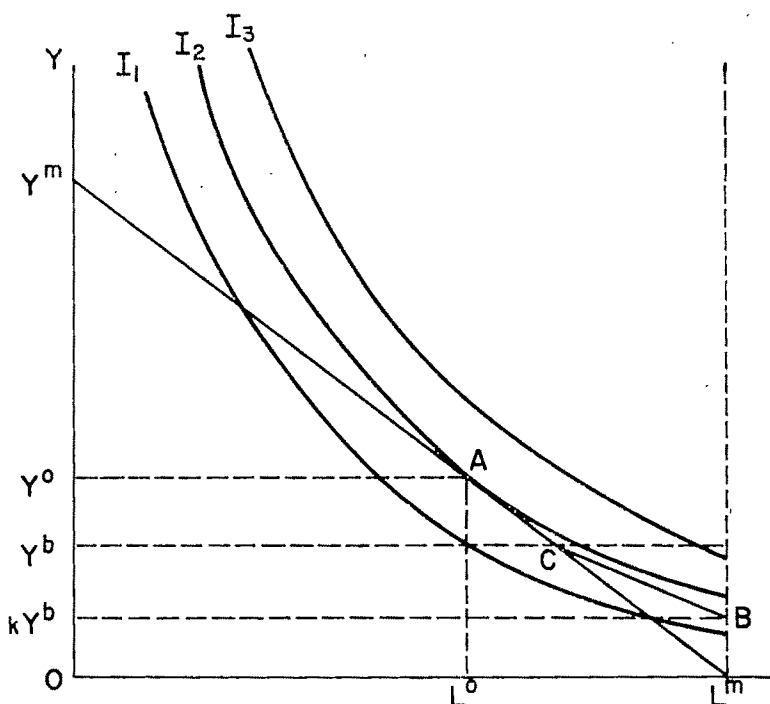


FIGURE 1

Now let us establish the level of income ( $Y^b$ ) which the state feels is the minimum necessary, "or proper," to sustain a consumer, i.e., for any consumer earning below this minimum the state will make up the difference between earned income ( $Y^e$ ) and the minimum ( $Y^b$ ), even if ( $Y^e$ ) equals zero. In addition, let us assume that some stigma is attached to the collection of G.A.P. so that the consumer applies to it a discount rate ( $k$ ), where  $0 \leq k \leq \infty$ . The discount rate ( $k$ ) would ordinarily be less than or equal to one but could exceed one for people who prefer living off the fat of the land. Given the existence of G.A.P. and the discount  $k$ , a new income constraint must be constructed. We begin at the point where the consumer specializes in leisure and re-

ceives the stigma-discounted G.A.P. income ( $kY^b$ ).<sup>3</sup> As the consumer begins to earn income, he views his income as his earned income plus ( $k$ ) times his portion of G.A.P., namely,  $Y^e + k(Y^b - Y^e)$ . Therefore the income constraint relevant for the decision of whether or not to demand a G.A.P. is given by the broken line  $L^mBCY^m$ .<sup>4</sup>

The institution of the G.A.P. program has resulted in the addition of the triangle  $L^mBC$  to the consumer's attainable set of combinations of income and leisure. Certainly such an increase in the attainable area cannot make the consumer worse off. Since the highest indifference locus the new income constraint is tangent to continues to be the original indifference curve  $I_2I_2$ , the consumer's equilibrium has not changed. However, a sufficiently large increase in the minimum income which society deems necessary or desirable (causing a parallel shift in line segment  $BC$ )<sup>5</sup> will result in the consumer's reducing his hours of work and demanding a G.A.P. income supplement.<sup>6</sup> Similarly, a decrease in the wage rate or an increase in the stigma-discount will increase the likelihood that the consumer will demand assistance payments.

If we now aggregate across consumers, we can get a demand schedule for assistance payments which, even though the individual demand

<sup>3</sup> If we assume nonsatiety of leisure, ( $kY^b$ ) will be the reservation income for the consumer. That is, the consumer's income, including any discounted G.A.P., must exceed ( $kY^b$ ) or he will specialize in leisure.

<sup>4</sup> The slope of line segment  $BC$  depends only on ( $w$ ) and ( $k$ ) and, hence,  $BC$  is linear. It can be proven easily since the consumer views his income constraint as:

$$(a) \quad Y = w(24 - L) + k[Y^b - w(24 - L)] \quad \text{if} \quad [Y^b - w(24 - L)] > 0$$

and

$$(b) \quad Y = w(24 - L) \quad \text{if} \quad [Y^b - w(24 - L)] < 0.$$

(a) above is the equation of the line segment  $BC$  and

$$\frac{\partial Y}{\partial L} = (k - 1)w. \quad \text{Q.E.D.}$$

<sup>5</sup> To show that line segment  $BC$  shifts parallelly due to shifts in ( $Y^b$ ), we need to show that the slope of the income constraint is invariant to changes in ( $Y^b$ ). Since

$$\frac{\partial Y}{\partial L} = (k - 1)w,$$

then

$$\frac{\partial \left( \frac{\partial Y}{\partial L} \right)}{\partial Y^b} = \frac{\partial^2 Y}{\partial L \partial Y^b} = 0. \quad \text{Q.E.D.}$$

<sup>6</sup> However, when Pigou [7, p. 731] states "it will, generally and roughly, be to the interest of everybody capable of earning by work any sum less than £3 a week [where £3 is our ( $Y^b$ )] to be idle and earn nothing," he must be assuming  $k = 1$ . For as we have seen, if  $k < 1$ , a consumer may choose (and a substantial number actually do) to work and collect a G.A.P. income supplement even though the consumer's total earnings are subtracted from his G.A.P.

functions are step functions, may be looked upon as the continuously differentiable function:

$$(1.1) \quad N = f(w, Y^b, k);^7$$

where  $N$  = number of assistance recipients,  $w$  = wage rate,  $Y^b$  = minimum income acceptable to the society, and  $k$  = average discount factor. The relevant partial derivatives have the following signs:

$$(1.2) \quad \frac{\partial f}{\partial w} < 0; \quad \frac{\partial f}{\partial Y^b} > 0; \quad \frac{\partial f}{\partial k} > 0.$$

The above discussion is based on the wage rate and the minimum level of income considered desirable by society, while, as seen in the introduction, those concerned about relief payments express themselves in terms of the absolute or relative incomes of relief recipients. However, it is a simple task to translate our argument, based on wage rates, into an argument based on earned income levels. For example, consider the indifference map depicted in Figure 1 and the initial equilibrium at point  $A$ . It is apparent that, given the equilibrium earned income ( $Y^o$ ), the higher the minimum income ( $Y^b$ ), the greater the likelihood that the consumer can make himself better off by demanding a G.A.P. Therefore, it appears that we may rewrite (1.1) above as follows:

$$(1.3) \quad N = h\left(\frac{Y^b}{Y}, k\right)$$

where:

$$(1.4) \quad \frac{\partial h}{\partial\left(\frac{Y^b}{Y}\right)} > 0; \quad \frac{\partial h}{\partial k} > 0.$$

In the following sections of this paper we bring the empirical evidence to bear on the question of whether the variables used in our theoretical analysis have their expected effect on the demand for G.A.P. In addition, we assess the empirical significance of these variables.

## II. *The Data*

Government assistance payments in the United States fall into two general types of programs. In the first group are programs operated jointly by the states and the federal government under the Social Security Act and its amendments. These programs provide for financial

<sup>7</sup>The individual either demands an assistance payment or does not. Hence, consider the step function:  $n_i = f_i(w, Y^b, k)$ , where  $n_i = 1$  if the  $i$ th consumer demands an assistance payment;  $n_i = 0$  if the  $i$ th consumer does not demand an assistance payment. If we aggregate across consumers with such demand functions, our dependent variable is  $n_i = N$ , which represents the number of assistance recipients.

aid to persons meeting specific qualifications which the recipient cannot meet through his own volition, either at all (age) or without taking quite drastic and often unlikely measures (blindness, total disability, dependent children) [12]. Since in these programs the deliberate choice of consumers between earned income and relief payments either is not present or is sufficiently inhibited that the effect would be small, we shall not consider them here. Similarly, the social insurance programs provide deferred earnings upon retirement or unemployment, and with respect to the latter, the period of coverage is limited and, hence, it cannot be treated as a permanent income alternative to work.<sup>8</sup>

A smaller group of programs called the "General Assistance Program" is available to those who do not meet the "objective" qualifications described above, but who still merit aid either because they are unemployable, i.e., have a zero wage alternative, or the level of income they are able to earn falls below the level deemed necessary or proper by society<sup>9</sup> [10, Appendix D]. These qualifications may be met by a deliberate action of the consumers and thus fall within the scope of our analysis. Consequently, we use data relating to this group of programs to test our analysis and to quantify the strength of the variables,

We therefore consider the number of general assistance recipients as the relevant dependent variable, i.e., as the ( $N$ ) in Part I of this paper. Data have been collected on numbers of G.A.P. recipients on an average per-month basis, annually by state for the period 1951-59 [13].

Unfortunately no direct measure of the average minimum level of income deemed desirable by society is available by state since the General Assistance Program is administered on a county basis rather than on a state basis. However, we do have data on the average G.A.P. income supplement by states on a comparable basis with the number of recipients. Therefore, we substitute the average monthly G.A.P. for the minimum income deemed desirable by society. We have collected these data by state annually for the period 1951-59 [13].

In addition to the above data, we have collected the following data on an average monthly basis, by state, annually for the period 1951-59: (a) average manufacturing wages, (b) population, (c) unemployment, and (d) nonagricultural employment [11]. We are using the average monthly manufacturing wage as the alternative cost of receiving a G.A.P., i.e., the wages which could have been earned if the

<sup>8</sup>This is not meant to imply that the demand for unemployment benefits is independent of the absolute level of the benefits or their level relative to wages, but only that they must be treated differently in the analysis. In addition, one generally must choose seasonal work in order regularly to demand unemployment benefits.

<sup>9</sup>In a few instances these payments are given where funds are needed to supplement the help given by the joint federal-state programs.

consumer chose a full-time job.<sup>10</sup> The rate of unemployment is necessary since it measures the number of consumers with zero wage alternatives. Unfortunately the general unemployment rate was unavailable by year by state, so we chose the insured unemployment rate. Non-agricultural employment, (d) above, when divided by population gives us a measure of the degree of urbanization by state.

Such a measure is useful for two reasons: (1) it has been suggested that the discount factor ( $k$ ) is smaller in rural communities than in urban communities, i.e., rural residents believe in the "Protestant Ethic" more strongly than do their urban counterparts; and (2) G.A.P. income supplements are not equally easy to obtain across the states and, as will be shown below, the degree of urbanization is closely and positively related to the ease of obtaining G.A.P. and, hence, is a good proxy for it. Thus, the greater the degree of urbanization, the greater will be the number of consumers demanding a G.A.P.

Lastly, we have classified the states according to the following three criteria: (1) allowance for an employed person in the household, (2) residence requirements, and (3) definition of "need" used in determining eligibility for a G.A.P. income supplement [14]. Each of these criteria represents in some sense the "ease" of getting on the G.A.P. rolls. Unfortunately, this classification could only be made for the terminal year of study.

### III. *The Estimation Procedure*

The data we have collected are a combination of cross-section and time-series, allowing us to take account of the effect of migration of consumers from the states in which G.A.P. payments are low to states where G.A.P. payments are high. If, as one would expect, such an adjustment will require several years, we can expect the disturbance terms for a given state in a given year to be positively correlated with the disturbance terms for that state in the closely preceding and succeeding years. This relationship between the disturbance terms allows us to increase the efficiency of our estimates by utilizing Aitken's generalized least squares [1]. Zellner has developed a technique for using the Aitken generalized estimators when the disturbance variances and covariances are unknown [8]. This method allows us to make use

<sup>10</sup> We would prefer the average wage rate available to the G.A.P. recipient. However, this would require knowledge of the distribution of G.A.P. recipients by skill by state and the wage rates by skill by state. Since these data are not available to us, we chose average monthly manufacturing wages as the best available alternative. In addition, if the distribution of general assistance recipients by skill is different from the skill distribution embodied in the average manufacturing wage, this measure is erroneous. However, as long as the difference in skill distribution does not differ systematically, no bias will result.

of both the cross-section and the time-series aspects of our data.

Unfortunately, as we noted in the preceding section, the classification of the states by the ease of getting on the G.A.P. rolls was only available for the terminal year of the data. Therefore, we are faced with the following choices for the handling of this variable: (1) disregarding what is certainly a significant determinant of the number of G.A.P. recipients, (2) assuming that the classification for the terminal year is valid throughout the period, or (3) finding a proxy variable for this classification which is available for the entire period.

Fortunately some work has already been done which indicates that the ease of getting on the G.A.P. rolls should be positively related to the degree of urbanization of the state.<sup>11</sup> In order to test this hypothesis we regressed our measure of degree of urbanization ( $NA^*$ ) for the terminal year (1959) on the three classification variables representing the ease of getting on the G.A.P. rolls with the following results:

$$(3.1) \quad NA_i^* = 30.62 - 0.75E_i - 3.58R_i - 1.50M_i; \quad \bar{R}^2 = 0.314; \quad m = 48;$$

(0.85) (0.74) (1.18) (0.70)

where

$NA_i^*$  = nonagricultural employment as a percentage of the population in the  $i$ th state;

$E_i$  = a dummy variable taking the following value:

0—if the  $i$ th state allows G.A.P. recipients to be employed;

1—if the  $i$ th state sometimes allows G.A.P. recipients to be employed;

2—if the  $i$ th state never allows G.A.P. recipients to be employed.

$R_i$  = a dummy variable taking the following value:

0—if the  $i$ th state has no residence requirement;

1—if the  $i$ th state has a residence requirement but allows exceptions;

2—if the  $i$ th state has a residence requirement and allows no exceptions.

$M_i$  = a dummy variable taking the following value:

0—if the  $i$ th state responds to any "kind" of a need of an eligible family or individual;

1—if the  $i$ th state responds to only specified types of need;

2—if the  $i$ th state responds to only "emergency" needs.

<sup>11</sup> This hypothesis is a combination of two studies. The first, by J. Heffernan, indicates that if the population is ranked on a liberal-conservative scale, those on the liberal end of the scale are more favorable to public assistance [4]. The second study by V. O. Key indicates that the more urbanized areas are more likely to fall on the liberal end of the scale [5]. Therefore, the more urbanized areas are likely to be more favorable to the G.A.P. program and to make the eligibility requirements easier to meet.

Equation (3.1) clearly indicates that the degree of urbanization as measured by the percentages of a state's population employed in non-agricultural pursuits is indeed a good proxy for the ease of getting on the G.A.P. rolls. All the coefficients of the dummy variables had the expected sign, and two of the three were significant at better than the 1 per cent level.

On the strength of equation (3.1) we have chosen the proportion of nonagricultural employment to population in each state as the proxy variable for ease of getting on the G.A.P. rolls. In addition, we assume that the distribution of income and ability within each state is constant for the period under consideration, so that we may express the number of assistance recipients in a given state as a percentage of that state's

TABLE 1—RESULTS OF GENERALIZED LEAST-SQUARES ESTIMATION OF COEFFICIENTS\*

Year	Equation (3.21)			Equation (3.22)			
	$a_1$	$b_{11}$	$b_{21}$	$a_2$	$b_{12}$	$b_{22}$	$b_{32}$
1951	0.4148 <sup>a</sup> (0.3244)	0.1236 <sup>d</sup> (0.0224)	0.1245 <sup>b</sup> (0.0622)	-0.7300 <sup>a</sup> (0.4817)	0.0684 <sup>d</sup> (0.0245)	0.1418 <sup>c</sup> (0.0671)	0.0649 <sup>d</sup> (0.0167)
1952	0.4195 <sup>a</sup> (0.3003)	0.0925 <sup>d</sup> (0.0208)	0.1326 <sup>c</sup> (0.0572)	-0.8122 <sup>b</sup> (0.4237)	0.0317 <sup>a</sup> (0.0198)	0.1952 <sup>d</sup> (0.0516)	0.0666 <sup>d</sup> (0.0147)
1953	0.3960 <sup>a</sup> (0.3502)	0.0935 <sup>d</sup> (0.0214)	0.0961 <sup>a</sup> (0.0738)	0.0673 <sup>a</sup> (0.5558)	0.0808 <sup>d</sup> (0.0257)	0.0938 <sup>a</sup> (0.0760)	0.0177 <sup>a</sup> (0.0198)
1954	0.3381 <sup>a</sup> (0.3039)	0.1141 <sup>d</sup> (0.0174)	0.0483 <sup>a</sup> (0.0393)	-0.1399 <sup>a</sup> (0.4992)	0.1006 <sup>d</sup> (0.0189)	0.0443 <sup>a</sup> (0.0388)	0.0251 <sup>a</sup> (0.0174)
1955	0.5493 <sup>b</sup> (0.3119)	0.1236 <sup>d</sup> (0.0182)	-0.0202 <sup>a</sup> (0.0525)	-0.2245 <sup>a</sup> (0.5043)	0.0967 <sup>d</sup> (0.0185)	-0.0422 <sup>a</sup> (0.0510)	0.0435 <sup>c</sup> (0.0161)
1956	0.7087 <sup>c</sup> (0.3060)	0.0887 <sup>d</sup> (0.0183)	0.0414 <sup>a</sup> (0.0521)	0.2102 <sup>a</sup> (0.4985)	0.0729 <sup>d</sup> (0.0175)	0.0402 <sup>a</sup> (0.0469)	0.0254 <sup>b</sup> (0.0148)
1957	0.6554 <sup>c</sup> (0.2982)	0.0850 <sup>d</sup> (0.0180)	0.0673 <sup>a</sup> (0.0424)	0.2868 <sup>a</sup> (0.5371)	0.0732 <sup>d</sup> (0.0177)	0.0583 <sup>a</sup> (0.0391)	0.0198 <sup>a</sup> (0.0169)
1958	0.1991 <sup>a</sup> (0.4248)	0.1100 <sup>d</sup> (0.0237)	0.1835 <sup>d</sup> (0.0578)	-0.8801 <sup>a</sup> (0.8324)	0.0825 <sup>d</sup> (0.0250)	0.1799 <sup>d</sup> (0.0550)	0.0544 <sup>b</sup> (0.0298)
1959	0.0670 <sup>a</sup> (0.4453)	0.1211 <sup>d</sup> (0.0237)	0.2090 <sup>c</sup> (0.0882)	-1.0300 <sup>a</sup> (0.9018)	0.0952 <sup>d</sup> (0.0274)	0.2180 <sup>a</sup> (0.0898)	0.0515 <sup>c</sup> (0.0339)
Mean	0.4164	0.1058	0.0981	-0.3614	0.0780	0.1033	0.0400

\* Standard errors appear in parentheses below the coefficients and all tests are two-tailed *t*-tests.

<sup>a</sup> Not significantly different from zero.

<sup>b</sup> Significantly different from zero at better than the 10 per cent level.

<sup>c</sup> Significantly different from zero at better than the 5 per cent level.

<sup>d</sup> Significantly different from zero at better than the 1 per cent level.

population. We can thus account for population differences over time and between states.

The equations estimated were of the following form:

$$(3.21) \quad N_{ij}^* = a_1 + b_{11} \left( \frac{P}{W_{ij}} \right) + b_{21} U_{ij} \quad i = 1, \dots, 48 \quad j = 1, \dots, 9$$

$$(3.22) \quad N_{ij}^* = a_2 + b_{12} \left( \frac{P}{W_{ij}} \right) + b_{22} U_{ij} + b_{32} N A_{ij}^*$$

$$(3.23) \quad N_{ij}^* = a_3 + b_{23} U_{ij} + b_{43} P_{ij} + b_{53} W_{ij}$$

$$(3.24) \quad N_{ij}^* = a_4 + b_{24} U_{ij} + b_{34} N A_{ij}^* + b_{44} P_{ij} + b_{54} W_{ij}$$

TABLE 1—Continued

Equation (3.23)				Equation (3.24)				
$a_3$	$b_{23}$	$b_{43}$	$b_{53}$	$a_4$	$b_{24}$	$b_{34}$	$b_{44}$	$b_{54}$
0.1189 <sup>a</sup> (0.9108)	0.1900 <sup>d</sup> (0.0671)	0.0423 <sup>d</sup> (0.0090)	0.00087 <sup>a</sup> (0.00357)	-1.1050 <sup>a</sup> (0.9484)	0.1700 <sup>c</sup> (0.0745)	0.0602 <sup>d</sup> (0.0177)	0.0247 <sup>c</sup> (0.0095)	0.0017 <sup>a</sup> (0.0038)
0.1368 <sup>a</sup> (0.7635)	0.1974 <sup>d</sup> (0.0596)	0.0314 <sup>d</sup> (0.0075)	0.00050 <sup>a</sup> (0.00274)	-1.0270 <sup>a</sup> (0.7140)	0.2103 <sup>d</sup> (0.0531)	0.0622 <sup>d</sup> (0.0154)	0.0134 <sup>b</sup> (0.0071)	0.00074 <sup>a</sup> (0.00251)
1.1120 <sup>a</sup> (0.8095)	0.1233 <sup>b</sup> (0.0730)	0.0359 <sup>d</sup> (0.0076)	-0.0033 <sup>a</sup> (0.0027)	0.6614 <sup>a</sup> (0.8869)	0.1021 <sup>a</sup> (0.0763)	0.0154 <sup>a</sup> (0.0206)	0.0311 <sup>d</sup> (0.0087)	-0.0024 <sup>a</sup> (0.0029)
-0.2308 <sup>a</sup> (0.6767)	0.1077 <sup>c</sup> (0.0393)	0.0398 <sup>d</sup> (0.0059)	0.00078 <sup>a</sup> (0.00204)	-0.4258 <sup>a</sup> (0.7601)	0.0848 <sup>b</sup> (0.0416)	0.0081 <sup>c</sup> (0.0183)	0.0385 <sup>d</sup> (0.0066)	0.0012 <sup>a</sup> (0.0021)
1.1200 <sup>a</sup> (0.6757)	0.0146 <sup>a</sup> (0.0498)	0.0435 <sup>d</sup> (0.0057)	-0.0028 <sup>a</sup> (0.0018)	0.4805 <sup>a</sup> (0.7483)	-0.0191 <sup>a</sup> (0.0529)	0.0295 <sup>b</sup> (0.0168)	0.0376 <sup>d</sup> (0.0061)	-0.0023 <sup>a</sup> (0.0020)
0.1777 <sup>a</sup> (0.6482)	0.1095 <sup>c</sup> (0.0508)	0.0299 <sup>d</sup> (0.0056)	0.00053 <sup>a</sup> (0.00558)	-0.1109 <sup>a</sup> (0.7063)	0.0916 <sup>a</sup> (0.0494)	0.0093 <sup>a</sup> (0.0154)	0.0280 <sup>d</sup> (0.0056)	0.0010 <sup>a</sup> (0.0017)
-0.3253 <sup>a</sup> (0.6341)	0.1281 <sup>d</sup> (0.0420)	0.0274 <sup>d</sup> (0.0054)	0.0018 <sup>a</sup> (0.0016)	-0.4464 <sup>a</sup> (0.7325)	0.1114 <sup>c</sup> (0.0409)	-0.0017 <sup>a</sup> (0.0176)	0.0276 <sup>d</sup> (0.0055)	0.0025 <sup>a</sup> (0.0017)
-2.0160 <sup>d</sup> (0.6882)	0.2608 <sup>d</sup> (0.0510)	0.0329 <sup>d</sup> (0.0062)	0.0050 <sup>d</sup> (0.0016)	-2.4270 <sup>c</sup> (0.9179)	0.2439 <sup>d</sup> (0.0526)	0.0162 <sup>a</sup> (0.0283)	0.0315 <sup>d</sup> (0.0067)	0.0053 <sup>d</sup> (0.0016)
-0.6812 <sup>a</sup> (0.5747)	0.2637 <sup>d</sup> (0.0883)	0.0386 <sup>d</sup> (0.0062)	0.00053 <sup>a</sup> (0.00095)	-1.1970 <sup>a</sup> (0.9775)	0.2584 <sup>c</sup> (0.0915)	0.0190 <sup>a</sup> (0.0344)	0.0370 <sup>d</sup> (0.0073)	0.00077 <sup>a</sup> (0.00100)
-0.0653	0.1550	0.0357	0.00043	-0.6219	0.1393	0.0261	0.0299	0.00095

where

$N_{ij}^*$  = per cent of  $i$ th state's population receiving general assistance payments in the  $j$ th year;

$P_{ij}$  = average monthly G.A.P. in the  $i$ th state for the  $j$ th year;

$W_{ij}$  = average monthly manufacturing wage in the  $i$ th state for the  $j$ th year;

$U_{ij}$  = unemployment rate in the  $i$ th state for the  $j$ th year;

$NA_{ij}^*$  = nonagricultural employment as a percentage of population in the  $i$ th state for the  $j$ th year.

We have, then, four sets of nine cross-sectional equations. The nine equations within each set are to be jointly estimated by the method of generalized least squares.

#### IV. *The Empirical Evidence*

##### *A. The Results of the Estimation*

The generalized least-squares results of the equation sets described in the previous section are given in Table 1. Inspection of Table 1 indicates that every equation contains at least one coefficient which is significant at better than the 1 per cent level, and in 26 of the 36 equations estimated there were at least two significant coefficients (at the 10 per cent level). Thus, our model is able to explain significantly the observed variation in G.A.P. recipients.<sup>12</sup>

In Table 2 we have summarized the results in Table 1 by presenting cumulative frequencies of coefficients significant at the 1, 5, and 10 per cent levels by equation set and coefficient.<sup>13</sup>

Inspection of Table 2 reveals that the G.A.P. income variable was consistently the most significant variable and had the correct sign in all cases. This is true whether this variable appeared in absolute terms, ( $P$ ) in equation sets (3.23) and (3.24), when it was significant in 16 of 18 cases at the 1 per cent level and in all 18 cases at the 10 per cent level, or in relative terms, ( $P/W$ ) in equation sets (3.21) and (3.22), when it was significant in 17 of 18 cases at the 1 per cent level.

The coefficient of the rate of unemployment ( $U$ ) was significant in three of 18 cases at the 1 per cent level and seven of 18 cases at the 10 per cent level in equation sets (3.21) and (3.22), while this coefficient was significant in seven of 18 cases at the 1 per cent level and in 15 of 18 cases at the 10 per cent level in equation sets (3.23) and

<sup>12</sup> Since the estimating technique used does not minimize the squared deviations around each individual regression, the multiple  $R^2$  is not a relevant statistic.

<sup>13</sup> The standard errors on which the significance levels are based are asymptotic standard errors. However, Zellner [9] has indicated that for the model which he investigated the finite sample results are not very different from the infinite sample results whenever  $n \geq 20$ . (In our case  $n = 48$ .)

TABLE 2—SIGNIFICANCE LEVELS OF COEFFICIENTS IN TABLE 1\*

Coeffi- cient of	<i>P/W</i>			<i>U</i>			<i>NA</i> *			<i>P</i>			<i>W</i>		
	1%	5% <sup>a</sup>	10%	1%	5% <sup>a</sup>	10%	1%	5% <sup>a</sup>	10%	1%	5% <sup>a</sup>	10%	1%	5% <sup>a</sup>	10%
Sign. Level															
Equation Set															
(3.21)	9	9	9	1	3	4	x	x	x	x	x	x	x	x	x
(3.22)	8	8	8	2	3	3	2	4	6	x	x	x	x	x	x
(3.23)	x	x	x	5	7	8	x	x	x	9	9	9	1 <sup>b</sup>	1 <sup>b</sup>	1 <sup>b</sup>
(3.24)	x	x	x	2	5	7	2	2	3	7	8	9	1 <sup>b</sup>	1 <sup>b</sup>	1 <sup>b</sup>

\* All significance tests are two-tailed *t*-tests.

<sup>a</sup> These classes are constructed to contain cumulative total of significant coefficients.

<sup>b</sup> This coefficient does not have the expected sign.

(3.24). Moreover, the coefficient of unemployment had the correct sign in all but three cases, and in each of these the standard error exceeded the coefficient.

The coefficient of our proxy variable for the ease of getting on the G.A.P. rolls had the expected sign in 17 of 18 cases where it appeared, and in the one case of wrong sign the standard error considerably exceeded the value of the coefficient. In addition this variable was significant in four of the 18 cases at the 1 per cent level and in nine of the 18 cases at the 10 per cent level.

In equation sets (3.23) and (3.24) the coefficient of average manufacturing wages (*W*) is significant in only two of 18 cases, once in each equation set associated with the absolute wage level, and has a positive rather than the expected negative sign. This coefficient does have a negative sign, albeit insignificant, in only four of 18 cases. However, considering that in 10 of 14 cases where the coefficient is positive, the standard error exceeds the coefficient, while in three of four negative cases the coefficient exceeds the standard error, it is very likely that our results are simply due to chance.

In addition, it can be argued that interstate differences in average manufacturing wages reflect to a large extent differences in skill and educational distributions of the population and, hence, do not reflect differences in the alternative cost of receiving G.A.P.<sup>14</sup> In view of this problem we have re-estimated equation sets (3.23) and (3.24), excluding average manufacturing wages, and we present the results of this estimation in Table 3.

<sup>14</sup> If, as is sometimes asserted, wages by occupation do not differ by state, then the wage variable is irrelevant in a cross-section analysis.

The results of equation sets (3.23\*) and (3.24\*) presented in Table 3 indicate that the G.A.P. income variable is again the most significant of all variables included, being significant in 17 of 18 cases at the 1 per cent level and in all 18 cases at the 10 per cent level. The sign was as expected in all cases. The coefficient of unemployment was significant at the 1 per cent level in five of 18 cases and at the 10 per cent level in 13 of 18 cases. This variable had the wrong sign in only one of 18 cases, and in this case the standard error exceeded the coefficient by over four times.

TABLE 3—EQUATION SETS (3.23) AND (3.24) EXCLUDING AVERAGE MANUFACTURING WAGES<sup>‡</sup>

Year	Equation Set (3.23*)			Equation Set (3.24*)			
	$a_1^*$	$b_{21}^*$	$b_{22}^*$	$a_1^*$	$b_{21}^*$	$b_{22}^*$	$b_{44}^*$
1951	0.2441 <sup>a</sup> (0.3168)	0.1507 <sup>a</sup> (0.0598)	0.0482 <sup>d</sup> (0.0075)	-0.7803 <sup>a</sup> (0.4759)	0.1502 <sup>a</sup> (0.0651)	0.0607 <sup>d</sup> (0.0172)	0.0292 <sup>d</sup> (0.0083)
1952	0.1992 <sup>a</sup> (0.2942)	0.1514 <sup>d</sup> (0.0538)	0.0367 <sup>d</sup> (0.0067)	-0.8904 <sup>d</sup> (0.4218)	0.1908 <sup>d</sup> (0.0483)	0.0631 <sup>d</sup> (0.0150)	0.0161 <sup>a</sup> (0.0066)
1953	0.2115 <sup>a</sup> (0.3500)	0.1219 <sup>b</sup> (0.0711)	0.0341 <sup>d</sup> (0.0067)	0.0178 <sup>a</sup> (0.5614)	0.1127 <sup>a</sup> (0.0743)	0.0155 <sup>a</sup> (0.0206)	0.0283 <sup>d</sup> (0.0083)
1954	-0.0952 <sup>a</sup> (0.3117)	0.0865 <sup>a</sup> (0.0364)	0.0442 <sup>d</sup> (0.0056)	-0.1555 <sup>a</sup> (0.4979)	0.0733 <sup>b</sup> (0.0378)	0.0089 <sup>a</sup> (0.0178)	0.0412 <sup>d</sup> (0.0062)
1955	0.2535 <sup>a</sup> (0.3212)	0.0160 <sup>a</sup> (0.0487)	0.0427 <sup>d</sup> (0.0057)	-0.2132 <sup>a</sup> (0.5097)	-0.0107 <sup>a</sup> (0.0509)	0.0323 <sup>b</sup> (0.0171)	0.0347 <sup>d</sup> (0.0061)
1956	0.2763 <sup>a</sup> (0.3179)	0.0338 <sup>a</sup> (0.0054)	0.0785 <sup>d</sup> (0.0488)	0.1523 <sup>a</sup> (0.5006)	0.0630 <sup>a</sup> (0.0466)	0.0119 <sup>a</sup> (0.0151)	0.0299 <sup>d</sup> (0.0055)
1957	0.1677 <sup>a</sup> (0.3119)	0.0953 <sup>a</sup> (0.0397)	0.0328 <sup>d</sup> (0.0052)	0.2779 <sup>a</sup> (0.5431)	0.0774 <sup>b</sup> (0.0382)	0.0007 <sup>a</sup> (0.0175)	0.0314 <sup>d</sup> (0.0054)
1958	-0.5144 <sup>a</sup> (0.4306)	0.2155 <sup>d</sup> (0.0543)	0.0423 <sup>d</sup> (0.0064)	-0.7190 <sup>a</sup> (0.8284)	0.1946 <sup>d</sup> (0.0536)	0.0176 <sup>a</sup> (0.0728)	0.0388 <sup>d</sup> (0.0070)
1959	-0.5253 <sup>a</sup> (0.4390)	0.2417 <sup>d</sup> (0.0833)	0.0410 <sup>d</sup> (0.0059)	-0.8181 <sup>a</sup> (0.8874)	0.2267 <sup>a</sup> (0.0868)	0.0197 <sup>a</sup> (0.0332)	0.0371 <sup>d</sup> (0.0068)
Mean	0.0232	0.1236	0.0445	0.3476	0.1198	0.0256	0.0319

<sup>‡</sup> Standard errors appear in parentheses below the coefficients, and all tests are two-tailed *t*-tests.

\* This superscript is used to distinguish these results from those of equation sets (3.3) and (3.4) with average manufacturing wages included as a variable.

<sup>a</sup> Indicates that this coefficient is insignificant.

<sup>b</sup> Indicates that this coefficient is significant at better than the 10 per cent level.

<sup>c</sup> Indicates that coefficient is significant at better than the 5 per cent level.

<sup>d</sup> Indicates that coefficient is significant at better than the 1 per cent level.

Our proxy variable for ease of getting on the G.A.P. rolls had the expected sign in all cases and was significant at the 1 per cent level in two of nine cases and at the 10 per cent level in three of nine cases.

### *B. The Interpretation of the Results*

The results discussed in the above section clearly indicate that the level of G.A.P. income is an important variable in explaining the percentage of a state's population which is on the G.A.P. rolls. The unemployment rate results indicate that while the variance in consumers with zero wage alternatives does explain a significant portion of the variance in G.A.P. recipients, it is an inferior explanatory variable to the G.A.P. income variables. In addition, our proxy variable for the ease of qualifying for the G.A.P. rolls presents further evidence for the conclusion that consumers react to the costs and returns of the G.A.P. program in determining whether or not to demand a G.A.P. income.

However, we must point out that such a relationship between G.A.P. income and numbers of G.A.P. demanders does not imply that society should not give G.A.P. income supplements. Consumers may have the "best" of reasons for choosing a G.A.P. income rather than working, e.g., the case of a widowed mother of several children. In this case there is a cost to society of such a person's working, and hence we institute a program which, while it does not force all widowed mothers to stop working, makes it more likely that they will come to this decision. The fact that some consumers may take steps to qualify for the program does increase the marginal social cost of the program, but does not imply that at the current levels the marginal social costs exceed the marginal social benefits.

In addition to these problems of interpretation of the results, we might wonder if possibly the use of the dummy variables themselves rather than the proxy we used might have affected the results in some way. In particular, if the level of the G.A.P. incomes is related to the ease of getting on the rolls and this ease is not clearly reflected in our proxy variable, part of the significance of the G.A.P. income variables may be due to this omission.

To test this hypothesis we ran single-equation least squares on the terminal years data of equation sets (3.22) and (3.24\*), and then ran these same data, replacing the proxy variable ( $NA^*$ ) with the dummy variable used in the previous section. The estimates of the coefficients of the G.A.P. income variables and unemployment are given in Table 4.

Inspection of Table 4 shows that, contrary to the above hypothesis, the substitution of the dummy variables for our proxy increases both the level of the coefficients of the G.A.P. income variables and their

TABLE 4—TERMINAL YEAR REGRESSION RESULTS FOR PROXY VARIABLE COMPARED TO DUMMY VARIABLES

Regression	Coefficient			$R^2$	$\bar{R}^2$
	$P/W$	$P$	$U$		
Proxy	0.0648 <sup>a</sup> (0.0524)		0.2376 <sup>a</sup> (0.2010)	0.336	0.290
		0.0231 <sup>b</sup> (0.0117)	0.2393 <sup>a</sup> (0.1957)	0.368	0.325
Dummies	0.1394 <sup>b</sup> (0.0613)		0.3088 <sup>a</sup> (0.2163)	0.278	0.192
		0.0423 <sup>a</sup> (0.0134)	0.3111 <sup>a</sup> (0.2052)	0.345	0.267

<sup>a</sup> Significant at 1 per cent level.

<sup>b</sup> Significant at 5 per cent level.

<sup>c</sup> Not significant.

significance. This at least indicates that our results are not due to the exclusion of the ease of getting on the G.A.P. rolls as measured by our dummy variables. However, the actual enforcement of the rules we have used may differ systematically by state and be positively related to our G.A.P. income variables. However, this is simply a conjecture, and no data are available to test it.

If we are willing to make a few heroic assumptions, we may get some further evidence concerning the effect of the level of G.A.P. income on the demand for assistance. Let us assume that the constant term represents the "hard core" of unemployables. We can then ask the following question: What would be the average percentage of the population receiving G.A.P. in the various states if unemployment were zero and there were no "hard core" of unemployables. In other words, we take equation sets (3.21) and (3.23\*), assuming that the unemployment is zero, and compute expected levels of percentage receiving assistance payments. From these expected levels we subtract the "hard core" of unemployables, i.e., the constant term, wherever it is positive. We then divide the remainder by the average of the percentages of the various states' populations receiving assistance. Thus, we get an estimate of the percentage of general assistance recipients due to the level of the assistance payment rather than zero wage alternatives. These percentages are presented in Table 5.

Table 5 shows that the percentage of assistance recipients due to the level of assistance payments ranges from 22.9 to 87.3. Considering the standard errors of the original estimates from which these were

TABLE 5—THE PERCENTAGE OF GENERAL ASSISTANCE RECIPIENTS DUE TO THE LEVEL OF ASSISTANCE PAYMENTS

Equa- tion Set	Year									
	1951	1952	1953	1954	1955	1956	1957	1958	1959	Mean
(3.21)	51.3	40.9	49.8	59.1	51.9	22.9	22.9	50.7	64.5	46.0
(3.23*)	62.5	59.9	60.2	87.8	74.2	61.8	66.7	80.7	87.3	71.2

derived we cannot place a great deal of faith on any one year's estimate. However, the two means may be considered with a great deal more confidence, and both of these indicate that nearly one-half of all assistance recipients are not on assistance due to zero wage alternatives.

It is important to remember that the validity of the above method depends on several conditions: (1) that the underlying relationship is in fact linear even for large changes in the explanatory variables; (2) that the variables themselves measure what we claim they measure. We trust that the latter condition holds, but we make no pretensions that the former does.

### V. *Summary and Conclusion*

On the theoretical level we have shown that the demand for G.A.P. can be treated as a special case of the traditional theoretical treatment of the demand for leisure. In this treatment we conclude that the decision of whether or not to demand a G.A.P. depends on: (1) the consumer's earned income; (2) the minimum income which society deems desirable; and (3) the discount factor which the consumer applies to relief doles. The analysis suggests that in addition to the "hard core" of unemployables and low-income earners the set of G.A.P. recipients should contain consumers who deliberately have chosen to bring their earned income below the minimum set by the society.

On the empirical level the analysis reveals the following results:

1. The G.A.P. income variables are positively related to numbers of assistance recipients, and in addition these variables are more significant than any other used in the analysis.

2. While the variance in the number of consumers with zero wage alternatives, as measured by the rate of insured unemployment, was positively related to numbers of assistance recipients, it was not as

significant a factor in explaining the variance in these assistance recipients as the G.A.P. income variables.

3. The ease of meeting the qualifications to get on the G.A.P. rolls, as measured by a proxy variable indicating degree of urbanization, is positively related to numbers of recipients and reinforces the conclusion in (1), since this represents a cost factor in the G.A.P. program.

In conclusion, we reiterate the fact that the results contained do not imply that the G.A.P. program should be stopped or cut back, but only indicate that G.A.P. recipients are like the remainder of consumers in that they react to economic incentives.

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# COMMUNICATIONS

## Notes on Marxian Economics in the United States<sup>1</sup>

*Cuius regio eius religio*

### I

Paul Baran's premature death March 26, 1964, deprived U.S. economics of its last eminent academic practitioner professing Marxism in both theory and policy. Considering the importance of Marxian economics in the current world, and likewise the U.S. tradition of liberalism, questions arise. What really goes on here? How much more freely do the hundred flowers really bloom, and the hundred schools of thought contend, in Jefferson's America than in Mao's China?

The vacuum created by Baran's death may conceivably be only temporary. This writer knows of only marginally orthodox resident candidates for the succession, but more promising prospects may exist. There is also a wide world from which to draw, including not only foreigners like Baran himself but also U.S. refugees from Joe McCarthy and successors. It is also true that Socialists of whatever variety have never thrived in U.S. departments of economics.<sup>2</sup> With the Cold War, however, the tiny minority of Marxists in particular has shrunk with peculiar sharpness, considering the over-all expansion in the numbers of university economists. Vacancies created by this expansion, or by deaths and retirements, go to less controversial figures. Baran's own place at Stanford will probably share the general fate. Questions about the hundred flowers and schools of thought remain decidedly—some say ominously—in point.

It is no part of my intention to memorialize Baran himself. This is a task for others who knew him better than I did. Yet I cannot avoid two features of his career. (1) As a Soviet citizen, Baran had sufficient courage to maintain Bukharinist deviations during Stalin's heyday, belying the stereotype of the Communist scholar degenerating to a Party mouthpiece. (2) By reason less of rhetorical and analytical competence than of resistance first to Stalinism and then to embryonic U.S. Fascism, Baran's international reputation and that of his writings—notably *The Political Economy of Growth* [1]—outrun their domestic fame. This is what makes the maintenance of Baran's dissident tradition more important, particularly to the international stature of U.S. economics, than academic deans or chairmen realize, with minds preoccupied by trustees, regents, or legislators.

<sup>1</sup>Parts of this essay draw upon a public lecture on "Academic Freedom Problems in American Economics," given at Kobe, Japan, in January, 1964. The writer is grateful to Dean Hiroshi Shinjo of Kobe University for criticisms of the original lecture.

<sup>2</sup>There is substantial historical literature on the spotty U.S. record in academic freedom issues involving economic matters, concentrating on the half-century prior to World War II. Two citations may suffice: Richard Hofstadter and Walter P. Metzger [4, Ch. 9] and Harold Laski [5, pp. 355-60, 368 f., 381-92].

## II

Why has Marxian economics always been so weak in our economics departments, and why has it declined since the end of World War II? There are two obvious explanations, one complacent and the other alarmist; the truth lies somewhere between the two.

The complacent explanation is of course that Marxian economics, like Physiocracy, *Historismus*, and Social Darwinism, has lost out in free and fair competition on "the market place of ideas" of the Affluent Society. Where it survives, it is only a secular religion, imposed by rote in totalitarian regimes or by totalitarian political parties. The alarmist explanation is that Marxism in general, including Marxian economics, is persecuted systematically by the U.S. power elite; its weakness proves both the anemic character of U.S. academic freedom and the imminence of Star-Spangled Fascism.

There are at least two dubious features in the complacent or competitive explanation. One is the somewhat unusual outcome of ideological competition in the United States, as compared to other countries of the anti-Communist world. The other is the institutional working of the U.S. systems of graduate training for economists and achievement of academic tenure, as they affect the prospective economics teacher with Marxian leanings.

To say that Marxian economics is weak in the United States involves comparison not only with the Communist blocs, but with almost any country one cares to name (excluding the Iberian peninsula, South Africa, Antarctica, and similar Rightist or illiterate enclaves). Most other countries foster Marxian economists of varying popularity, influence, competence, and sophistication. In many places, the Soviet textbook, *Political Economy* [11] (not a prepossessing standard-bearer), has outsold Marshall and Samuelson combined, with the aid of some loss-leader pricing. On many, if not most, Japanese campuses, for example, the most popular U.S. economists are Paul Sweezy and Paul Baran, and the Labor Research Associates' analyses of the U.S. scene take precedence over anything the Council of Economic Advisers or the National Bureau can offer.

This is not to suggest that "they" are right while "we" are wrong. Ideological competition in academic market-places overseas is marred by prejudices operating in an anti-American, and therefore anticapitalist, direction. Foreign intellectuals, economists included, are simultaneously jealous of our (relatively) high pay and comfortable working conditions; they are contemptuous of the ambiguous position of the "egghead" in our business society, and exaggerate our resistant racisms. We can see the moles in their eyes, but may there not also be beams in our own—meaning in this context, imperfections in U.S. ideological competition?

Here let us overlook the ownership and control of mass communications, to consider only the U.S. systems of graduate training for economists and the limitations on academic freedom and tenure in this country. Certain features that we feel necessary to maintain reasonable standards of competence seem positively menacing from the viewpoint of a foreign Marxist. Without accepting Seligman's [9] dismissal of contemporary orthodoxy

as technique for its own sake, one can admit that, in raising the mathematical and statistical standards of U.S. economics, we have probably displaced something. What has that "something" been? It varies, no doubt, from one institution to another, but basically it is a melange of foreign languages, economic and intellectual history, philosophy, law, allied social studies, and/or (more usually and) concern with unconventional and "obsolete" positions within economics itself. The neophyte with simultaneous leanings to both academic economics and analytical Marxism faces, at the outset of graduate training in the former, some four to six years in which the latter interest pays off, if at all, only by assimilation in particular planning and programming techniques (input-output and its applications). If the "Economics of 1984" leaves him cold, he has nowhere to go but abroad, nothing to do at home but convert to some variant of more conventional wisdom, or abandon theoretical economics altogether.

Passing from training to tenure problems, the most "natural" approach, internationally speaking, probably resembles the Austro-German *Katheder* system more closely than the U.S. one. A typical *Katheder* consists of an Ordinary (Full) Professor, an Extraordinary (Associate) Professor, an Instructor, and one or two Assistants. (There are also *Privat-Dozenten*, licensed teachers marginal to the system, whom we ignore.)<sup>3</sup> Once the aspirant is under the aegis of the *Katheder*, even as a lowly Assistant, some modest future is secure for him. He may never rise, and may live out his career as a perpetual malcontent. However, Fascism and revolution aside, he has tenure, his salary escalates with his years of service and family responsibilities, and he ends his days on a modest pension. He can live the life of "ein Mann, der anders denkt," as controversially (or conventionally) as he pleases. In the United States a similarly limited and stuffy approach to Nirvana normally requires at least seven years in the limbo of short-term contracts, and/or promotion to associate professor. In terms of years and hostages to fortune, the representative U.S. economist withdraws his spouse from the "Ph.T." labor market, and brings one or more children into the world, before passing under the tenure umbrella. This is no "fate worse than death" in a growing economy with teacher shortages—unless, of course, our aspirant becomes *persona non grata* to the hiring authorities! This he may do in a number of ways, none surer than premature labeling as a controversial figure. To put it bluntly, the aspiring Marxist with academic ambitions had better hold his Marxism in cold storage for five or ten years until he has tenure at a good school and his youthful fervor is diluted with fuddy-duddyism, alias maturity. Small wonder if, as an economist, he avoids the academic calling, or if, as an academician, he selects some less "sensitive" department!

Mary McCarthy's *Groves of Academe* [6] features an obnoxious English teacher who wins tenure from a self-consciously liberal college president by

<sup>3</sup>To ignore the *Privat-Dozenten* is admittedly to overrate the liberalism of the Austro-German tenure system, since many a dissident, including Karl Marx as an aspiring philosopher, was left to wither on this vine. The *Privat-Dozent* had tenure—in an economically meaningless form. He was licensed to teach, but received no salary. His income came from the fees of whatever students cared to study under his direction.

inventing a Marxist background, complete with fictitious Communist Party membership, and daring the president to let him go. Whatever happens in English at "Jocelyn College," economics in flesh-and-blood institutions is rather different. The most famous case in point is that of Paul Sweezy, already mentioned as one of America's most influential economists among "progressive intellectuals" overseas. Sweezy did not achieve tenure at Harvard, shortly after the publication of his *Theory of Capitalist Development* [10]. Under the Harvard "up or out" system he would presumably have been asked to resign, unless another vacancy opened up within his ken. In fact, he resigned well before the deadline to accept a temporary government position. Thus far, the Sweezy story is no different from dozens of others. There is little if any evidence connecting his Marxism with the (extraordinarily close) Harvard decision against his promotion to tenure rank. Only the sequel sets his case apart. Other losers in "the Harvard competition" are recommended for, and achieve, tenure positions at good universities. Sweezy, on the other hand, was supported for no desirable academic assignments whatever, and achieved none. Thanks to private means, he was able to co-found the Socialist journal *Monthly Review*, and later the associated Monthly Review Press. Twenty years after, there are Marxists who insist that Sweezy was purged for his beliefs, first from Harvard and then from academic life. The charge is palpably false as regards Harvard, but not as regards the academic community, where "we have no academic freedom problems; we are careful whom we hire."

Given the imperfections in our ideological competition, it is not difficult to rationalize them under the rubric of "maintaining standards." In terms of contemporary jargon about multiple goals and trade-offs between them, my own rationalization (to foreign questioners) is rather crude. Four of the professional goals, conscious or otherwise, of academic economists are (1) high real incomes, including such psychic elements as social prestige and comfortable working conditions; (2) low responsibilities of what medical men would call the "scut-work" of the profession, namely the teaching and administration of unpromising and disinterested "warm bodies," alias students, from freshmen on up; (3) freedom to set our own professional standards and choose our own colleagues; and finally (4) freedom to expound and publish what we believe correct and expedient—academic freedom in the conventional sense. For the sake of the first three goals, we permit some erosion of the fourth, not for ourselves but for our junior, pre-tenure colleagues. We may have chosen consciously or viscerally, wisely or foolishly, but we have chosen. So—consciously or viscerally, wisely or foolishly—have our foreign (and some domestic) critics. Whether the U.S. choice involves important elements of "selling out" or becoming "kept cats," God alone can determine, but many can suspect. Either choice, ours or theirs, can be honestly wrong or, for that matter, dishonestly right. If the U.S. choice be wrong, it is within our power to modify it in the direction of foreign practice to a greater extent than foreigners are free to move in the direction of the U.S. system.

Which is in fact the correct choice? With the "subjectivist bias" of "bourgeois decadence," I am quite unable to say. I personally favor some revision

of the trade-offs between our goals in the direction of what one observes abroad, but only to refurbish the international repute of U.S. liberalism here and now, not for the sake of abstract virtue or the correct line *sub specie aeternitatis*.

### III

If the low standing of U.S. economic Marxism cannot be explained exclusively by perfect competition between ideas, the rival explanation—persecution and discrimination—is likewise incomplete. A number of its difficulties are fairly obvious.

In the first place, if U.S. Marxism is weak in any pure or self-assertive form, it is also strong as a pervasive element in an eclectic whole. (The mutual consistency of the Marxist and "bourgeois" parts of this eclectic whole may be questionable, as characteristically happens with eclectic doctrines.) We might say that U.S. academic economics is simultaneously full of Marxism and empty of Marxists!

The "eclectic whole" of the last paragraph is left-wing Keynesianism of the liberal-labor, or "lib-lab" variety. Its strongest proponent is perhaps the late-model Joan Robinson. In the United States the name of Leon Keyserling, a nonacademician, comes to mind. Academic left-wing Keynesianism is pro-government, pro-planning, pro-labor, pro-redistribution, anti-business, anti-free market, and antimilitary. It is, in advanced countries at least, more Social Democratic than Leninist. It is reformist rather than revolutionary in politics, parliamentary rather than conspiratorial. It is, as we have suggested, eclectic in its intellectual borrowings. Keynesians are not only at least as Keynesian as they are Marxist, but also as Veblenian, or sometimes even Marshallian. (The orthodox Marxist, by contrast, hates to borrow outside the fold. If religious analogy is permissible, the U.S. lib-lab is Unitarian where the Marxian is Fundamentalist.) On specific public policy questions, however, lib-lab and Marxian views have run parallel with sufficient frequency to disturb William Buckley [3],<sup>4</sup> Merrill Root [8], and the *National Republic*. Whether the lib-lab or the New Frontiersman "dominates" our academic economics is a complex, if not a meaningless, question and permits no uniform or easy answer, but can we not agree that, dominant or not, this eclectic Keynesian Left, Marxian overtones included, has suffered no widespread or effective persecution?

Even turning from lib-labs to forthright Marxists, the domestic, resident campus economist is far from the whole story. Marxists work in unions, research organizations, journalism, the Civil Service—perhaps even Madison Avenue—in larger proportions than in universities. On college campuses, the Marxian economist, often self-trained, is found more frequently pursuing some other interest in some less sensitive department. A few outsiders, including Sweezy, not only lecture at leading universities but hold temporary appointments for a term or a year at a time. An occasional economics department compromises with suspicious trustees or legislators and presents the Marxian

<sup>4</sup>Incidentally, none of the Yale economists attacked by Buckley was a Marxist, although several might have been called "lib-labs."

position to graduate students through a series of visitors from various European and Asian countries, including Iron Curtain borderlands like Poland and Yugoslavia. These visitors' offerings are too scattered and irregular to make up an organized program in Marxian economics through Marxian eyes, but they help Socialist students devise such programs independently of the permanent faculty.

We should also mention the various Marxian "seminars" or "study groups" which meet openly, if sporadically, on many famous campuses. These informal groups, always including and sometimes stressing the economic side, are conducted by faculty members of "other" departments (from the humanities to the natural sciences), by off-campus Marxists, or by advanced students, usually from countries whose curricula have stressed Marxist points of view.

How seriously these groups are taken, and what level of sophistication they attain, is hard to guess. The thrill of forbidden fruit seldom outweighs for a long period the lack of academic credit—especially where the fruit is not really forbidden! Yet there have been exceptions to this generalization, as at Harvard in the 1930's and Berkeley in 1947-52. Conservative economists have sometimes complained about students slighting sound economics in favor of seditious seminars and occasionally have tried to drive off-campus such unfair competition to their mummified lecture notes. What is more important to the present discussion is that the seminars run openly, and that the professors or graduate students who organize and conduct them are seldom discriminated against in any significant way, unless the seminar's "unity of theory and practice" develops into riotous demonstrations or recruiting for the Spanish Civil War. Rather than being suppressed, Marxian seminars and study groups are generally tolerated as buffers between timid administrations and impetuous students, and as providing the latter a chance to "let off steam."

Nature abhors a vacuum, and if there existed any crypto-Fascist conspiracy to suppress Marxian economic thought in the United States, it would logically include replacement of even Left Keynesianism with some more conservative brand of revealed truth. Such campaigns do in fact exist, but remain toothless and anemic, except at the kept colleges of particular business groups or religious sects. (Harding College in Arkansas and Grove City College in Pennsylvania are prototypes of this sort of institution.) Beyond these vest-pocket academies, the conservative anti-Keynesian campaign takes such unexceptionable forms as distributing in cheap editions the works of Austrian School and Social Darwinian writers, providing summer employment for college teachers within the management cadres of Big Business, and sponsoring seminars where invited speakers propound conservative doctrine.

What has this campaign accomplished to date? A *Gedankenexperiment* may be in point: Suppose that a college or university had the choice of inviting as visiting professor for a year, at no cost to itself, either an outstanding off-campus Marxian economist like Victor Perlo or an eminent off-campus conservative economist like Henry Hazlitt. Which would its economists prefer? My forecast would be that, Hardings and Grove Cities aside, a full two-

thirds would select Perlo. If so, the conservative counteroffensive has thus far been a failure.

Anticonservative feeling among university economists sometimes extends even further, to cover fellow-academicians as well as eminent outsiders. On at least a substantial minority of U.S. campuses, such academic eminences as Milton Friedman and Armen Alchian are more often sponsored primarily by student Conservative, Individualist, or Republican Clubs than by economics departments proper. At the same time that they are suspected of suppressing campus Marxism and Marxian economics, conservatives complain of their own exclusion from contact with economics students, at least outside of business schools. One conservative writer, Ayn Rand, has, on grounds of its altruist and collectivist bias, given up hope for the present generation of intellectuals, economists included, and called for replacement by an entirely new group, trained largely outside the present hierarchy and uncontaminated by it [7, pp. 10-57].

#### IV

So the marginal status of Marxism in U.S. economics proves little if anything. It proves neither the inferiority of Marxism in free ideological competition, nor yet the falsity of U.S. claims to academic freedom. Elements of both explanations are initially plausible, but neither case is correct to the point of excluding the other, as a hypothesis if not a fact.

In such a muddled and muddy situation as this, it is almost inevitable to end, like T. S. Eliot's world and *Wasteland*, "not with a bang but a whimper." The writer's particular whimper or whimpers are intended to decrease the imperfections of our market place of ideas, in their relation at least to the Marxian heresies.

The first whimper: As U.S. Marxists are currently required to study, to the point of considerable technical familiarity and some risk of conversion, contemporary marginalist price analysis and income and employment theory, we might well require of all aspiring economic theorists a considerable degree of familiarity with some important dissident positions, including the Marxian one. There is no longer any excuse for repeating as *Marx-Kritik* all the miscomprehensions of Böhm-Bawerk's famous review of the 1890's [2], which treats Marxism exclusively as static price theory, and gives it failing grades.

The second whimper: More U.S. universities, particularly those which foreign students attend in large numbers, should follow the Stanford practice when Baran was hired shortly after World War II. They should bend over backwards to include competent Marxists in their economics faculties, as tokens of the viability of the U.S. liberal tradition and in contrast to the situation prevailing in Mainland China or the Soviet Union. At first, perhaps, trouble would be minimized if the men selected were neither closed-mind Party fanatics nor particularly charismatic types, and if they were kept out of the "thundering-herd" lecture sections of the principal undergraduate courses. Eventually, if all goes well, even these barriers might be lowered for Marxists to the same extent as for others.

Some such tendencies may develop automatically, if the teacher shortage

continues. The ban on the linguistically handicapped teacher, never severe in the United States, has almost vanished, especially where mathematics is a workable substitute for English. The bans against minority races and religions, and against neurotics, "deviants," and females are falling and failing too. Rather than conclusive "three-strike" handicaps, they are down to the "one-strike" level in most institutions. Defensible prejudices against the intellectually handicapped are yielding likewise. What was unpardonable stupidity and incompetence in depressed periods is now called "practical," "realistic," and even "good undergraduate teaching." In this environment, is it too much to anticipate some similar improvements for the ideologically handicapped? As for Marxists like Baran: In the first place, they may be more right than the rest of us. In the second place, even if they are not, might not lowering their handicap from three strikes to one pay for itself in refurbishing our overseas image? Or would it be too little and too late?

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#### The Neoclassical Theorem

The object of this paper is to present a diagrammatic proof of the neoclassical theorem that, in balanced growth, consumption per man is maximized when the marginal product of capital is equal to the natural rate of growth.<sup>1</sup>

<sup>1</sup>We are assuming a one-commodity world so that no index-number problem arises in defining the marginal product of the capital. The incremental output of consumer goods is of the same stuff as the stock of capital, and the marginal product of capital is a pure number with the dimensions of the rate of interest.

Our proof has a number of advantages. Firstly, it makes use of the production function and so focuses on the purely technological nature of the theorem. Secondly, it is perfectly general since it can incorporate both the rate of population growth and Harrodian neutral innovations. Thirdly, it facilitates the ease with which the theorem is grasped since our proof involves a simple extension of the familiar diagrammatic technique developed by Solow to show convergence to balanced growth [2]. Finally, a neat economy of exposition is achieved since the diagram can be used to illustrate both convergence to balanced growth and the neoclassical theorem.

Consider the diagrammatic representation of Solow's fundamental differential equation  $\dot{r} = sF(r, 1) - nr$ .<sup>2</sup> Along the  $x$ -axis, we measure the capital-labor ratio; along the  $y$ -axis, output per man. The ray through the origin, with slope  $n$ , represents the function  $nr$ . Draw the function  $F(r, 1)$  which shows output per worker as a function of capital per worker—following Solow, it is drawn to pass through the origin and is convex upwards—i.e., no output unless both inputs are positive, and diminishing marginal productivity of capital. The function  $sF(r, 1)$  lies below the total product curve by the factor of proportionality  $s$ . It shows total saving per man (output per man multiplied by the saving ratio).

All potential equilibrium points for alternative saving ratios at which the economy clicks, at time zero, into balanced growth with a constant capital to labor ratio are defined by the intersection of  $sF(r, 1)$  and  $nr$  and so must lie along the ray  $nr$ . The distance between the ray  $nr$  and the  $x$ -axis then defines all potential total saving per man for all potential saving ratios; in turn, the distance between the ray  $nr$  and the total product curve  $F(r, 1)$  defines all possible levels of consumption per man. In balanced growth, capital, labor, output, and consumption grow at the same constant exponential rate; when consumption per man is maximized at time zero, it is maximized as well into all perpetuity. Theorem: to maximize consumption per man into perpetuity along a balanced growth path, choose that capital-labor ratio (point  $C$  in Figure 1) at which the tangent drawn to the total product curve is parallel to the ray  $nr$ ; i.e., at which the marginal product of capital is equal to the rate of population growth (the slope of the ray  $nr$ ). If we assume competitive imputation allows the marginal product of capital to be identified as the real rate of interest, then at point  $C$ , the ratio of savings to income,  $BC/AC$ , is equal to the ratio of profits to income  $(RS/AC)$ .<sup>3</sup> In no case, however,

<sup>2</sup> In this equation  $r$  is the ratio of capital to labor  $K/L$ , and  $\dot{r}/r = \dot{K}/K - \dot{L}/L$ . Let  $\dot{L}/L = n$  and  $\bar{K} = sF(K, L)$  where  $s$  is the savings ratio. Then  $\dot{r} = rsF(K, L)/K - nr$  and by the property of homogeneous functions, this can be written  $\dot{r} = sF(r, 1) - nr$ . Cf. Solow [2, p. 69].

<sup>3</sup> Along any balanced growth path, the real rate of interest  $RS/OC$  stands in relation to the natural rate of growth  $BC/OC$  in the same ratio as does the relative share of capital  $RS/AC$  to the savings ratio  $BC/AC$ . The former ratio  $RS/BC$  is the present discounted value of the returns to investment when the discount factor is taken to be the natural rate of growth (the rate of growth of labor in efficiency units). James Tobin has recently advocated the natural rate as the appropriate social rate of discount free from the taint of time preference in the sense that an indifference curve relating this year's consumption to next year's consumption per unit of efficiency labor is symmetrical around a 45 degree line through the origin [1] [3]. For Tobin, the flow of innova-

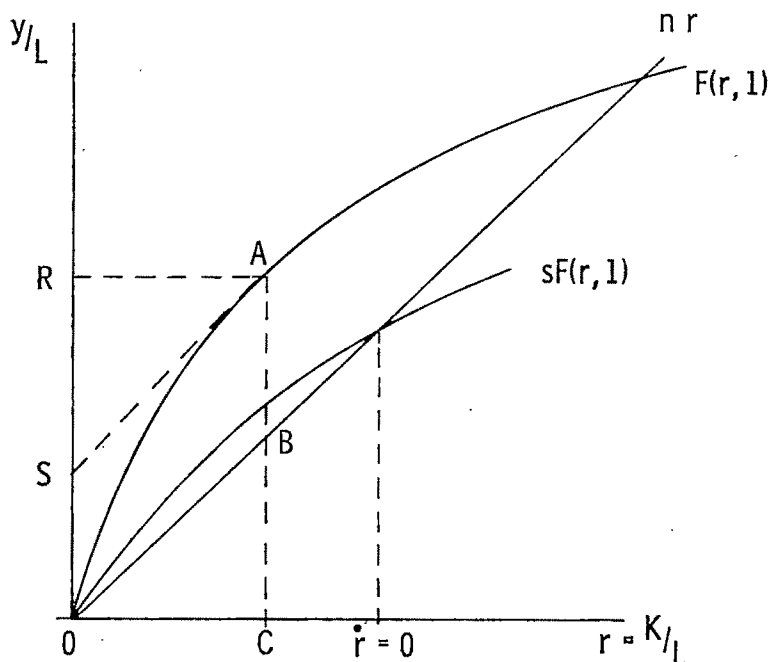


FIGURE 1

should the theorem be made to depend on capitalists' saving all their profits and workers' consuming all their wages. The theorem is best regarded as defining the counterpart for a growing economy of classical capital satiety. In the classical case, in the absence of population growth and technological change, capital satiety takes place when capital deepening along a given production function reduces the marginal net product of capital to zero. Similarly, when the natural rate of growth is positive, capital satiation takes place when the economy clicks into balanced growth with a capital-labor ratio which reduces the marginal net product of capital to equality with the natural rate of growth. To push capital deepening further is to purchase an increment to output per man at the expense of a still greater increment to savings per man and a permanent reduction in consumption per man. This can unequivocally be ruled out as wasteful. It is, given our assumptions, the logical counterpart in a growing economy of pushing the accumulation of capital in the classical stationary state to the point where the marginal net product of capital is negative.

Up to this point, the analysis has dealt solely with population growth. Our diagram is capable, however, of handling the case of Harroldian neutral

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tions and the growth of population are independent of social policy and so determine a unique natural rate. If these variables are themselves influenced by social policy, no unique natural rate exists which can be taken as the rate of discount.

innovations which leave relative shares unchanged. The contribution of Harrodian neutral innovations to output is measured by the growth of output which occurs at a constant capital-output ratio and a constant labor force. Under constant returns to scale, if output and capital both grow at  $z$  per cent, neutral technological change operates as though the labor force of given efficiency were growing at  $z$  per cent; or that a given labor force is  $z$  per cent more efficient so that labor, measured in efficiency units, is growing at  $z$  per cent.<sup>4</sup> For purposes of this analysis, all that is needed to take account of innovations is to measure labor in efficiency units. The natural rate of growth is then the sum of the increase of labor in natural units plus the increase in output per man. This total defines labor's growth in efficiency units. In balanced growth, the ratio of capital to labor measured in efficiency units is constant, while the capital, output, and consumption are increasing at  $z$  per cent per unit of labor measured in natural units. The problem is exactly the same as before: to maximize consumption per unit of efficiency labor, the ratio of capital per unit of efficiency labor must be such as to reduce the marginal net product of capital to the natural rate of growth—i.e., to the growth of labor measured in efficiency units.

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<sup>4</sup>When innovations are Harrodian neutral, the production function at time  $t$ ,  $F_t(K, L)$  can be written  $F_0(K, H(t)E)$  where  $H(t) > 1$  so that each worker at time  $t$  is equivalent to  $H(t)$  workers or, alternatively, labor of given quality can be thought of as growing at  $H(t) - 1 = z$  per cent.

The analysis could in the special case of a Cobb-Douglas function be extended to innovations which raise the marginal products of all factors in equal proportions for all possible input combinations—i.e., Hicksian neutral innovations. For, in this special case, the Harrodian and the Hicksian definitions of innovations are equivalent and the analysis can proceed as before. However, recent empirical work supports the conclusion that the elasticity of substitution is less than unity and it does not seem worth while to incorporate this special case into the analysis.

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### Equity Yields, Growth, and the Structure of Share Prices: Comment

In an interesting article in this *Review* [2], Burton G. Malkiel devotes most of his paper to three subjects: the construction of a model of stock price determination which takes into account expected growth in earnings and dividends; second, the use of this model to estimate the market discount rate on a

"representative standard share" in the stock market; and third, on the basis of certain assumptions, the further application of the model to explain deviations in the prices of growth shares from the general level of stock prices (or, more precisely, deviations in the respective price-earnings ratios). While the article gives many valuable insights into the problems involved, it should be pointed out that the basic model presented by Malkiel had been developed before and that the principal assumptions in his two main applications seem questionable.

Malkiel shows that, if dividends, earnings, and prices grow at the same percentage rate in perpetuity, the market rate of discount (or the rate of return required by investors in stock) is equal to  $r = D_0/P_0 + g$  where  $D_0/P_0$  represents the current dividend-price ratio and  $g$  the growth rate. An equivalent result was obtained much earlier by Gordon and Shapiro [1], who showed under the same assumptions that if dividends were paid and discounted continuously at the annual rates  $D_t$  and  $r$ , the market rate of profit ( $r$ ) is equal to  $D_0/P_0 + g$ . Since dividends are normally paid quarterly and growth rates are typically measured (by Malkiel as well as others) annually, the "true"  $r$  for estimation purposes lies in between the two formulas, but the difference in any case is trivial. Subsequent to the Gordon-Shapiro article, I presented an identical formula to that developed by Malkiel and showed that if the terminal price-earnings or price-dividend ratio is assumed equal to the initial ratio, the above formula holds for any finite time period.<sup>1</sup> I stated that I believed that this assumption represented a good first approximation for most companies for most periods of time so long as one does not take too short a period.

The only significant difference I can find between Malkiel and his predecessors in the development of the above formula is that he seems to assume that certainty (in knowledge of future dividends and prices) is required for its validity. This assumption does not appear to be necessary, and it is not clear why it is made. While it can be argued of course that under uncertainty the warranted price of a share cannot be determined from the expected future flow of return without further knowledge of the dispersion around expected return and the effect of such dispersion on the market discount rate, market price is known in Malkiel's model which he uses to estimate the market discount rate of a standard share. It would seem appropriate (or at least preferable to the available alternatives) to use the model to determine the implicit market discount rate associated with the mean values of expected future returns. This rate would reflect investors' collective reaction to the uncertainty attached to such future returns. Presumably, there is little interest in using stock market data to compute market discount rates under certainty unless the untenable assumption is made that the interest rate on a perpetuity is better obtained from the stock market than from the bond market. Even if such an assumption is made—and there is no indication that Malkiel is so doing—the market discount rate he derives for a representative standard

<sup>1</sup> See my Technical Appendix in [3].

share would still have to be adjusted somehow for the difference in risk between a very-long-term bond and the representative stock.

Turning next to his use of the model to derive this market discount rate for a standard share, he states "It will be seen that a given standard earnings multiple uniquely determines this rate" [2, p. 1010]. This thesis—which is repeated throughout his analysis, is basic to many of his conclusions, and might be considered the main point of departure in this article from previous work—is in my opinion incorrect, though it follows from his assumptions. Malkiel's "demonstration" that this discount rate "uniquely can be determined once the standard earnings multiple is known" [2, pp. 1011-12] simply consists of noting that, once constant payout is assumed,  $D/P$  is known if  $P/E$  is known, and that  $g_s$  is "taken to be a constant," where the subscript  $S$  refers to the standard share. The assumption of the constancy of  $g_s$  avoids the problem rather than meets it. It is extremely difficult to measure  $r$  simply because it is extremely difficult to measure  $g$ .

There is no obvious reason to presuppose, as Malkiel apparently does, that fluctuations in  $E/P$  or  $D/P$  for the standard share are matched by fluctuations in  $r$ , which is implied by the assumption of a constant  $g$ . Actually  $E/P$  or  $D/P$  and  $g$  are likely to be strongly inversely correlated not only for individual securities as implied by Malkiel where growth is measured over short periods of time, but it is also true for individual securities over long periods and more important for representative stock over time. Thus for individual securities I have found that the average 1960-62 price-earnings multiples for each of 64 Moody's stocks (included in the income, growth, and utility indexes) were fairly strongly correlated with the annual compound rate of growth in earnings from 1946-48 to 1960-62 ( $R = .82$  for all stocks combined and  $.63$  for utilities alone).<sup>2</sup> This is not necessarily inconsistent with Malkiel's model even though the growth rates used are very long term. However, similarly for the stock market generally, I would suppose that the drastic reduction in the  $E/P$  ratio over the postwar period was associated with a substantial increase in investors'  $g$  expectations. Malkiel apparently believes (though he is not explicit on this) that the drastic reduction in  $E/P$  reflects a similar decline in  $r$ —and this during a period when interest rates on long-term U.S. government bonds nearly doubled. I would expect that a rise in  $g$  or the expected future growth rate was fully as important as any decline in  $r$ . Any significant change in the growth rate of the standard share ( $g_s$ ) would appear to contradict Malkiel's basic assumption.

Now it might be maintained that as a result of his assumption of certainty, which I have questioned earlier,  $g$  can be assumed known and hence a constant. However, not only would such an assumption make the model largely irrelevant to its subsequent (and almost any other) application, but it would still be as meaningless to say that  $(P/E)_s$  determines  $r$  as to say  $g_s$  determines  $r$ .

Malkiel's subsequent use of his assumption of a constant  $g_s$  to draw cer-

<sup>2</sup> See [3] for the basic data.

tain conclusions about the relative stock market behavior of growth vs. nongrowth issues might also be questioned: viz., "But a falling level of share prices also implies that the dividend yield on standard shares tends to rise. Therefore, the discount rate used to convert the future value of the growth stock to present worth increases" [2, p. 1016]. The second sentence quoted no longer follows if  $g_s$  is not a constant, and a similar statement may be made about the conclusion: "Hence, *ceteris paribus*, the price-earning ratios of nondividend-paying growth stocks may be expected to fall by a greater percentage rate than those of ordinary shares." The verbal demonstration as well as the mathematical proof [2, p. 1029] assumes that the standard  $P/E$  multiple is independent of  $g_s$ . The conclusion that growth stocks are inherently more volatile than standard issues is probably valid but not for the reason given. The  $P/E$  ratio of the growth stock is probably more volatile than the  $(P/E)_s$  of the standard share because  $g$  is more volatile than  $g_s$ , and it is also possible that  $r$  is more volatile than  $r_s$ —if growth stock is regarded as more risky than standard issues and if the risk element in the market is more volatile than the pure interest rate.

In his actual derivation of a specific market discount rate for a representative standard share (which rate he applies subsequently to value growth shares as well), Malkiel assumes that for the stock market generally investors believe that the terminal  $(P/E)_s$  ratio is likely to be the same as the current ratio, so that only  $g_s$  has to be estimated to obtain  $r_s$ . For this purpose, he assumes an annual growth rate of 2 per cent in recent years for the Standard and Poor's Industrial Stock Average which is taken to represent the standard share. It seems doubtful that so low a growth rate could be justified on the basis either of investors' expectations during this period or of past growth. The basic rationalization given for this low growth rate is the growth in earnings for the most recent five-year period available (1957-62) and the result implied by Cowles Commission data from just after the Civil War to the middle of the 1930's [2, pp. 1018-19].

It is difficult, however, to find any brokerage literature which indicated so low a growth expectation during recent years. Moreover, if reliance is placed on the more objective statistics, the Cowles Commission data spliced to the comparable Standard and Poor industrial indexes show annual increases of 2.4 per cent in earnings, 2.7 per cent in dividends, and 3.7 per cent in stock prices from 1872-74, just after the Civil War, to 1961-63, centering the beginning and end of year values to eliminate some of the short-run movement in prices;<sup>3</sup> and increases of 4.4 per cent in earnings, 3.9 per cent in dividends, and 5.1 per cent in stock prices from 1895-97, just before the Spanish-American War, to 1961-63. The recently published data of the University of Chicago Center for Research in Security Prices show a 9.1 per cent average annual rate of return on a portfolio of New York Stock Exchange stocks bought in 1926 and held through 1960, which is generally

<sup>3</sup> It might also be pointed out that the low growth rates in earnings and dividends in the early part of this period were associated with very high earnings-price and dividend-price ratios, which again suggests an inverse correlation between  $g_s$  and  $(D/P)_s$ , or a positive correlation between  $g_s$  and  $(P/E)_s$ , instead of the independence Malkiel assumes.

consistent with (though somewhat lower than) the Standard and Poor Industrial Stock Indexes over this period showing stock prices up by somewhat over 5 per cent per annum and earnings and dividends by 4 per cent. The growth experience since the mid-20's therefore seems quite consistent with that since the latter part of the nineteenth century. The experience of reported earnings over the five years through 1962 probably understates the true rate of growth in earnings as a result of the changed depreciation procedures and related devices for reducing book profits in recent years and is substantially less than the growth rate achieved in 1963. Malkiel may have had in mind some adjustment for the effects of general inflationary pressures—i.e., increases in the prices of goods and services—on the growth rates in earnings, dividends, and stock prices, but he does not mention this problem, and the figures he quotes do not seem to be adjusted in this fashion.

Finally, Malkiel applies his market discount rate for a representative standard share, derived in the manner indicated, to determine the warranted price-earnings multiple for growth stocks at two recent points of time by assuming that after certain specified periods of above-average growth the recent  $P/E$  ratio of the growth stocks will revert to the recent  $P/E$  ratio of the standard stocks (and the growth rates of the two types of stock, it is assumed, will similarly coincide). The above-average growth rates assumed for specified periods are generally those reasonably consistent with recent performance, and the periods to which above-average growth rates are applicable are generally taken to be quite short, though the implications of other assumptions are also spelled out. While all the assumptions which are required for Malkiel's earlier analysis, and which I have criticized above, apply here as well, it should be noted that one additional assumption of some importance is introduced in this last part of his paper which might also be questioned, viz., that the market discount rate for growth stocks is on the average the same as for standard issues.

More precisely, Malkiel states that "there should be no clear risk premium difference between growth and ordinary shares . . . for two securities which are equally risky in all other respects" [2, p. 1019]. While this may or may not be true if above-average growth is being projected a year or two in advance, I doubt that it is true on the average or in any operational sense when substantially above-average growth is projected for much longer periods (though it should be noted he apparently considers three to five years the maximum number of years permissible for projecting above-average earnings).<sup>4</sup> It might be possible to make this statement true in some nonopera-

<sup>4</sup> Actually, if *any* above-average growth rate  $g > g_s$  is projected in perpetuity, which might be regarded as consistent with substantially above-average growth for any appreciable period, then Malkiel's basic condition for a growth stock, viz. that

$$\frac{D_0}{P_0} + g > \left( \frac{D_0}{P_0} \right)_s + g_s,$$

would imply that  $r > r_s$ , indicating a positive risk premium difference between growth and other shares. This, of course, does not imply that  $g > r$  in perpetuity, a possibility Malkiel rejects for obvious reasons. The result  $r > r_s$  could not occur under conditions of certainty, but neither could  $r_s > i$  where  $i$  is the pure interest rate.

tional sense if "equally risky in all other respects" is suitably defined. The case Malkiel presents to illustrate his point, involving "the unreliability of the dividend payment of so august a security as United States Steel" as contrasted with "the much less risky . . . extraordinary rate of growth by International Business Machines Corporation," does not seem to be too relevant to a consideration of two securities "which are equally risky in all other respects," including for example vulnerability to cyclical declines in earnings and dividends. In the recent 1961-62 stock market decline, U.S. Steel was one of the relatively few "august" nongrowth stocks which declined proportionately more in price than such growth stocks as IBM.

In his determination of the warranted price-earnings multiple for growth stocks in recent years, Malkiel again introduces the requirement of certainty for the validity of his analysis: "We find that to justify the market price for these issues at the market peak (in 1961), an investor would have to be certain of extraordinary rates of growth (typically larger than had ever been achieved in the past) for a very long period of years" [2, p. 1022]. This seems to be incorrect since, as he has noted earlier, "Our empirical internal rate of return is not a 'pure' yield, for it includes an allowance for the risk inherent in holding a standard industrial share" [2, p. 1019].

His further inference from this analysis—that an excessive number of years of extraordinary growth would be needed to justify the 1961 peak prices—also seems to me to be questionable on the basis of the data presented. The years of growth he finds needed at the rates experienced over the past five years by these growth stocks ranged from four to eight and averaged somewhat over five [2, p. 1023]. The main rationale for his belief that these are excessive, apart from the implications of compound arithmetic, is that "ordinarily five years should be considered the absolute maximum for the investment horizon" and that it represents the maximum horizon used "by financial analysts in attempting an analytical forecast of earnings." At least for utilities, I have seen 10- to 20-year forecasts. With only moderately higher than average growth, lengthening the time horizon in this manner has a major impact on warranted price or implied market rate of discount. Thus a well-known 20-year forecast of a 5 per cent annual growth in earnings per share for the electric utilities made in 1960, with an initial  $P/E$  ratio of 18, implies a discount rate slightly over 8 per cent if the growth rate reverted to even 3 per cent at the end of the period. This is only a moderate reduction from the 8.8 per cent discount rate implied by a 5 per cent growth in earnings in perpetuity (with the same dividend payout ratio of somewhat below 70 per cent in both cases).

I find it difficult to believe even for nonutilities that the analyst is (or should be) only concerned with an "absolute maximum" of the next five years, in spite of the fact that his estimates beyond that point admittedly have a substantially larger margin of error than his near-term estimates. On the other hand, though I am dubious about Malkiel's reasoning on this point, his conclusion implying unduly high prices of growth stocks at the 1961 peak is of course consistent with the subsequent course of events. My own

interpretation is that growth-stock investors in late 1961 were effectively projecting higher than average growth rates for much longer periods than implied in Malkiel's application of his model, but that the market discount rates for standard as well as growth stock were significantly higher than those he derived (for reasons which I have presented earlier). The growth-rate expectations for the particular stocks Malkiel selected almost certainly declined subsequently and may not have fully recovered by the time of the following bull market (1963-64). However, it is not possible to tell what happened on balance to market discount rates from the preceding boom to 1963-64, though they probably first rose and then declined.

IRWIN FRIEND\*

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### Equity Yields, Growth, and the Structure of Share Prices: Comment

The article on this subject by Burton G. Malkiel in the December, 1963 issue of this *Review* [2] presents a mathematical model for estimating present and future values of "growth" and other common stocks that constitutes a step forward, despite its serious flaws. The model may have some predictive value, but it does not provide a reliable basis for evaluating current levels of share prices as claimed by Malkiel. Especially interesting is his treatment of growth as a factor separate from the earnings rate; his use of an "apparent marginal efficiency of investment" concept; and the manner in which he utilizes a common-stock index to develop "normalized" values for the price-earnings multiplier, the earnings growth rate, and the marginal efficiency of investment.

Comparison of individual multipliers and growth rates against those of a common-stock average taken as a norm is a well-known method which is relied on, for example, in the latest edition of the widely quoted work, *Security Analysis* [1, Chs. 30-31, 37-39].<sup>1</sup> However, the authors use the

<sup>1</sup> The authors find from past data that the appropriate basic multiplier for both the Dow-Jones Industrial Average and Standard and Poor's Composite Index is 15 for current earnings, and 13 for future earnings (average for the next seven years). Thus the DJIA was estimated at 572 for early 1962, or 13 times estimated average earnings of \$44 for 1962-68. This compares with a Central Value estimate of 534. Actual market value of the DJIA in January, 1962 was 705, far above the "intrinsic" value, implying a greatly overvalued market at the time. Curiously enough, Malkiel's critical comments about this book have reference to the older 1951 edition.

older, nonmathematical approach, and in part at least the purpose is different, namely, a search for "intrinsic" value by deriving a fixed, true price-earnings multiplier from historical data; or by determining a "Central Value" through the use of an arbitrary multiple ( $1\frac{1}{2}$  times) of the "basic interest rate."

Malkiel does not fall into the trap of assuming that there is some underlying, true price-earnings multiplier and related capitalization rate that apply to a stock average at all times.<sup>2</sup> He recognizes that the investing public changes the discount rate it places on future earnings from time to time, and that this will alter the multiplier. He does err, however, in assuming that in a given period "warranted" normalized prices can be established for growth stocks to serve as a standard for gauging over- and undervaluation of the latter. This implies incorrectly that the market rather typically miscalculates the values of growth stocks, presumably mainly because earnings plus growth are discounted too many years ahead. A sounder position, it seems to me, is that the market *typically* reflects with reasonable accuracy the warranted values of both growth and nongrowth shares in terms of their earnings prospects. Prices of common stocks rest on discounted future earnings. The highly organized, competitive stock market, with its strong nucleus of informed buyers and sellers, performs this difficult function of price determination very well for most stocks. Errors in assessing the earnings prospects of particular stocks are by no means unknown, and future developments are at times unforeseen or their importance is misjudged. Yet the market's assessment of future prospects is usually close to the mark, as is attested to by its reliability as a lead indicator of cyclical declines. In the case of growth stocks, with a seemingly assured rapid expansion of earnings in the years immediately ahead, this expectation is justifiably reflected in current prices based on higher than average price-earnings multipliers.

### I. *Malkiel's Model*

Let us now examine Malkiel's model, in particular certain questionable assumptions and concepts underlying it. Then it will be appropriate to propose an alternative model that I consider to rest on sounder theoretical ground and to work more satisfactorily.

1. It is assumed, following the position taken by J. B. Williams [3, p. 55], that the present value of a share of stock is equal to the sum of its discounted future dividends. This emphasis on dividends runs counter to the growing reliance of the investing public on earnings rather than dividends in appraising common-stock values and prospects. Earnings are rightly recognized by informed investors as being more fundamental because they represent not only current dividend-paying power, but also, through investment of undistributed profits, a means for future growth of earnings

<sup>2</sup> A leading advisory service, Value Line, seems to have fallen into this trap. The service expects a catastrophic decline in the "flagrantly overvalued" stock market in the not so distant future. Price-earnings multiples are expected to revert to historic levels well below those prevailing in the past decade, and the dividend yields on major stock averages are expected to rise above the average yield on bonds [5, p. 400].

and dividends. As we shall see later, empirical data support the position that share prices are based on future earnings, not dividends.

Because of this reliance on dividends in Malkiel's model, he finds it necessary to treat nondividend-paying growth stocks as a special case, and to devise a modified model to deal with them. This is unnecessary if dividends are ignored and reported earnings are used as the basis of value for *all* common stocks, as is shown below.

2. In the model,  $r$  is defined as the apparent marginal efficiency of investment of the "representative share," equal to  $d_s + g_s$ . Here the representative share is Standard and Poor's 425 Industrial Stock Average;  $d_s$  is the ratio of the dividend at the close of the current year to the existing price of the standard representative share; and  $g_s$  is the rate of growth of earnings. Conceptually, it would seem that  $r = e_s + g_s$  would be more appropriate, as marginal efficiency should have reference to that rate of discount which equates the sum of future earnings (not dividends) with present value. There is also the objection that  $r = d_s + g_s$  assumes that the growth rates for earnings and dividends are identical. This is not supported by the facts, even for stock averages, and is particularly wide of the mark in the case of individual stocks.

Malkiel argues that  $r$  can be uniquely determined, once  $m_s$ , the standard price-earnings multiplier, is known. Since

$$(1) \quad d_s = \frac{D_s(1 + g_s)}{P_s},$$

and  $P_s = m_s E_s$ , by substitution

$$(2) \quad d_s = \frac{D_s(1 + g_s)}{m_s E_s}, \quad \text{or}$$

$$(3) \quad m_s = \frac{D_s(1 + g_s)}{E_s} \bigg/ d_s.$$

That is,  $m_s$  is equal to the payout ratio

$$(4) \quad \frac{D_s(1 + g_s)}{E_s}$$

(assumed to be constant) divided by the dividend rate based on next year's dividend. Alternatively,  $m_s$  may be viewed as determining  $r$ . With a fixed payout ratio, if  $m_s$  is known, and  $r = d_s + g_s$ ,  $r$  is also given, once  $d_s$  is determined.

This cumbersome procedure for determining  $m_s$  and  $r$  evidently seemed necessary to Malkiel because he assumed that dividends are the basis of share values and therefore equated  $r$  with  $d_s + g_s$ . Actually,  $r$  usually can be obtained directly by a simple method that avoids making inaccurate assumptions of a constant dividend payout ratio and identical growth rates for  $E_s$  and  $D_s$ . The method is to calculate the rate of discount which makes

the current value of a share (representative or other share) equal to its value in some earlier year. The formula here is

$$(5) \quad P_s = \frac{P_n}{(1+r)^n}, \quad \text{or}$$

$$(6) \quad (1+r)^n = \frac{P_n}{P_s},$$

where  $P_s$  is the share's value in the earlier period (treated as present value), and  $P_n$  is the accumulated value to the current period. The equation may be easily solved for  $r$ , the marginal efficiency of investment. Then, if we assume that  $r=e+g$ , as suggested earlier, once  $r$  is obtained,  $e$  is also determined, as  $g$  is known. Further, since  $P/E=m$ , and  $E/P=e$ , where  $E$  is next year's earnings,  $m=1/e$ , and  $P=E/e$ .

3. The formulas developed by Malkiel for the derivation of a "normalized"  $P$  and  $m$  for each individual growth stock seem to rest on two misconceptions. The first is that the market tends to overvalue growth stocks, making it necessary to adjust  $P$  and  $m$  downward to more realistic or warranted levels. If, as I deem more plausible, the market typically establishes prices for growth shares that reasonably reflect future prospects, taking account of the uncertainties with which it must deal, there is no justification for "normalized" prices and earnings multiples.

A second misconception is that  $m$ , the average price-earnings multiple in recent years for a representative share, is a significant norm against which the  $m$  of any given growth share should be compared. The misconception here arises in part from a failure to recognize that each individual stock has its own characteristic price-earnings multiple for a given period, against which its current earnings multiple more appropriately should be compared; and partly from a failure to realize that a stock average has no special significance, other than being an average, since *all* stocks, growth and non-growth, are capitalized on the basis of their earnings and growth rates, with due allowance for supposed differences in degree of risk and uncertainty.

4. Malkiel's concept of the structure of equity prices appears to be influenced by the second misconception noted under (3) above. He regards that structure as being such that the normalized price of a nondividend-paying growth stock is some multiple of the price-earnings ratio for the representative standard share. For reasons already indicated, it would seem more plausible to hold that the structure of equity prices rests on differences in earnings and growth rates, after allowance for their regularity and persistence, without regard to representative shares and normalized prices. A stock average is itself dependent on the structure of equity prices prevailing at a given time.

## II. *The Proposed Alternative Model*

Granted that the criticisms made above of Malkiel's model are well taken, what changes are needed to make it more satisfactory? Obviously, several

substantive changes are in order: the use of earnings in place of dividends as a basis for capitalization; the elimination of normalized prices for growth stocks; and a redefinition of  $r$  in terms of earnings and growth. Direct calculation of  $r$  for a substantial number of common stocks convinces me that  $r$  is indeed based on  $e+g$  instead of  $d+g$ .

The proposed alternative model may be summarized as follows:

$$(7) \quad P_n = \frac{E(1+r)^n}{e} = mE(1+r)^n,$$

the future value of a share, where  $E$  is next year's earnings,  $e$  is  $r-g$ ,  $g$  is the average growth rate of earnings per share, and  $r$  is the marginal efficiency of investment.

$$(8) \quad P = \frac{P_n}{(1+r)^n} = \frac{E}{e} = \frac{mE(1+r)^n}{(1+r)^n} \quad \text{or} \quad mE.$$

$$(9) \quad r = e + g,$$

the rate of discount required to equate present and future values of a share of stock.

$$(10) \quad m = \frac{P}{E} = \frac{1}{e},$$

the average price-earnings multiplier for recent years, using current price to next year's earnings.

Formula (8) above differs from Malkiel's

$$(11) \quad P = \bar{m}_s \frac{E(1+g)^n}{(1+\bar{r})^n}$$

for nondividend-paying growth stocks in several important respects. It is applicable to both growth and nongrowth stocks. For compounding earnings  $(1+r)^n$  is used, not  $(1+g)^n$ , since accumulated future value depends on the combined earnings and growth rates. Finally, the  $m$  is used in place of  $\bar{m}_s$  (the  $P-E$  ratio for a representative share), and  $(1+r)^n$  is used in place of  $(1+\bar{r})^n$ . Malkiel's  $\bar{r}$  is the marginal efficiency investment for a representative share, and equals  $d_s+g_s$ , not  $e+g$ .

Now, let us first apply the model to the Dow-Jones Industrial Average (DJIA). Taking  $P_n$  as 767, the Average at the close of 1963, and  $P$  as 454, the Average for the last quarter of 1957,  $r=9.1$  per cent.<sup>3</sup> Owing to the wide fluctuations in the earnings growth rate, and the exceptionally high earnings in 1957, the rate was calculated over a longer period. For 1953-63,  $g=3.9$  per cent, and for 1948-63,  $g=3.6$  per cent. While both figures are close to several recent estimates, the lower one was taken so as to be on the conservative side. [1, pp. 454-56]. Accordingly,  $e=9.1-3.6$  or 5.5 per cent, and  $m=18.2$ .

<sup>3</sup> On a quarterly basis,  $r=8.8$  per cent.

At the close of 1963, the anticipated federal tax cut made it plausible to assume that  $E$  would rise 5 per cent, from \$40 in 1963 to \$42 in 1964.<sup>4</sup> Thus we have

$$(12) \quad P_3 = \frac{42(1.091)^3}{.055} = 992,$$

the estimated DJIA three years hence; and

$$(13) \quad P = \frac{992}{(1.091)^3} = 764,$$

the estimated present value of the DJIA for the close of 1963, compared to the actual  $P$  of 767. In March 1964,  $E$  for the year of 1964 was commonly being estimated at roughly \$44–\$45. Taking  $E$  at \$44, by formula  $P=800$ , which is the same as the actual value of  $P$  in early March.

Our alternative model applies also in cases where earnings show no growth or are actually declining. Obviously where  $g=0$ ,  $e=r$ ; and if  $g < 0$ ,  $e < r$ , as a negative  $g$  is subtracted from  $r$  to obtain  $e$ . In the case of the Dow-Jones Railroad Average (DJRA), earnings have gradually declined. For 1953–63,  $g=-.045$ , and for 1957–63 it is  $-.034$ . Taking  $g=-.04$  as an approximation, and computing  $r$  from the DJRA for the last quarter in 1959 and 1963, we have  $e=.0386-(-.04)=.0786$ , and  $m=1/.0786=12.7$ . Using Value Line's estimate that  $E$  for 1964 will total \$14.20, then  $P=14.20/.0786$ , or 181 for the last quarter of 1963. The actual  $P$  for that period was 179.

Inland Steel illustrates the case of a stock where  $g=0$ . There has been no discernible earnings growth since 1955. Using end-of-year prices for the period 1954–63,  $r=.0723$ . Taking  $g=0$ , we have  $e=r=.0723$ , and  $m=13.8$ .  $E$  is estimated at \$3.25 for 1964. This yields  $P=44$  at the close of 1963, identical with the actual  $P$  at that time.

Application of the model to individual stocks presents greater problems because prices and earnings tend to fluctuate more erratically, and in some instances data are limited to too few years. Where necessary, however, it is possible to derive  $r$  indirectly, by estimating  $e$  from the average or characteristic  $m$  of the stock for recent years. Table 1 presents measures obtained by applying the model to six well-known stocks, American Telephone and Telegraph, General Public Utilities, and four of the growth stocks used by Malkiel in applying his model, namely, International Business Machines, Litton Industries, Perkin Elmer, and Howard Johnson. Estimated prices for the latter three stocks based on Malkiel's model are given for comparative purposes. In applying the model, it was assumed that the market was still below its peak. Hence,  $\bar{m}$ , was taken at 18.2 in place of his 20.3, and  $\bar{d}$ , and  $\bar{r}$  were raised to 3.1 per cent and 5.1 per cent respectively. His estimated growth rates are used, though his growth rate for Perkin Elmer seems much

<sup>4</sup> Up to the close of 1963 the DJIA earnings for that year tended to be underestimated, at about \$40. A subsequent official estimate is \$41.11 [4, p. 55].

too high. The "normalized" prices thus estimated are considerably above actual prices. By contrast, our model yielded an estimated  $P$ , calculated as of the close of 1963 or beginning of 1964, that was exceedingly close to the actual  $P$  in all but one case, as Table 1 shows. Note that there is no evidence of the gross overvaluation that was supposed by some to exist at the time. Actual prices were close to estimated prices calculated on the basis of historical relationships in the recent past.

Malkiel called attention in his article to the volatile nature of growth-stock prices and used estimated peak and trough standard and growth-stock price-earnings multiples to illustrate the extent of fluctuations that take place. I prefer a different method of showing this, in the hope that further

TABLE 1—ESTIMATED AND ACTUAL  $P$  FOR SIX STOCKS AS OF CLOSE OF 1963

Measure	T	IBM	LIT	PKN	HJ	GUY	Malkiel's Model <sup>a</sup>		
							LIT	PKN	HJ
$E$	\$6.30	\$12.66	\$3.16	\$1.95	\$2.75	\$1.80	\$3.16	\$1.95	\$2.75
$e$	.046	.025	.045	.04	.055	.055			
$g^b$	.057	.212	.381	.176	.184	.046	.28	.37	.18
$r^c$	.103	.237	.426	.216	.239	.101			
$m$	21.7	40	22.2	25	18.2	18.2			
$P_1$	184	958	203	98	83	44			
$P$	138	507	70	49	50	33	105	79	72
Actual $P$	139	510	81 <sup>d</sup>	53	50	33	81 <sup>d</sup>	53	50

<sup>a</sup> In applying the model,  $m$  is taken as 18.2,  $d_1$  as 3.1 per cent, and  $\bar{r}$  as 5.1 per cent.

<sup>b</sup> Periods used for calculating  $g$  were as follows: T, IBM and LIT, 1957-63; PKN and HJ 1958-63; and GUY, 1953-63.

<sup>c</sup> For calculating  $r$ , the periods used were: T and GUY, 1953-63; IBM and LIT, 1957-63; PKN, 1958-63; and HJ, derived from  $m$ , 1958-63.

<sup>d</sup> Litton receded to about 70 by mid-February, 1964. The stock was recently split 2 for 1.

light will be cast on the disconcertingly large shifts that at times occur in growth- and even nongrowth-stock prices.

Assume that later this year a cyclical contraction is expected, i.e., is anticipated to take place early in 1965, so that  $E$  is expected to fall to \$41 instead of rising to \$44 by then. If the recession is viewed as minor,  $e$  would probably rise only slightly, with a corresponding decline in  $m$  and a larger drop in  $P$  to the level where  $E/P$  would equal the new  $e$ . For instance, if  $e$  rose for the DJIA to 6 per cent from 5.5 per cent,  $m$  would fall from 18.2 to 16.67, and  $P$  would drop from, say, 800 to 685 or under. An expected sharper contraction might cause  $e$  to rise to, say, 7 per cent, with  $m$  falling to about 14.3 and  $P$  adjusting downward to 41/.07, or approximately to 585, a fall of some 215 points in the Average, or nearly 27 per cent. Assuming no change is expected in the growth rate,  $r$  would rise at least temporarily to 10.6, implying an accelerated rate of increase of  $P$  in the subsequent cyclical expansion.

A larger percentage decline is likely to take place in growth stocks. For example, if IBM stood at 560, and  $E$  was expected to drop from, say, \$14

to around \$12, and  $e$  rose from 2.5 to 3.4 per cent,  $P$  would fall to about 350, some 37.5 per cent. Some growth stocks would of course fall more precipitously than IBM because a sharper drop in earnings is expected or because future growth seems less certain.

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#### Equity Yields, Growth, and the Structure of Share Prices: Reply

In their two comments, Irwin Friend and Roy C. Cave have attacked several assumptions of my valuation model and have questioned both the applications of the model and its principal implication concerning the greater volatility of growth stocks. Before turning to the major points of disagreement between the authors and myself, it would be useful to emphasize the major purpose of my article as well as the questions to which my analysis was *not* addressed. I was not attempting to evaluate, predict, or even suggest the determinants of the level of stock market prices. Nor did I offer, or ever claim to present, a full-uncertainty analysis of share valuation. Moreover, I never considered the empirical section of the paper to be, in any sense, a test of the model—the particular calculations performed were purely illustrative.

It is well known that individual securities may differ in risk and that investors may alter their estimates of earnings growth rates. My purpose was to show that, apart from these factors, it is possible to define a meaningful structure of share prices at any point in time, and that the level and structure can change in the absence of changes in growth rates and risk premiums. Perhaps the most interesting and important results of this analysis concerned the volatility of growth stocks. Abstracting from all the reasons usually adduced to explain the greater volatility of growth stocks, such as those suggested by Friend and Cave, I found an important additional reason to expect such a pattern. Most growth stocks are "longer in duration"<sup>1</sup> than standard issues. The greater volatility of growth stocks is perfectly analogous to

<sup>1</sup> A holder of standard (dividend-paying) shares gets more of his money back sooner than a holder of low- or nondividend-paying growth stocks. Growth stocks are thus longer in "duration" to use Macaulay's [7, pp. 44-53] measure of maturity.

the well-known observation that long-term bond prices are more volatile than short. The intention of my analysis was to uncover just such structural relationships which have yet to be treated in the literature and which are quite interesting in their own right.

### I. Professor Friend's Comment

In view of the general purpose of my article, it would appear that many of Friend's comments which quarrel with my particular illustrative calculations or which claim that, in fact, my *ceteris paribus* assumptions may not hold are, to some extent, beside the point. The more important questions are whether or not Friend has successfully overturned the principal theorems of the model. Consequently, my response will largely be confined to these questions.

#### A. The Volatility of Growth Stocks and the Constancy of the Standard Growth Rates

Friend's major argument is built on the observation that, in fact,  $g_s$  (the growth rate for standard shares) is not likely to be a constant, but rather is a major explanatory factor of the level of share prices ( $m_s$ ). Hence he concludes that my model is inadmissible because it assumes  $g_s$  to be a constant. In particular, he implies that the theorems stating that the values of growth stocks are inherently more volatile than standard issues do not hold because a change in  $m_s$  does not necessarily imply a change in  $r$  (the apparent marginal efficiency of the representative standard share), as I assumed [8, p. 1012]. Although he admits that my volatility conclusions may hold in reality, he believes they hold not for the reasons implied by my model, but rather because the public's estimates of  $g$  (the anticipated growth rate of growth stocks) may themselves be more volatile than are estimates of  $g_s$ .

Friend's assertion that  $g_s$  may vary and hence may be a major determinant of the level of share prices is quite plausible, as I have stated [8, p. 1027]. While it was not my original purpose to deal with this possibility, I agree it would be worth while to incorporate this assumption into the model. First, it should be noted that allowing  $g_s$  to vary does not destroy the applicability of the model in determining a structure of share prices at any time. In fact, if, as Friend suggests, the projected standard-security growth rate ( $g_s$ ) at the December, 1961 market peak was higher than 2 per cent, then my judgment about the overvaluation of growth stocks is supported *a fortiori*, since the growth rate of growth stocks would have had to be correspondingly greater to provide any justification for the high market prices that then prevailed. Had the standard-security growth rate been higher, the standard discount rate would have been higher, and the implicit growth rates for my sample of growth stocks would have been even larger (and/or the growth periods longer) than I had suggested.

More important, all the important theorems of the model still hold. It is not necessary to assume (as Friend does) that estimates of prospective growth rates of growth stocks fluctuate proportionately more than growth rates

of standard issues to conclude that growth stocks will be more volatile than standard issues (although anticipations may indeed follow such a pattern, as I mentioned [8, esp. p. 1027]). It can be shown that these volatility relationships hold even when  $m_s$  varies only in response to equiproportional changes in all growth rates.<sup>2</sup> Thus, even accepting Friend's assumption, the principal conclusions of the model are not upset.

While my results can thus be extended to the case where the level of share prices depends upon anticipated growth rates, there were, nevertheless, good reasons to examine the effects of *ceteris paribus* changes in  $m_s$  that are not associated with any alteration in investors' estimates of  $g_s$ . Indeed, it is pre-

<sup>2</sup> This can be most easily demonstrated for nondividend-paying growth stocks. In this case [see 8, p. 1029] the "warranted" price-earnings multiple ( $m$ ) is given by

$$(1) \quad m = m_s \frac{(1+g)^N}{\left(1+g_s + \frac{D_s(1+g_s)^N}{m_s E_s}\right)^N}$$

where  $g$  stands for the growth rate of earnings per share for the growth stock over the next  $N$  years, and  $D_s$  and  $E_s$  are the dividends and earnings of the standard share for the year just past. In accordance with Friend's assumption we let the standard earnings multiple ( $m_s$ ) be a function of the standard growth rate.

$$(2) \quad m_s = f(g_s), \quad f' > 0.$$

We further assume that investors tend to raise and lower growth estimates for all securities proportionately, i.e., when investors double their estimate of the growth rate for the representative standard share, they double the estimated growth rate for all securities. Note that this demonstration does *not* require that  $g$  be more volatile than  $g_s$ . We then have

$$(3) \quad g = h(g_s),$$

specifically  $g/g_s = K$  where  $K > 1$  by our definition of a growth stock. Accepting Friend's assumption that  $m_s$  changes in response to changes in anticipated growth rates, the new condition for growth stocks to be more volatile than standard shares becomes

$$(4) \quad \frac{1}{m} \cdot \frac{\partial m}{\partial g_s} > \frac{1}{m_s} \cdot \frac{\partial m_s}{\partial g_s}.$$

The percentage change in the price-earnings multiple for a growth stock must be greater than that for a standard security in response to a generalized increase in expected growth rates. Differentiating and evaluating the relevant expressions, we find that (4) holds since

$$(5) \quad \frac{1}{m_s} \cdot \frac{\partial m_s}{\partial g_s} = \frac{f'}{m_s}, \quad \text{while} \quad \frac{1}{m} \cdot \frac{\partial m}{\partial g_s} = \frac{f'}{m_s} + \Delta$$

where  $\Delta > 0$ .

In the case of dividend-paying shares, I was, as before [8, pp. 1016-17], unable to deal with the relevant expression analytically. I was forced instead to examine ranges of representative values of the function for different values of the variables to determine whether my results still hold under Friend's assumptions. These calculations support the conjecture that most dividend-paying growth stocks will still be more volatile than standard issues even when  $m_s$  changes only in response to changes in  $g_s$ . However, as I conjectured earlier, dividend payments lessen the volatility of growth stocks. Moreover, the volatility of dividend-paying shares increases with the growth rate and the growth horizon. I am greatly indebted to R. E. Quandt who programmed these calculations.

cisely because my model abstracts from many real-world phenomena that *ceteris paribus* changes in the standard price level are to be expected. One could list a number of important occurrences in the postwar period which are ignored by the model and which could be expected to affect the standard price level. Perhaps most important, there has undoubtedly been a significant reduction in investors' perceived appraisals of the risks involved in holding common stocks. The mildness of postwar business fluctuations has gradually convinced investors that the disastrous losses incurred by holders of common stocks in the early 1930's are no longer likely. Consequently, the risk premium demanded by investors holding common shares has probably declined significantly in the postwar period. If such risk premiums tended to be variable in the short run, *ceteris paribus* changes (in terms of my model) in the standard price level would be more often the rule than the exception.

Many additional factors, not treated by the model, have affected the postwar level of share prices. Important legal and institutional developments, including the growth of pension and mutual funds and relaxation of the limitations on common-stock investment on the part of life insurance companies, have significantly broadened the market for common shares. At the same time, the ability to avoid taxes on capital gains unrealized until death has probably contributed to a locking-in of a substantial portion of the existing stock of equities.

Finally, a change in the market rate of discount for nonrisk streams will affect the standard price level by changing an important component of the total return. Of course, as Friend has pointed out, the postwar rise in  $m$ , occurred despite a substantial rise in the riskless yield. Nevertheless, he agrees that (for the postwar period) a decline in  $r$  was fully as important as a rise in  $g$ . Our differences on this score are then, at most, matters of degree, and it is certainly admissible and apposite to inquire into the effects of changes in the level of share prices which are not accompanied by changes in  $g$ . Thus it appears that Friend's major criticism of my article has misfired. There is sufficient ground for interest in a model that employs the assumption of *ceteris paribus* changes in the standard price level to which Friend objects. Moreover, the principal conclusions of the model continue to hold even on Friend's own assumptions.

#### B. *Uncertainty and the Horizon Period*

Most of Friend's other comments deal with two (related) matters: the treatment of uncertainty and the horizon period. He questions why the original model was cast in terms of perfect certainty and argues against the application of the standard discount rate to growth stocks without the addition of a premium to account for their greater risk. Such a premium, he claims, can explain the relative volatility of growth stocks because "the risk element in market evaluations is more volatile than the pure interest rate." Finally, he objects to my use of a limited horizon period for projecting extraordinary growth rates.

As mentioned above, my analysis never claimed to offer a general model for the valuation of shares under uncertainty, although some elements of uncertainty were later added to the analysis. It was because my model addressed itself to other questions and because no generally acceptable method for dealing with uncertainty exists that the abstract model was couched in terms of perfect certainty. It is one thing to believe that an extension of the model to cope with uncertainty would leave particular findings untouched, but quite a different matter to build a full-uncertainty valuation model.<sup>3</sup>

When I undertook to apply the model, I decided to "ignore any risk differences resulting solely from the fact that the expected yield from one security may come largely from capital appreciation, whereas the yield from the second is expected to be derived largely from dividend payments." My argument to support that position relied on the dividend-irrelevancy theorem of Modigliani and Miller [9]. I readily admitted, however, that growth stocks may still be riskier than standard shares [8, p. 1019n.]. The matter will never be settled outside a full-uncertainty valuation model and an attendant empirical investigation.<sup>4</sup> Inability to deal fully with uncertainty is the most important lacuna of my model. Nevertheless, as I shall argue presently, my use and alteration of the horizon period can serve as a means for taking account of at least some elements of uncertainty.

In my analysis I utilized the concept of a *limited* horizon period over which extraordinary growth could be projected. The finite-horizon method for dealing with uncertainty has always figured prominently in the literature of investment theory and has attracted a considerable following among practitioners. Economists have pointed out that this technique leaves much to be desired for, among other things, it ignores any prospective receipts after the cut-off date and can be shown in some circumstances to lead to rather absurd rankings of investment projects. But the growth-stock model employed a finite-horizon device which was rather less crude. If investors' horizons are limited as a practical matter to five years, this does not imply that all receipts to the shareholders after the horizon period are ignored. All that is implied is that only normal growth is projected after the cut-off period.<sup>5</sup>

<sup>3</sup> I am guilty, however, of using the word "certain" carelessly in the passage cited by Friend [8, p. 1022]. I meant that an investor would have had to "expect" extraordinary growth rates. I had specifically argued that the portion of the analysis in which the quoted passage appeared did not assume certainty.

<sup>4</sup> I am well aware that my example comparing U.S. Steel and IBM is unconvincing. It was chosen only because a leading investment advisory service had assigned equal risk ratings to the two companies. Both Friend and I have been guilty of frequently resorting to casual empiricism in our references to uncertainty. We will be able to shed light on these more intractable problems of investment analyses only through the route of painstakingly building up the empirical evidence.

<sup>5</sup> Three reasons were adduced for suggesting a five-year horizon for industrial corporations, purely as a practical matter. First, the workings of compound arithmetic render highly implausible the projection of extraordinary growth rates for prolonged periods. Moreover, the typical historical pattern for growth companies has involved only limited periods of extraordinary growth. Finally, I argued that, in practice, financial analysts are unable to forecast for longer periods. The fact that utilities, which are, in effect, guaranteed a satisfactory rate of

But, in fact, I admitted later that no maximum reasonable horizon exists, only a *putatively reasonable horizon*, a variable characteristic of market sentiment at any time [8, p. 1024]. I argued that such horizons underwent rapid changes and that these changes must be accorded an important role in any explanation of the greater volatility of growth stocks. The point of this discussion is not that I claim to have incorporated into the model a generally satisfactory method for coping with uncertainty. At best the finite-horizon method deals with only a part of the problem. The point is rather that this method is, I believe, partially descriptive of the actual valuation process and, most important, that it generates precisely the same type of results as would changes in the discount rate ( $r$ ). Thus, Friend's "alternative" argument, which explains part of the greater volatility of growth stocks in terms of the volatility of the risk discount, is really analogous to my analysis which relied heavily on changes in the *putatively reasonable horizons* used by investors in valuing growth stocks.

### C. *The Nature of the Assumptions of the Valuation Equation*

Both Friend and Cavé have been critical of my growth-stock-valuation equation: Friend impugns its originality, while Cavé attacks its fundamental logic. Both criticisms are wide of the mark and reflect a lack of understanding of the differences in assumptions between my model and previous ones.

Friend suggests that my basic model is not original and cites writings, including his own, in which a similar model was developed before. I had always recognized and stated that the model was built on the work of J. B. Williams [10]. Nevertheless, one of the original aspects of my model is that the earnings multiple for a growth stock reverts back to a standard level after the period of extraordinary growth is completed, and I thus avoid "counting the same trick twice."<sup>6</sup> A fundamental difference between my valuation equation and those that preceded it, including Friend's, is my view that it is improper to assume that the future price-earnings multiple will be the same as today's.

Indeed, it is my use of the "standard" earnings multiple in the valuation equation to which Cavé most strenuously objects. He claims that the price-earnings ratio used for comparison should be the characteristic historical multiple for the individual stock in question. Here Cavé commits what might be called, the "Value Line" fallacy. The popular Value Line investment service relies on the assumption that each stock has its own characteristic earnings multiple against which the current multiple should be compared to detect cases of over- and undervaluation.

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return by the regulating agencies, can often project their earnings for much longer periods, is an exception rather than a refutation of my argument.

<sup>6</sup> Compare this with the following assumption in Friend's model [2, p. 23]: "The one important assumption is that investors expect that the price earnings multiple . . . at the end of the period when they are going to liquidate their investment will be the same as when they invest. This is a very reasonable assumption, certainly a very good first approximation for most companies for most periods of time."

To illustrate the difficulty which besets such an approach, suppose a company is expected to enjoy an interval of extraordinary growth after a period of stable earnings. By using its average past-earnings multiple as an appropriate standard, an investor would be induced to avoid the stock if, as would be likely and consistent with my model, its multiple rose in anticipation of the company's favorable prospects. Now assume the growth period ends after several years and the company's earnings become stable again. My model suggests, *ceteris paribus*, that the price-earnings ratio should revert back to its former (nongrowth) level. But investors who utilized a past average-earnings multiple (including the high multiples which anticipated the extraordinary growth) as a norm, would tend to maintain the higher historical multiple and thereby realize inferior returns. This would be the typical history of such an approach; underestimating the worth of the issue before and during its dynamic growth period and overestimating its worth after the growth period has terminated. Of course Friend's model is subject to precisely the same criticism. It is not applicable to growth stocks since it implicitly embodies the "Value Line" fallacy by continuing to value growth stocks at extraordinarily high price-earnings multiples even after the termination of unusually high growth rates.<sup>7</sup>

## II. Professor Cave's Comment

Cave's fundamental objection to my model is that one shouldn't even try to establish "warranted" normalized prices for growth stocks. He says, "This implies, incorrectly, that the market typically grossly miscalculates the values of growth stocks." Not only is this statement a *non sequitur*, but Cave cannot go on to insist that market prices reflect warranted values unless he has independent evidence of what these warranted values are. I believe [8, p. 1027] that actual market values do oscillate about a structure of prices that does a reasonably good job of equalizing net yields. Nevertheless, I question the plausibility of Cave's view, that at any time "the market typically establishes prices for growth shares that reasonably reflect future prospects."<sup>8</sup> On the contrary, investors characteristically change the prices they are willing to pay for particular growth prospects. For example, during December, 1961, International Business Machines' shares sold at 580. Six months later the same shares sold at 300. Currently IBM shares are again selling at approximately 580, adjusted for recent stock splits. A check with several influential Wall Street security analysts convinces me that throughout this period there

<sup>7</sup> Only for the representative standard share did I use the assumption of a constant price-earnings multiple. In this way I avoided having to predict future market levels. This was why I called the discount rate derived, the *apparent* marginal efficiency. In this case it is true that both Friend [2] and Gordon and Shapiro [4] had previously developed my equation (3) [8, p. 1011].

<sup>8</sup> Cave adduces the stock market's "reliability as a lead indicator of cyclical declines" as proof that the "market's assessment of future prospects is usually close to the mark." A recent study by M. Hatanaka [6, Ch. 12] suggests rather that stock-price indices are relatively poor indicators of actual business activity. A record of 55 per cent success in predicting movements of the economy hardly seems to constitute an impressive record of prediction.

was no change in informed professional opinion as to either the short- or the long-run prospects for IBM. This evidence would call into question Cave's explanation for the volatility of IBM which relies on an expected drop in earnings. My model showed how such price variability can be explained even if earnings prospects are not revised.

#### A. *Dividends, Earnings, and the Valuation of Shares*

Cave goes on to claim that I err by emphasizing dividends as the fundamental source of valuation. On the contrary, I emphasized repeatedly the Miller-Modigliani view which treats dividend policy as a mere detail [8, pp. 1007, 1015]. Cave's misunderstanding of this point leads him to a serious error in his own model. The dividend-irrelevancy theorem states that, given the investment decision, dividend payments to current shareholders merely reduce the value of their claim in the enterprise. This is so because additional equity shares in the business (representing an amount equal to the dividend payment) must be sold in order to finance the predetermined level of capital expenditures. This theorem can readily be demonstrated in the context of a dividend model of share valuation. Indeed, Miller and Modigliani themselves evaluate shares in terms of prospective dividends to obtain this result [9, pp. 418-19]. The theorem does not imply, however, that two firms with identical future growth rates of earnings will sell at identical capitalizations of earnings irrespective of their dividend payments. Yet it appears that Cave has failed to realize this.

The underlying logic of Cave's equation (9),  $r = e + g$ , is never made explicit. Nevertheless, it must rest on the assumption that the present worth of a share is equal to the discounted sum of the stream of future earnings.<sup>9</sup> But such an assumption cannot serve as a rational basis for valuation, as has been pointed out by several writers (e.g., [3, pp. 102-3] and [9, p. 420]). The crucial point it neglects is the cost of the additional capital required to maintain the earnings stream at its specified level. The assumed growth of earnings may require the steady reinvestment of all or part of the earnings stream. If two firms have identical expected earnings streams, but one firm can pay out all of its earnings in dividends, whereas the second must reinvest all its earnings to support the earnings stream, a stockholder would always be better off by buying the dividend-paying shares.

<sup>9</sup> Let

$$P = \sum_{i=1}^N E \left( \frac{1+g}{1+r} \right)^i.$$

Summing the geometric progression and solving for  $r$  we obtain  $r = e + g$ , where

$$e = \frac{E(1+g)}{P},$$

the earnings yield based on the earnings for the following year.

### B. *The Apparent Marginal Efficiency of the Standard Share*

Cave then offers a "superior" alternative method of determining the relevant discount rate for the representative standard share. He simply calculates the discount rate that makes the discounted current price of a share equal to its price during some base period. Why Cave ignores the dividend payments that accrue to the holder (and increase his return) is not explained. Moreover, Cave's measure is far more sensitive than mine to the base period chosen for comparison. Starting from 1957 he gets an  $r$  of 9.1 per cent. Had he started from the close of 1958, his  $r$  would have been close to 2 per cent. The problem is that Cave includes in his measure the capital appreciation received since 1957 which resulted from an increase in the price-earnings multiple for standard shares. The last quarter of 1957 represented the bottom of a sharp decline in market values. During 1958 the market prices (and price-earnings multiples) rose sharply. Of course much the same problem besets my measure of standard growth rates. However, the growth rates represent only a portion of the " $r$ " of my model, and they are much less sensitive to changes in the base date than are market prices themselves.

Friend also utilizes a similar type of argument to suggest that my estimate of the standard growth rate is too low. He cites the University of Chicago Study [1] to suggest that average annual gains of 9 per cent were realized on a portfolio of shares bought in 1926 and held through 1960. In the first place, the 9 per cent return assumes reinvestment of dividends. If dividends are withdrawn, the rate of return falls to 6.9 per cent. While Friend may be right that my illustrative growth estimate is too low, it is worth pointing out that this type of evidence can be quite misleading if applied to certain time periods. The Chicago Study's measure of growth rates does not distinguish between growth that results from increases in the earnings and capital gains that result from a decrease in the capitalization rate applied to those earnings. There is no theoretical or empirical reason for believing that past increases in price-earnings multiples can simply be projected into the future. Indeed, such a practice is particularly dangerous because it can easily lead to a "vicious circle": a higher market price serves to justify itself by increasing the past over-all return. It was precisely this kind of reasoning to which Graham and Dodd [5, Ch. 30] attributed the "new-era philosophy" of the 1920's.

### C. *Cave's Model*

Cave finally constructs an alternative model which embodies his criticisms and, consequently, "rests on sounder theoretical ground." His model turns out to be an identity: the price of a share equals the earnings per share times the price-earnings multiple. The appropriate earnings multiple is derived in one of two ways; Cave either uses the characteristic historical multiple for the stock in question, thus committing the "Value-Line fallacy," or calculates the multiple after finding the over-all returns realized over some period in the past, thus opening the Pandora box of problems I mentioned under

II (B) above. Moreover, his entire approach confuses *ex post* with *ex ante* returns. *Ex post* data may reflect in large part errors in estimating future performance. They do not provide a reliable guide for estimating *ex ante* required returns. It is not true that the investor in the common shares of Litton Industries requires a 42.6 per cent return while the investor in Perkin and Elmer Corporation requires but a 21.6 per cent return, as is implied by Cave's Table 1. Finally, as indicated earlier, Cave's neglect of dividend payments leads him seriously astray.

Cave does not even follow a consistent earnings approach to valuation. For the Dow-Jones average he calculates  $r$  over the period 1957-63, but  $g$  is calculated over the period 1948-63. Similarly, in dealing with individual issues, he arbitrarily picks both the time periods and the method of computation to suit his convenience in arriving at his valuations.<sup>10</sup> This makes the remarkable degree of convergence between his estimates and actual market values less surprising than might appear at first glance. Had Cave followed one consistent valuation method (with consistent time periods) he would have seen that his model fails to produce useful results. Cave therefore provides neither a reliable basis for evaluating general market levels nor a useful method for appraising individual common shares.

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<sup>10</sup> Moreover, his application of my model rests on several arbitrary assumptions. I never argued that past growth rates could simply be projected into the future. I only cautioned that security analysts should be especially wary if the market value of growth stocks already discounts several years of growth at a high historic rate, particularly when the historic growth rate was achieved on a very low earnings base.

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### Capital-Labor Ratios in Theory and in History: Comment

The prolonged debate over factor proportions during economic development took a new and challenging turn in the article by Professors Fei and Ranis in the June, 1963 issue of this *Review* [6], together with a portion of Professor Ranis' paper at the December, 1962 meetings [16]. These authors propound a doctrine of "capital-shallowing" in the early industrialization of retarded countries and present it with great ingenuity on both the theoretical and the empirical levels.

Most other writers find or assume that such development entails increasing capital-intensity in some degree; and they differ chiefly as to the optimal degree and timing. Fei and Ranis, however, deny both the necessity and the desirability of intensifying capital in the early stages of development. They claim to have found that "capital-shallowing" has occurred in at least one historical instance; they commend that path to investment planners; and they infer that the maximum-employment objective and the maximum-output objective can usually be reconciled along that path.

The present comment seeks to revindicate the principle that ordinarily industrialization requires capital-deepening. The argument will be made first by indicating the methodology of the Fei-Ranis model, then by re-examining and recomputing the Japanese data, by reinterpreting the concept of "labor absorption," and by applying qualitative evidence from the history of Japan, India, and Communist China.

#### I. The Model

The Fei-Ranis article in question here is concerned with the expansion of a relatively small, commercialized "industrial sector," alongside a large subsistence-type agricultural sector whose role during development was examined in an earlier article by the same authors [18], both discussions being elaborated from Arthur Lewis' celebrated two-sector model of economic development under conditions of "unlimited supplies of labor" [9]. The Lewis model, however, does not hinge on the degree or trend of capital intensity within the modernizing sector, since it indicates only that, with given capital and given production functions, the capitalist sector will at most draw from the subsistence sector just so much labor as will bring the marginal productivity of the transferred workers into equality with the low and constant

wage,<sup>1</sup> this wage being set at average subsistence earnings plus a differential necessary to attract subsistence workers to capitalist industry. Fei and Ranis have reformulated this model in abstract taxonomic terms which make explicit the possibilities of capital-shallowing as well as capital neutrality and capital-deepening, within the industrial sector during the developmental sequence. But there is no assurance here that actual market conditions, together with available technologies, will permit instances of each of these possibilities to occur. Consequently our authors turn to actual experience and rest their argument on the differential between two cases: namely, Japan as an instance of "successful" development, allegedly marked by capital-shallowing up to 1917, in contrast to India, which during 1949-60 is said to show capital-deepening accompanied by developmental lag.

These data are then used to demonstrate the importance of "innovations" in the process of labor absorption, with the sheer accumulation of capital playing a minor role. This is done by the residual method, whereby a part of the actual increase in industrial employment is attributed in direct proportion to the increase in capital stock (with no allowance for improvements in quality of capital, and on the assumption of constant returns to scale); and the remainder of employment growth is attributed to "innovations" (which the authors analyze conceptually into elements of "labor-bias," "innovational intensity" and "demand elasticity for labor," but which they use in practice as a single catchall extending well beyond technological change). Because of this residual method, the results of the whole model are affected by whatever biases creep into the conceptions and the empirical measurement of growth in employment and in capital.

## II. *The Fei-Ranis Estimates for Japan*

The measurement of capital-intensity and of its trends over time always presents stubborn problems of translating the abstract concepts of capital and labor into the mixed and imperfect terms of empirical records (cf. [2]). It is difficult to obtain long series for capital and for labor that are both consistent within themselves and appropriate to each other. It is still more difficult to take account of indirect inputs of these factors (including the capital and labor embodied in imports) or to adjust for qualitative changes over long time-spans (innovations in tangible capital, improvements in human capital). In the case of Japan, the analyst is tempted to seize upon several new series that have recently been published [11] [14] [19] [23] and use them to construct important aggregative relationships. But it is dangerous to draw strong inferences from such series, as Fei and Ranis do, in face of the limitations of the primary data and the heroic assumptions on which they have been worked up [15] [22].

Fei and Ranis have devised a long sequence to represent the capital/labor ratio ( $K/L$ ) in Japan for the sector they label "industrial"—which is in fact the whole economy minus the sector of agriculture and other "primary" ac-

<sup>1</sup>Of course the accumulation of capital in the growing industrial sector will tend to raise the capital-per-capita ratio of the whole economy—abstracting from population growth—but our concern here is solely within the industrial sector.

tivities. To obtain their annual series on industrial capital ( $K$ ), they very ingeniously combined a stock figure with flow data. Beginning with Ohkawa's version of the official estimate of national wealth in 1930 [14, p. 164], and excluding some items as not being industrial capital, they projected it backwards all the way to 1888 by applying Professor Rosovsky's recent estimates of annual investment in Japan [23, pp. 109-10]. In fact, they successively subtracted from the 1930 stock of industrial wealth Rosovsky's figures for annual gross outlays on private nonresidential construction, private durable equipment, and all public investment, after deflating those annual figures for price changes (using Ohkawa's "non-agricultural price index" [14, p. 130], derived from wholesale prices of staple commodities), and then reducing to net terms by deducting a constant ratio of 20 per cent of the annual gross investment to represent depreciation. These procedures obviously permit considerable distortion of the data and understatement of the record.<sup>2</sup> In addition, the authors retained in their 1930 capital stock figure all of buildings and all of inventories; while the annual investment figures, which they subtracted from the 1930 stock, exclude inventories and also exclude residential buildings. We must also note that all the figures used by Fei and Ranis include most of military goods, agricultural equipment, and perhaps a few other categories which are not properly "industrial."

To obtain their industrial work-force figures, however, Fei and Ranis took an even broader scope for the "industrial" category. They simply subtracted persons in the primary occupations from the total of the gainfully occupied in each year. They utilized the Hijikata estimates as presented by Ohkawa, but made no allowance for Ohkawa's criticisms of those estimates as overstating the primary occupations in the early years and correspondingly understating the remainder of the labor force [14, pp. 146-49]. In addition, they were unable to adjust the labor input for the gradual decline of man-hours per worker [3, pp. 137-38], [7, p. 412], nor conversely for the gradual rise in the quality of workers as industrialization advanced.

The net effect of these lines of treatment for  $K$  and  $L$  is to show an excessively large initial figure for  $K$ , to which the authors then add unsuitably small annual increments; while for  $L$  they have adopted an initial figure which is probably too small, and to which they add huge annual increments—indeed, the whole annual increase of the national labor force (since the numbers in agriculture remained nearly constant). Inevitably, as can be seen in Table 1 of the article in question, the annual percentage increase in  $K$  appears small, and grows more slowly at the beginning of the period than later on, while the annual percentage increase in  $L$  looks large, and so the  $K/L$  ratio appears to decline. More precisely, the ratio shows a decline until 1917, when the growing annual investments as measured by these authors finally became large relative to the indicated capital stock at that time, while the indicated growth ratio of  $L$  kept declining almost steadily from 1888.

<sup>2</sup> No attempt was made to adjust for quality improvements over time, an adjustment which would have raised the calculated rate of capital accumulation and its contribution to growth; cf. Solow [24] versus Denison [4]. Fei and Ranis reserve the quality improvements for their category of "innovations," whose magnitude they obtain only indirectly by the residual method indicated above.

Not only are these statistical series defective, but also they do not really measure "successful absorption" of labor in "the industrial sector." The Fei-Ranis procedure lumps tertiary services together with secondary occupations in both  $K$  and  $L$ . Consequently, their "industrial sector" is a nonfunctional mixed bag of relatively high-productivity activities (modern kinds of manufacturing, transportation, commerce and banking, etc.) mingled with the many low-productivity activities (mostly traditional service occupations such as petty trading, manual transport, manual excavation and construction, handicrafts, domestic help, entertainment, etc.) which in overpopulated countries occupy many individuals who cannot find work yielding higher individual income and net social output.<sup>3</sup> The resulting mixed figures, being at most an average of the diverse subgroups, do not represent "successful absorption" for the whole industrial sector, since Fei and Ranis—still following W. A. Lewis—require that "absorption" take place at "the industrial real wage" and not below it (i.e., not at real social marginal productivity approaching zero). Therefore a  $K/L$  ratio based on workers' mere presence in nonagricultural occupations, regardless of productivity, cannot have clear implications as to the sources of Japanese real growth in aggregative terms, let alone implications as to optimal factor proportions for individual projects or for particular industries. Such figures certainly should not be used for comparison with the capital/labor ratios computed by Fei and Ranis for India, where they deal with an "industrial sector" more narrowly defined (i.e., "large-scale manufacturing and mining and small-scale or cottage industries").

### III. *Alternative Estimates for Japan*

Because of the imperfections in the immediately available data, it is not easy directly to remove the biases which have crept into the Fei-Ranis treatment. One approach is explicitly to revert to whole-economy measures, instead of selecting sectors which, while giving the appearance of homogeneity, often prove to be internally inconsistent or externally ill-matched. Perhaps the simplest and safest measure of this kind was made by Ohkawa, who constructed a series on capital per capita for the whole economy of Japan.

For  $K$  he used eight historical estimates of national wealth—after excluding land, consumers' household goods, bullion holdings, the balance of foreign indebtedness, and a few other categories of "nonproductive wealth," and adjusting a few other categories; and after deflating the estimates by a price index of construction costs for some items, and a general wholesale price index for the other items. For  $L$ , Ohkawa used the total population. The results (Ohkawa's variant " $K_1'/N$ ") [14, p. 170] are shown in Table 1. This series shows a strongly rising trend, not a fall, throughout this period since 1905, except for 1917-19. To be sure, the data here do not go back to the very be-

<sup>3</sup> Cf. [11, pp. 131-32]. This phenomenon of overcrowded occupations in the traditional services, with a social marginal productivity approaching zero, has been observed in many retarded countries; cf. [12, Ch. 1]. It should be possible to check directly on this phenomenon in the Japanese record by examining the trend of incomes per capita in the various sectors. Unfortunately, the best estimates currently available, pages 100-115 of the Ohkawa volume already cited [14], are deficient at this point, since for the early years Ohkawa simply linked the whole tertiary sector with the whole goods sector by way of a fixed ratio—which of course begs the whole question.

TABLE 1—CAPITAL WEALTH PER CAPITA IN WHOLE ECONOMY OF JAPAN  
(yen, 1928–32 values)

1905	346	1919	438
1910	422	1924	685
1913	455	1930	831
1917	385	1935	953

ginnings of Japan's modernization, and they are not free of serious defects, especially as regards the methods of valuing the stocks and the techniques of price deflation, so that the finding of a generally rising trend is not conclusive.

Another showing that the  $K/L$  trend for the whole economy was upward, rather than downward, is obtained by adapting the Fei-Ranis method of flows applied to a stock, using a broader base to improve the consistency and the matching of these series. This is done by taking Rosovsky's annual figures for nearly *all* investment (counting in the residential construction, although still excluding agricultural improvements and inventory growth because the annual data are not available) and applying them (with the same deflation and depreciation allowances) backwards from the terminal (1930) figure on *all* capital stock (the same used by Fei and Ranis for the "industrial sector" alone); while the denominator of this  $K/L$  ratio is the *total* population. The results of this computation are shown in Table 2, at five-year intervals drawn from the complete annual series. Once again we observe a persistently rising

TABLE 2—STOCK OF CAPITAL PER CAPITA  
JAPAN, 1888–1928

	Fei-Ranis Estimates Adjusted <sup>a</sup> (millions of yen, 1928–32 values)	Total Population <sup>b</sup> (thousands of persons)	Ratio <sup>c</sup> (yen per capita)
	(1)	(2)	(3)
1888	18,492.72	39,362	469.8
1893	19,598.36	41,127	476.5
1898	20,925.34	43,275	483.5
1903	22,361.31	45,988	486.2
1908	24,267.96	48,407	501.3
1913	27,222.69	51,856	524.9
1918	30,594.79	54,960	556.7
1923	35,891.34	58,331	615.3
1928	42,559.10	62,122	685.1

<sup>a</sup> Aggregate annual investment figures from Rosovsky, [23, pp. 109–10], reduced from gross to net by 20 per cent allowance for depreciation, and reduced to constant 1928–32 values of the yen by nonagricultural price index from Ohkawa [14, p. 130]; resulting figures applied backwards from 1930 real capital stock, Fei-Ranis version derived from Ohkawa [14, p. 164]. Coverage of investment figures here includes residential and nonresidential construction and durable equipment in both the private and the public sectors; coverage of 1930 capital stock includes these classes plus current inventories.

<sup>b</sup> From Ohkawa [14, pp. 140–41].

<sup>c</sup> Col. (1) divided by col. (2).

trend, which is not appreciably altered by interpolation between the years shown, nor by substituting for  $L$ , in place of total population, the above-cited Hijikata estimates of the total labor force.

If we can accept these findings of a continuous rise in the over-all  $K/L$  ratio, then *a fortiori* we must expect a still stronger rise in the "industrial" sector alone, since it is generally agreed—for Japan as for most underdeveloped countries—that measured agricultural investment per capita tends to lag behind industrial investment per capita.

It would of course be highly desirable to make a direct measure of the  $K/L$  ratio for the industrial sector in the exact or narrow sense if the deficiencies of the present data could somehow be circumvented. One approximation is possible by taking the rate of growth in the stock of durable equipment at years close to the estimates of national wealth used in Table 1 above, and comparing that growth rate to the trend of the industrial labor force; see Table 3. For the former rate of growth, we take Rosovsky's figures for private

TABLE 3—COMPARATIVE GROWTH RATES OF DURABLE EQUIPMENT AND INDUSTRIAL LABOR FORCE OF JAPAN

	Private Gross Investment in Durable Equipment* (millions of current yen) (1)	Private Net Investment in Durable Equipment <sup>b</sup> (millions of current yen) (2)	Stock of Durable Equipment at Beginning of Year <sup>c</sup> (millions of current yen) (3)	Percentage of Annual Investment to Stock <sup>d</sup> (4)	Percentage Rate of Growth of Corre- sponding Labor Force	
					Fei-Ranis "Industrial Sector" <sup>e</sup>	Ohkawa "Second- ary In- dustry" <sup>f</sup>
A. Around 1905						
1905	45.44	36.35	1,056.720	3.43	2.52	1.51
1906	59.56	47.64	1,093.070	4.35	2.64	1.48
1907	66.36	53.09	1,140.710	4.65	2.56	4.04
B. Around 1913						
1913	89.58	71.66	2,073.136	3.45	2.23	2.16
1914	103.64	82.91	2,144.796	3.86	2.22	1.99
1915	107.64	86.11	2,227.706	3.86	2.09	1.62

\* H. Rosovsky [23, p. 109, col. 3]; his "five-year moving totals" for each year divided by 5 to obtain annual figure for each year.

<sup>b</sup> After deduction of 20 per cent of the gross investment, following Fei-Ranis procedure for depreciation [6].

<sup>c</sup> Comprising manufacturing machinery, railways and tramways, vehicles and aircraft, shipping, waterworks, as given in National Wealth estimates for 1905 and for 1913 in K. Ohkawa [14, p. 164], adjusted by addition or subtraction of the annual net investments shown in col. (2) above. For 1913, we have included "Property of Departments of Government," a category which is blank in Ohkawa's tabulation for 1905 and presumably represents reclassification of the railways (which had largely been nationalized, and are shown by Ohkawa for 1913 at less than half their 1905 valuation).

<sup>d</sup> Col. (2) divided by col. (3). Price changes during these sets of years were very minor.

<sup>e</sup> Table 1, col. 2, p. 290 of the article cited [6]; comprises all occupations other than primary.

<sup>f</sup> Computed from annual labor-force data in Ohkawa [14, p. 145, Table 2]; comprises mainly private manufacturing, and excludes construction, transportation, and government factories.

TABLE 4—CAPITAL-LABOR RATIO, SPINNING INDUSTRY OF JAPAN

Interval (annual average)	Capital (spindles per day)	Labor (operatives)	Capital-Labor Ratio
1886-90	128,989	5,997	21.5
1891-95	391,087	29,518	13.4
1896-1900	960,577	57,857	16.6
1901-05	1,296,471	66,840	19.6
1906-10	1,613,390	80,852	20.0
1911-15	2,331,272	109,228	21.3
1916-21	3,618,503	150,568	24.0
1920-24	4,120,918	184,287	22.4
1925-29	5,514,794	204,586	27.0

gross investment in durable equipment in 1905, 1906, and 1907<sup>3</sup> and after deducting the afore-mentioned 20 per cent for depreciation, we apply the figures in then-current prices to the 1905 stock of durable equipment also in then-current prices;<sup>4</sup> and we do a similar operation for 1913, 1914, and 1915. For the rate of growth in the labor force, we use either the Fei-Ranis series in their "industrial sector" (which is somewhat too wide for our durable-goods category) or Ohkawa's series for secondary industry (which is a little too narrow). In every case, as shown in Table 3, we find that the capital stock so defined was growing faster than the labor force. In other words, the  $K/L$  ratio was evidently rising during this pre-1917 period when Fei and Ranis allege it was falling.

Using the same body of data, we can construct average and incremental  $K/L$  ratios for each period. We find that, in every case, the incremental ratio is far above the average ratio, indicating that the average must have been rising.

Still another indicator may be extracted from data provided by Professor Ranis in an earlier article [17]. Table III in that article (p. 601) provides a  $K/L$  series for a single industry, the cotton-spinning industry in Japan, as shown in Table 4. The ratio given in the last column in Table 4 shows a rising trend in every interval except the first and next to last.<sup>5</sup> Thus the evidence for a single important industry agrees with the trend we have found for the whole industrial sector.<sup>6</sup>

<sup>3</sup>In the stock figure we have left government durable equipment (which includes military goods), also many structures which belong in a construction category. Since the annual investment figures have narrower coverage, the effect is to depress the indicated growth rate of  $K$ —which nevertheless shows up relatively high.

<sup>4</sup>Surely the figure of 21.5 for the first  $K/L$  ratio is far out of line and must be due to gross underreporting of the operatives. Especially lacking in the Japanese statistics are direct reports on the workers in the small-scale shops, which have been a prominent feature of industry in that country from the outset of her modernization and gradual mechanization; cf. [1] [11] [20].

<sup>5</sup>A similar trend for the significant early years 1896-1900 may be found in [7, p. 403], showing horsepower rising faster than work force in mechanized factories, while the nonmotorized workshops were declining in number and in workers.

None of these approaches, to be sure, is the precise and final word on the trend of the  $K/L$  ratio in Japan. Ohkawa, Rosovsky, and others, in current research projects both in this country and in Japan, are reworking the data that are now available. But in the meantime the consensus of the measures shown here seems to be a rising trend of the ratio in Japan's evolution right from the beginning, at least as far back as 1888, with no period of declining ratio from 1888 to 1917, as alleged. In broad contour, the path of rising capital-intensity in Japan parallels that which Fei and Ranis present for India.

#### IV. *Qualitative Evidence*

As the data presented above are not altogether conclusive against the doctrine of capital-shallowing, it may be useful to refer to some qualitative features of economic development. There are only a few ways by which the  $K/L$  ratio might be carried downward in the industrial sector during its development: (a) the premodern  $K/L$  ratio might have been very high—whether due to a capital-intensive technology or due to underutilization of plant capacity—and at any rate higher than the marginal  $K/L$  ratio of the newly introduced modern industries; (b) the initial stage of industrialization might consist mainly in installing projects of high capital-intensity (notably “social overhead capital”), while the second stage concentrates on lower  $K/L$  projects and on fuller utilization of the earlier ones; (c) the industries of high labor-intensity might be expanded more than the capital-intensive industries; (d) in the new industries, whatever their initial  $K/L$  ratios may be, labor might be continuously expanded faster than capital, either by substituting  $L$  for  $K$  or by adding more  $L$  to given  $K$ .

None of these possibilities seems to fit very well the historical experience of most underdeveloped countries, bearing in mind that we are dealing with conditions of surplus labor and are concerned with the expansion of wage-employment industries. The Japanese record, as set forth in [1] [7] [11] [13], among others, gives a particularly bad fit. Indeed, possibility (a) above actually presupposes in the premodern economy a relative scarcity of labor, not a surplus. Possibility (b) would require in Japan a rising  $K/L$  ratio at least into the first decade of the twentieth century, to be followed by a falling ratio during the years when Fei and Ranis allege it was rising (after 1917). As for possibility (c), this runs counter to what we know about trends in labor-intensive small-scale industries (in Japan, some of the old handicrafts expanded but many were destroyed, and others survived and grew only when modernized by some application of additional capital, as further explained below). Finally, possibility (d) requires capital-saving, labor-using innovations in technology, since capitalists, however “paternalistic,” will not hire many workers whose marginal productivity falls below the “industrial wage”; but in fact we observe that even in labor-surplus countries the rational capitalist tends to replace “cheap” labor with machines which prove to be still cheaper (to the firm); thus the surplus labor is left to crowd into service occupations where its marginal productivity approaches zero (cf. [10]).

When we turn to the Japanese practices which Fei and Ranis themselves

cite as means of achieving capital-shallowing in that country, we find that they do not mention our possibility (a), and present some rather a-historical instances of (b), (c), and (d). Their statement [6, p. 301] is that nineteenth-century Japan adopted "labor-using innovations" which "included, among others, the natural survival of modernized cottage-style manufacturing as part of a large-scale industrial complex based on subcontracting arrangements; the speed-up of machines and the adoption of multiple shifts; and the widespread substitution of surplus labor in all sorts of activities peripheral to the machine process proper." Unfortunately, these suggestions treat cross-sectional differentials (i.e., among industries in Japan or in comparisons between Japan and Western countries) as equivalent to historical sequences within Japan. Actually, the mentioned practices were either ancient Japanese labor-intensive methods which were slowly displaced by more capital-intensive methods during the course of industrialization or they were factory practices adopted almost concurrently with the introduction of modern machines (e.g. high-speed, multiple-shift working of machines [7, p. 412]). Rarely were they improvements gradually introduced over the years, as Fei and Ranis seem to believe. In particular, many of the celebrated small-scale industries in Japan—which indisputably used more labor relative to capital than did the modern factories either in Japan or abroad—survived and flourished insofar as their traditional methods were modified to incorporate some elements of modern capital (e.g., pedal-driven and power-driven looms and knitting machines in cottage weaving; improved tools in woodworking; general-purpose lathes in metalworking; bicycles in local transportation, etc.); see [1, Chs. 4, 5] [11, Chs. 3, 4] [20]. In general, labor in Japan was not historically "substituted" for capital in industry in the absolute or displacement sense; labor substituted mainly in the relative or opportunity sense of avoiding still higher capital-intensity during modernization. Thus the fact that the rise of capital-intensity was restrained must not be converted into a belief that capital-intensity was actually reduced. "Successful absorption" of labor in Japanese industry generally entailed some degree of capital-deepening. By contrast, it was *unsuccessful* absorption when labor "substituted" for equipment by desperately crowding into certain service occupations where capital and modern technology were hardly introduced at all, and where marginal productivity approached zero.

Curiously enough, one of the few examples of the labor substitution which Fei and Ranis have in mind is actually to be found in India. The Central Government's recent policy of restricting operations in the large-scale textile mills, in favor of handicraft weaving, was intended to enlarge the volume of employment, and accordingly does promote capital-shallowing for the weaving industry as a whole. But it does not promote efficiency and growth, as attested by the unfortunate effects of that policy upon output and upon incentive to modernize [8, Ch. 3].

On the other hand, there is little basis for comparison of India today with Japan around the turn of the century. A great many present-day features, both

domestic and international, are comparatively more adverse to development in India today, and prevent any simple attribution of India's lag to any greater capital-intensity of her industrial projects. Indeed, Indian industry today has already developed beyond many of the devices which in initial stages of Japanese development were used to restrain somewhat the rise of the over-all  $K/L$  ratio.

In recent history, the most distinct and extreme move toward capital-shallowing is to be found in Communist China, in the "mass labor" program, during the 1950's and especially during the "Great Leap Forward" of 1957-58. Millions of laborers, mobilized on the assumption of extensive surplus labor-time, were put to work for little or no material compensation, and with almost no equipment, not even simple tools in many instances [5]. Assigned to manual construction (e.g., earth dams, irrigation ditches, dirt roads), or to primitive steelmaking (the backyard furnaces), or to agriculture (deep-plowing, close-planting, extermination of crop pests), or other fields of work, these laborers showed substantial early achievements, notably in water conservation [21]. Beginning in 1957, however, the regime evidently went overboard into excessive compulsion of labor-time and into the wrong types of assignments. The resulting disorganization, exhaustion, and waste undoubtedly were major factors in the severe setback of the entire Chinese economy after 1958. It would require too much space here to indicate that the experience of Communist China, tied to particular conditions and policies, is not a complete refutation of the whole Nurkse-Lewis theory of the "under-employment potential." But we can recognize at least that the Chinese effort, under a regime wielding totalitarian power, was far from a success in its own terms. These results do not commend the doctrine of capital-shallowing which is in question here.

The foregoing considerations also throw some light on Fei and Ranis' discussion of India as a country which is below her optimum position in regard to both maximizing employment and maximizing output and could therefore advance along both lines simultaneously by means of a lower degree of capital intensity in new industrial projects. This form of advance may well be available for India, as John P. Lewis [8] and others have insisted. But nothing in the experience of Japan demonstrates that the industrial investment pattern can or must be actual "capital-shallowing"; or that, with the given rates of capital formation and population growth, the new facilities soon will "successfully absorb" more than a small fraction of the total labor supply. Unless domestic saving and/or foreign assistance can be raised very high, we are left with the basic and often very painful choice between maximizing output for the sake of economic progress, and maximizing employment for the sake of welfare and political stability in the less developed countries.

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### Capital-Labor Ratios in Theory and in History: Reply

Professor Reubens' comments are directed exclusively to one aspect of our view of development in the dualistic economy in which the rapid reallocation of labor from the subsistence agricultural sector to the commercialized industrial sector represents the essential feature of successful growth. It seems to be his contention that our arguments somehow rest on a "faulty doctrine of capital-shallowing" in the industrial sector of the dualistic economy. We propose to examine this issue from the point of view of the logical necessity of capital-shallowing, its desirability, and its empirical validity in the case of historical Japan.

With respect to the logical necessity of capital-shallowing, as Reubens himself admits, neither the original Lewis model nor ours in any way hinges on the trend of industrial sector capital-intensity over time. Conceptually, capital-shallowing, capital-deepening, or constancy in the capital-labor ratio are all possible and, in fact, compatible with successful development.<sup>1</sup> The question is really one of determining which pattern is more *likely* to lead to successful development and hence more desirable.

In an economy characterized by a scarcity of savings fund and the existence of a large reservoir of surplus labor in the agricultural sector, it follows logically that the more we can "stretch" our industrial capital to accommodate labor productively, the better the economy's chances of escaping the Malthusian trap. The more labor-using industrial production techniques and the industrial output mix, the more workers can be transferred to higher-productivity industrial employment, and the more likely that the country can succeed in achieving economic maturity. The phenomenon of capital-shallowing is merely a manifestation (perhaps an extreme one) of such a successful search for labor-using techniques and product mixes. Although it is by no means clear from his comments, perhaps Reubens also does not dissent from this view of the eminent desirability of capital-shallowing in the course of the industrialization process. Thus his quarrel with us is neither with our theory<sup>2</sup> nor with our policy conclusions; it is with the empirical fact of whether or not a particular case of the adoption of increasingly labor-using technology, i.e. capital-shallowing, has, in fact, occurred in the case of Japan. This then becomes merely a problem of statistical verification—to which most of his comments are indeed directed—and to which we shall now turn our attention.

<sup>1</sup>See, for example, our discussion in relation to Diagram 6 [2, p. 299] to the effect that capital-shallowing does not necessarily mean success and that capital-deepening does not necessarily mean failure.

<sup>2</sup>We should take note of one other item of criticism on the theoretical level, i.e., that we assume constant returns to scale and disembodied technological change. We agree that our "innovations," like Solow's [11], thus become a residual catchall. But we are not aware—perhaps Reubens is—of an empirically satisfactory method of disentangling the various components of output growth which cannot be attributed to changes in the physical factors of production, Denison's interesting work [1] notwithstanding.

With respect to Reubens' criticisms of our data on Japan, we are in full agreement that it would have been desirable to have a better statistical breakdown between the subsistence and commercialized sectors of the dualistic economy. Unfortunately, although the problem is quite clear-cut conceptually, it is not as easy to implement statistically, i.e., it is difficult to distinguish between those portions of tertiary employment which meet the "commercialized" condition (e.g., banks and utilities) and those portions which do not (e.g., personal services and distributive trades). The procedure of permitting "agricultural" and "nonagricultural" to serve as proxies for "subsistence" and "commercialized" can undoubtedly be improved upon if the statistical problem can be solved.

We would respond in a similar vein to Reubens' comments with respect to the inclusion of residential construction in our 1930 capital stock estimates (because no breakdown between residential and nonresidential was available) while such construction was excluded from our investment or flow figures. However, when we do make an attempt to correct the 1930 base year estimates<sup>3</sup> and then apply the Rosovsky investment figures as before, the phenomenon under discussion, i.e., that of industrial capital-shallowing in nineteenth-century Japan, is maintained, although the turning point now appears somewhat earlier (see below) than with our original data:<sup>4</sup>

Year	Industrial Labor thous. <i>L</i>	Adjusted Ind. Cap. mil. yen <i>K</i>	<i>K/L</i>
1888	4,993	7,674.1	1.537
1893	5,980	8,395.7	1.404
1898	7,000	9,494.5	1.356
1903	8,111	10,862.3	1.339
1908	9,234	12,587.7	1.363
1913	10,360	15,239.7	1.471
1918	11,480	18,268.8	1.591

We are thus quite prepared to concede that the data presented are far from perfect and merely suggestive. But Reubens' own alternative estimates leave perhaps even more to be desired and, what is more important, do not support the conclusions he draws from them. Even if we accept his evidence of a rising *K/L* ratio for the economy as a whole, for example, we fail to understand

<sup>3</sup>To exclude residential construction and inventories. The former is accomplished by applying the 1889-1930 breakdown between private residential (31 per cent of total) and nonresidential construction (69 per cent of total) to the 1930 buildings figure.

<sup>4</sup>The same industrial labor-force figures have been used as before [2]. Although Reubens criticizes these figures, taken from Ohkawa [4] and based on Hijikata, as defective—as they are—he apparently, like us, finds them to be the best available: witness use of same in his Table 3. Neither did he apparently follow his own advice and adjust labor inputs by changes in man-hours per worker and in the quality of workers.

why this leads us to expect "a still stronger rise in the industrial sector alone." It seems that Reubens comes to this conclusion because *a fortiori* we may expect agricultural *investment* per head to lag behind industrial *investment* per head; but he has completely disregarded the *a fortiori* equally convincing argument that we may expect the agricultural capital *stock* per head to be much lower than the industrial capital *stock* per head. Thus as the center of gravity of the economy shifts from the agricultural to the industrial sector, industrial capital-shallowing is not only consistent with capital-deepening for the economy as a whole but, in fact, represents the most likely and realistic outcome.<sup>5</sup> It should also be well noted that, while the aggregate  $K/L$  ratio increased by only 11 per cent between 1905 and 1917, it increased by 117 per cent between 1917 and 1930 (after industrial capital-deepening set in), according to Reubens' own figures in his Table 1! Thus, even if there had been no actual capital-shallowing in the industrial sector between 1917, the extent of capital-deepening was certainly negligible as compared with after the turning point, which lies at the heart of our thesis.

Reubens' attempt to produce independent evidence on the capital-labor ratio for the industrial sector only seems beset with the same neglect of a fundamental index-number problem. The "approximations" presented in his Table 3, for example, are derived by letting durable equipment serve as a proxy for capital stock. But the fact that one—relatively minor—component of the total industrial capital stock is growing at a particular rate permits one to conclude absolutely nothing about the growth rate of the total capital stock, and thus, together with the growth rate of the industrial labor force, about the industrial capital-labor ratio over time. In fact, private gross investment in durable equipment represented less than 15 per cent of total gross investment between 1888 and 1905, his initial year [10, pp. 109-10].

Reubens' findings of a rising industrial  $K/L$  ratio are thus entirely misleading. Nor is he able to support this contention by citing the figures previously compiled by one of the authors on capital intensity for the Japanese spinning industry alone [6]. Only by asserting entirely *ex cathedra* that inconvenient figures—in this case average annual data over a five-year period,

<sup>5</sup> Reubens surely must realize that the aggregate  $K/L$  ratio at any one point in time is a *weighted* average of the sectoral  $K/L$  ratios  $(K/L)_i$  and  $(K/L)_a$ , i.e.,

$$\frac{K}{L} = w_i \left( \frac{K}{L} \right)_i + w_a \left( \frac{K}{L} \right)_a$$

where  $w_i$  and  $w_a$  are the weights. To illustrate briefly, let us assume (realistically for Japan) that at the beginning of the period 80 per cent of the economy is subsistence and 20 per cent commercialized, i.e.,  $w_{a1} = .8$  and  $w_{i1} = .2$ , while at the end of the period the proportions are 50-50, i.e.,  $w_{a2} = .5$  and  $w_{i2} = .5$ . Then, given a higher level of  $K/L$  in industry than in agriculture in period 1, e.g.,  $(K/L)_{i1} = 5$  and  $(K/L)_{a1} = 2$ , we have initially  $2.6 = .2 \times 5 + .8 \times 2$ . If we then permit the industrial capital-labor ratio to fall, e.g.,  $(K/L)_{i2} = 4$  (keeping that in agriculture which is not at issue constant, i.e.,  $(K/L)_{a2} = 2$ ), we have  $3.0 = .5 \times 4 + .5 \times 2$  at the end of the period. Thus, not only is industrial capital-shallowing perfectly consistent with aggregate capital-deepening but it is exactly what we would expect to happen.

1886-90—are subject to underreporting and are thus to be disregarded, can he make his point with respect to the pre-World War I period under discussion. Moreover, these data again do not reflect changes in plant and, as Reubens himself must realize [9], it was in the continuing reorganization of plant, via subcontracting, “putting-out” arrangements, etc. that major labor-using technological changes were to be encountered—in this case in the weaving branch of the important textile industry cited.

Finally, let us turn to the so-called qualitative evidence presented in Section IV of Reubens’ comments. At the outset he cites four possible reasons why capital-shallowing may conceptually take place in the industrial sector of the developing economy. We have no quarrel with these—though the force of point (a), dealing with a higher  $K/L$  ratio in the preconditioning period with which our article was not directly concerned, escapes us. In fact, we should welcome the introduction of what may be a useful taxonomic device, were it not for the rather strange uses to which it is subsequently put.

As we understand it, Reubens’ first reason, his point (b), is based on the to-be-anticipated relatively larger role of the relatively more capital-intensive social (and economic) overheads in the initial stage of development. But why should this lead to “a rising  $K/L$  ratio at least into the first decade of the twentieth century, to be followed by a falling ratio”? Reubens seems to be expounding some sort of technological law that the typical developing economy must experience exactly three to four decades of overhead construction to be followed by the expansion of directly productive industrial activities. This surely is an unreasonable view of reality. Granted that roads, railways, and other overhead construction must be undertaken early, their relation to the mushrooming of directly productive industrial activities is certainly not of a discontinuous, sequential nature. There is no reason to assume that growth in the latter, proceeding at a rapid clip virtually from the outset, will not swamp the effects of the former.

Reubens’ second reason for capital-shallowing, his point (c), deals with the possibility that industries of high labor-intensity may be expanding faster than those of low labor-intensity. Again, we agree. But we are less sure of the relevance of the destruction of old handicrafts by modernized small-scale industry of higher capital-intensity. Instead of dealing with the central issue of a likely shift in the product mix as between industries, he appears mainly interested in the possible decline of particular nonviable traditional methods of production within a given industry. It is a fact that historical Japan, especially as she was able to participate more fully in the world economy, found herself in a position to continuously shift her product mix toward the more labor-intensive industries, while importing more and more of the more capital-intensive products required.

Finally, in discussing his third reason for capital-shallowing, point (d), he appears again to be in perfect agreement on the existence of a labor-using innovational potential within a given industry and on the possibility of its exploitation by rational profit-maximizing capitalists. But in virtually the next sentence he rejects either or both of these propositions entirely on the basis of his real-world observations. The relevant evidence is not presented,

but even if it were, it is strange logic to assert that a proposition is faulty just because we can observe contrary practice. In fact, it was the main burden of our argument in reviewing India's performance [2] that the practice of adopting increasingly capital-intensive production functions, either consciously for prestige reasons or unconsciously as a consequence of direct government controls (which serve to undervalue the cost of domestic and foreign capital), proved detrimental to the growth process under conditions of an unlimited supply of labor. Unfortunately, the attitudes and policies described are by no means rare in the contemporary less developed world—and neither are cases of nonsuccess in development, as those of India and Communist China cited by Reubens.<sup>6</sup>

In summary, Reubens insists that labor cannot be continuously "substituted" for capital in the developing economy's industrial sector and that the instances of labor-using innovations cited by us for historical Japan merely represent cross-sectional examples of restraint in adopting more capital-intensive methods that were available. We do not agree—and neither, apparently, does Lockwood:<sup>7</sup>

If Japan's experience teaches any single lesson regarding the process of economic development in Asia, it is the *cumulative* importance of myriads of relatively simple improvements in technology which do not depart radically from tradition or require large units of new investment. . . . Much of the real substance of Japanese economic growth . . . is found in the more modest types of improvements which were more easily and pervasively adopted, more economical in cost, and often more productive of immediate returns in income. For any poor country beginning to industrialize, one of the crucial problems is to introduce and *spread* such innovations as widely as possible [3, pp. 198-99, italics ours].

The adoption of new devices to absorb the surplus agricultural labor force was a continuous process within particular industries; such devices included adoption of double-shifting, the speeding-up of machines, and the gradual replacement of long-staple American by short-staple Indian cotton—i.e., machine-oriented innovations, as well as, and more importantly, continuing at-

<sup>6</sup>With respect to India, Reubens must surely realize—from the industrial profit-maximizing assumption in our model, if not from our entire discussion—that we are *not* advocating the artificial retention of uneconomic Ghandian handicrafts for the sake of creating employment in the welfare sense, but the continuing search for indigenous technological and organizational improvements in response to the pressures of factor endowment (see also [8, p. 770] for a fuller statement). While we have never wished to claim that India faces as easy a task today as Japan did 70 years ago, her chances for success would be much brightened if she undertook to facilitate, rather than hinder, the spread of innovational ideas and impulses across the masses of would-be participants in the development process. With respect to Communist China, we certainly do not believe that the adoption of a strategy of capital-shallowing (concentrated incidentally in agriculture) provides protection against other "bad" policies, e.g., setting up uneconomic backyard furnaces and disregarding peasant incentives.

<sup>7</sup>Rosovsky's [10, p. 11] conclusions are identical: "Rapid growth of output could result from moderate improvements in labor-intensive techniques, small amounts of capital and . . . mostly rapid increases in the labor force, in the form of migrants from rural districts. This is precisely what happened in Japan."

tempts to reorganize the industrial process via more efficient handling and subcontracting routines—i.e., plant-oriented innovations (see [6]). Finally, and undoubtedly of the greatest significance, was the possibility of a continuing shift in the product mix, especially as Japan was increasingly able to explore the world markets for her labor-intensive industrial products. As Rosovsky put it:

... the policy of enforced free trade had very beneficial long-run effects ... Japan was poor in resources and capital; her main assets were a large, cheap and relatively efficient labor force, and considerable entrepreneurial talents. These factor proportions suggested the *development* of industries permitting the use of labor-intensive techniques [10, p. 101].

And Lockwood:

... the demand for Japanese manufactures ... was such that a greater share of production was directed towards particular products and processes which required larger amounts of labor per unit of output [3, p. 184].

For example, the labor-intensive textile industry, which represented 18.2 per cent of national income originating in industry in 1878, represented 26.1 per cent by 1895 [7], and, to cite another indicator of what was happening, commodity exports as a proportion of domestic commodity output rose from 5 per cent in 1878-82 to 15.5 per cent in 1918-22 [5, p. 140]. Though we do not find it necessary from either the theoretical or policy points of view expressed in our article to insist on more than a restraint in the rise of the capital-labor ratio over time, we see no reason to abandon our belief that capital-shallowing actually took place in the Japanese case.

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### The Stability of Neutralism: Comment

Professor Hirschman's refreshing treatment of "The Stability of Neutralism"<sup>1</sup> suggests the type of contribution that economic tools can make to the solution of problems in the other social sciences. However, such an excursion into interdisciplinary analysis requires a high degree of precision, especially when new concepts are introduced. When using new concepts care must be taken not to revert to their often imprecise and confusing colloquial connotations; in this instance lack of care resulted in an error in reasoning.

Two concepts—economic independence and political neutrality—were introduced and lack of differentiation between them led to conflict. In his diagrammatic presentation Hirschman represented economic aid on the ordinate and political alignment on the abscissa. Aid is available from both Russonia and Usonia, and each is willing to give a maximum of *OT* tons of gold (Figure 1) to a fully committed nation; *economic independence* may be defined as equal aid for Thirldonia from each of the two big powers. On the other hand, political alignment is measured by the proportionate distribution of the underdeveloped land's votes in the United Nations General Assembly; *political neutrality* is achieved at *ON* when its votes are distributed equally between the two superpowers.

The true meaning of the concepts may best be demonstrated in the case where Russonia and Usonia act symmetrically and do Not Penalize Neutrality. No conflict between the two ideas arises; Thirldonia can maximize the volume of bullion it receives by behaving like a neutralist and demanding economic independence. Referring to Figure 1b in the article (reproduced as Figure 1 below), we find that political neutrality can be achieved without sacrificing economic independence; *OA* tons of gold will be forthcoming from each donor if *ON* votes are delivered to Russonia and Usonia at appropriate times in the General Assembly. In Hirschman's framework this is a stable solution—the concave opportunity locus determined by the policies of the two powerful, industrialized nations will be tangent to a convex indifference curve of the independent and neutralist Thirldonia.

A pitfall in the use of the two concepts arises when one of the powers attempts to take advantage of this stable equilibrium. A change by one of the

<sup>1</sup> This *Review*, March 1964, pp. 94-100.

rich nations (say, Russonia) from a policy of Not Penalizing Neutrality to one of Punishing Neutrality leads to the paradoxical situation that by giving less aid a country can secure more votes in the United Nations. This is illustrated in Figure 2 by a slightly modified version of Professor Hirschman's fifth figure.

In this case, Russonia and Usonia are each contributing *OI* tons of gold for the development of Thirldonia, while Russonia is receiving a larger share

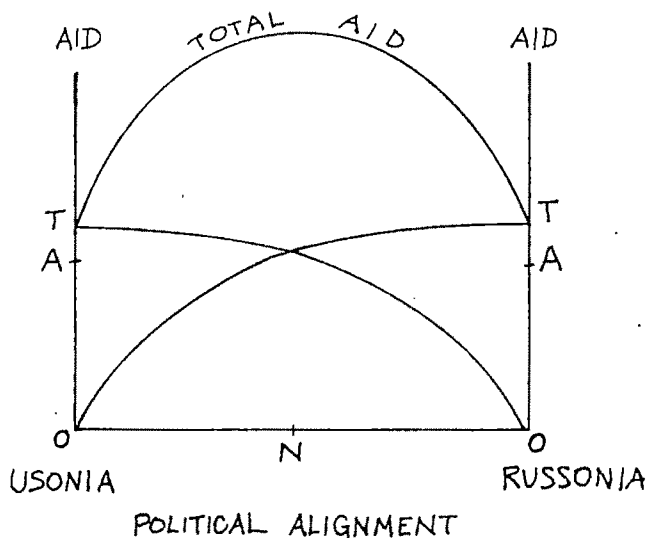


FIGURE 1

of the votes. Clearly, Thirldonia is not politically neutral as Professor Hirschman suggested when he said "If Thirldonia is neutralist . . . it will now select the political position that will equalize aid from both camps and will therefore tend to take up the political position . . . rather close to Russonia." Economic independence and political neutrality are conflicting goals in this example; a move to distribute votes more equally in the General Assembly will force Thirldonia to become economically dependent on Usonia. Therefore, Thirldonia's position will not be as simple as indicated in the note. Although the country cannot influence the total volume of bullion it receives because the policies of the two richer nations determine its opportunity locus, Thirldonia can determine the amount of aid it receives from each donor by its voting behavior. The poor country will be torn between two positions of political neutrality and economic independence, represented by *N* and *I*, respectively, in Figure 2. Thirldonia, with a preference for both political neutrality and economic independence, will be as unhappy about remaining in position *D* as Usonia will be about its loss of support in the United Nations. A move to an intermediate position (*C*) involves a trade-off of less political

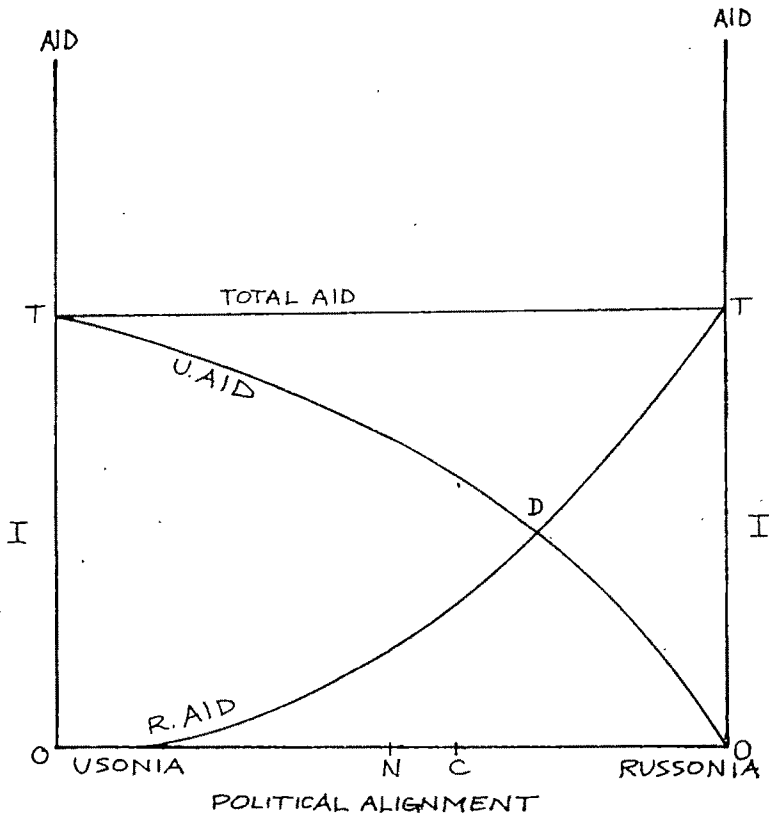


FIGURE 2

alignment with Russonia (more neutrality) for more economic dependence on Usonia (less independence).

Whatever position the neutrally and independently inclined country opts for, the paradoxical situation alluded to by Hirschman persists: Thirldonia favors Russonia as a direct result of its reduction in aid. This, of course, would last only until Usonia reacts to Russonia's policy by adopting a policy Penalizing Neutrality; in this event the world would conform to the unstable combination of Aid-plus-Independence (or more precisely, Independence-plus-Neutrality) and Penalizing Neutrality of Hirschman's schema, that is, minimum development assistance for Thirldonia.

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\*The writer, a graduate student in the department of economics at Yale University, is indebted to Professor Neil Chamberlain for helpful comments.

### The Stability of Neutralism: Reply

Mr. Barkin's point is a useful elaboration of my note. The nonsymmetrical aid-giving behavior I portrayed in Figure 5 is indeed likely to set off a conflict between a neutralist aid-receiving country's desire to remain politically equidistant between the super-powers and its propensity to distribute its receipts of aid as equally as possible between them, with the result that some compromise between these two conflicting urges would emerge. Unfortunately, his eagerness to convict me of "an error in reasoning," while achieving for himself a "high degree of precision," obliges Mr. Barkin to surround his argument with an air of unreality by unhelpfully "precise" definitions of neutralism and economic independence. These two concepts cannot be distinguished as neatly as he proposes to do; it would be more realistic, so it seems to me, to speak of two behavioral dimensions of neutralism (or of both neutralism and independence): one could term them the equidistance propensity and the equidistribution (of aid) propensity. After all, it is largely the desire to appear neutral which makes Thirldonia wish to distribute its receipts of a given total of aid as equally as possible between Usonia and Russonia. I neglected one of these dimensions (and quite a few more, I suspect) and am happy to see that my model can handle it with ease.

I take this opportunity to point out that the move of one of two aid-giving powers from a policy Not Penalizing Neutralism (NPN) to one that does (PN)—see my Figures 1b and 5 or Barkin's 1 and 2—bears a striking similarity to the attempt on the part of a trading country to improve its terms of trade by imposing a tariff. A unilateral transition of one of the aid-giving powers from the NPN to the PN policy permits it to reap better "terms of aid"—the country that lowers its aid schedule will paradoxically achieve a political gain. One limitation of this gain lies in the likelihood of retaliation; but, even in the absence of retaliation—and this is a further parallel to the tariff argument—there is an *optimum* point in the unilateral shift from the NPN to a PN policy beyond which Thirldonia is likely to turn away from the aid-policy-shifting power and back toward the country which maintains an NPN policy. Suppose Russonia suddenly decides that, short of complete alignment, no aid whatever will be forthcoming. Then the total aid curve would have the shape of a distorted *V* (somewhat like a square root without the horizontal portion) and, if Thirldonia is neutralist, we would be essentially back at the unstable AI-PN combination: Thirldonia may go all the way back to Usonia, but this is uncertain. A more determinate result is obtained if we follow Barkin and let the equidistance propensity come into play: Thirldonia would then most likely end up at a point somewhere between complete alignment with Usonia and the mid-point on the alignment scale, thus achieving its minimum requirements of nonalignment at

the smallest possible sacrifice of aid. In this fashion, it can be shown that a *radical* policy of penalizing neutralism is self-defeating even, and perhaps especially, in the absence of retaliation.

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### Baumol's Sales-Maximization Model: Comment

Professor Baumol has provided an addition to the ever-increasing body of oligopoly theory by substituting sales maximization, with a minimum profit constraint, for profit maximization as the goal of the large business firm. On the basis of a model which he has developed, Baumol claims to be able to show that the oligopolist is in equilibrium at that price and output where realized profit is equal to the minimum acceptable profit. It is my contention that the equilibrium which Baumol derives is only one of many possible constrained output levels.

Baumol's basic static oligopoly model is presented in Figure 1. Output is measured on the horizontal axis, and total revenue, costs, and profits are measured on the vertical axis.<sup>1</sup>

If the firm were a profit maximizer, it would produce  $X_1$  units of  $X$ ; however, Baumol hypothesizes that the firm attempts to maximize sales (total receipts) subject to a profit constraint. Assuming that the minimum acceptable profit is  $\pi_1$ , the firm produces the output  $X_2$  where total receipts are maximized. With an output of  $X_2$  the firm earns  $\pi_2$  profits, which are greater than the minimum required by the stockholders.

Had the minimum acceptable profit been  $\pi_3$  rather than  $\pi_1$ , the firm would have had to stop at  $X_n$  units of output. At output  $X_n$  it would not have been maximizing total receipts; therefore, "two types of equilibria appear to be possible; one in which the profit constraint provides no effective barrier to sales maximization [ $X_2$  units of output with a minimum acceptable profit of  $\pi_1$ ] and one in which it does [ $X_n$  units of output with a minimum acceptable profit of  $\pi_3$ ]" [1, p. 55]. The firm varies output by increasing or decreasing price, and it does so without concern as to the reactions of rival firms [1, pp. 27-32]. Therefore, the oligopolist has an independent price policy which he uses to maximize sales revenue if not constrained from doing so by a minimum acceptable profit.

By introducing advertising into his model, Baumol claims that an unconstrained sales maximization analogous to output  $X_2$  in Figure 1 is not pos-

<sup>1</sup>For the sake of simplicity we shall assume that the firm produces a single product and that it faces a linear demand and cost function.

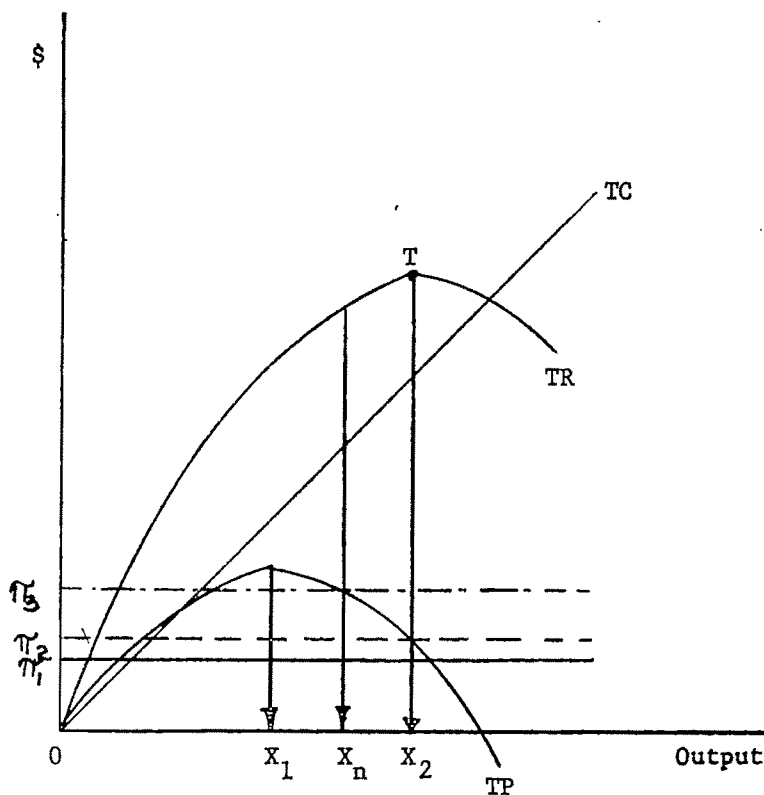


FIGURE 1

sible. In Figure 2 advertising outlay is measured on the horizontal axis with total revenue, costs, and profits on the vertical axis [1, p. 60]. The total revenue curve increases throughout the relevant range because of the assumption "that increased advertising expenditure can always increase physical volume, though, after a point, sharply diminishing returns may be expected to set in" [1, p. 60]. Baumol goes on to say that "unlike a price reduction, a *ceteris paribus* rise in advertising expenditure involves no change in the market value of the item sold."<sup>2</sup> Therefore an increase in advertising outlay must necessarily increase total revenue. "As a result it will always pay the sales maximizer to increase his advertising outlay until he is stopped by the profit constraint—until profits have been reduced to the minimum acceptable level" [1, p. 61]. Baumol does not consider the case where the firm changes price along with the advertising budget, both of which influence output, in order

<sup>2</sup> I assume by this statement that Baumol means price to be given when the firm varies advertising outlay.

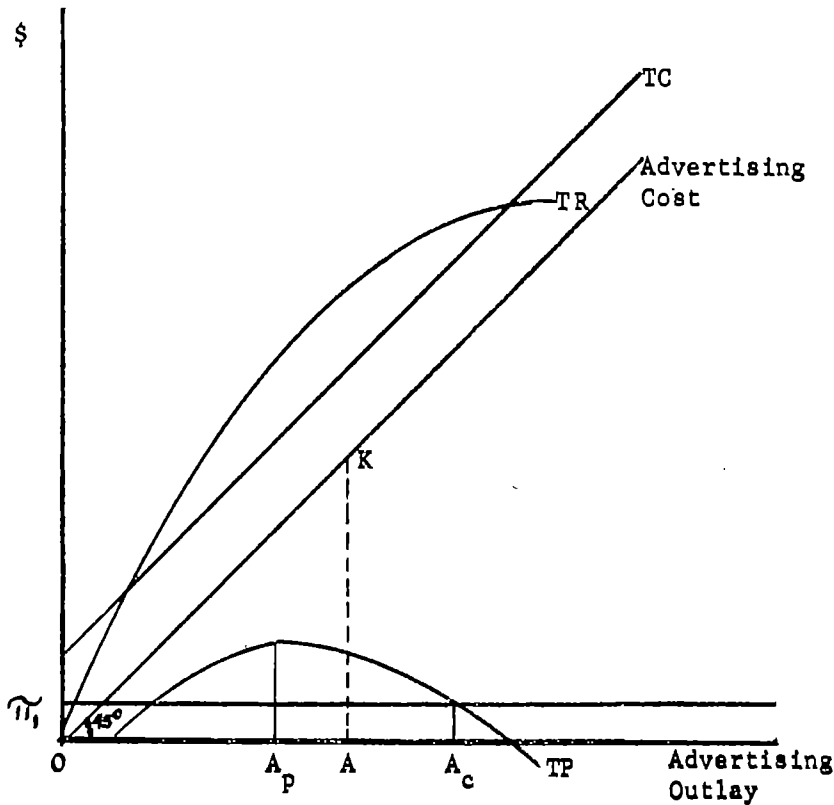


FIGURE 2

to obtain the highest sales revenue given the restriction of earning a minimum profit.

According to Baumol, "the interrelationship between output and advertising decisions now permits us to see the reason for the earlier assertion that an unconstrained sales maximizing output . . . will ordinarily not occur. For if the price is set at a level which yields such an output profits will be above their minimum level and it will pay to increase sales by raising expenditure on advertising, service or product specifications" [1, p. 61]. Once again Baumol fails to mention that sales revenue may be increased by changing price as well as the advertising budget.

It appears that Baumol is saying the firm will produce output  $X_2$  (Figure 1) and charge a price of  $X_2T/OX_2$ . This provides a profit level of  $\pi_2$  which is greater than the minimum acceptable profit of  $\pi_1$ ; therefore, the firm will increase advertising outlay given the price of  $X_2T/OX_2$  (Figure 2). In other words, the total revenue function as presented in Figure 2 assumes a given price. This being the case, there is a different revenue function for every price

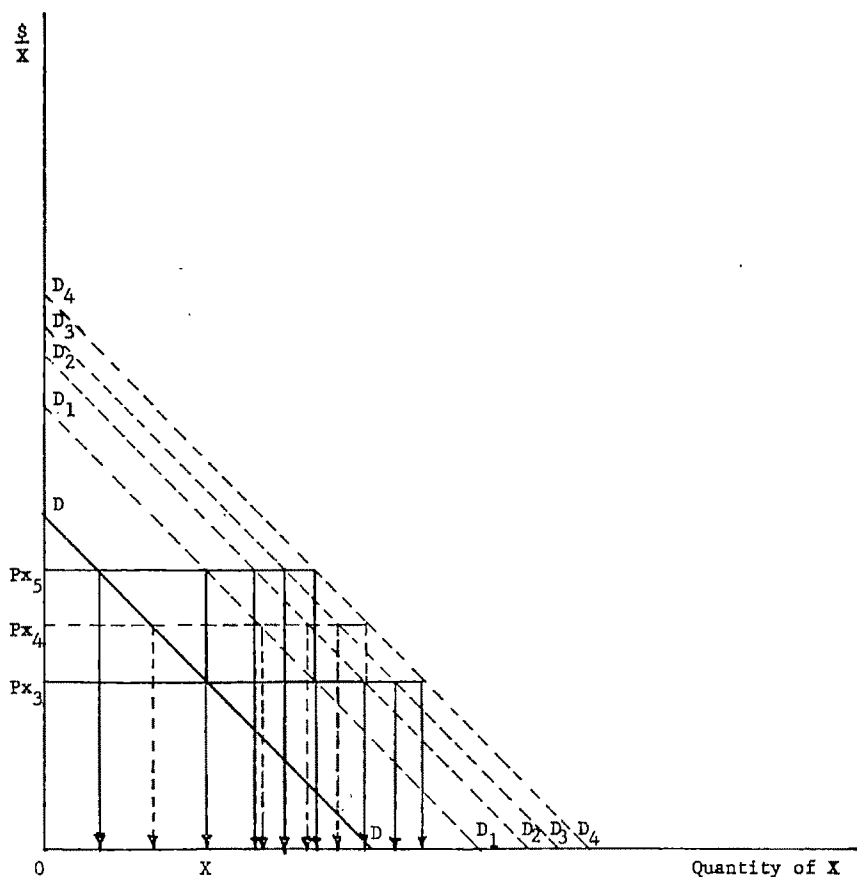


FIGURE 3

which may be assumed. What we have then is a family of revenue functions and, given costs, a family of profit functions.

Let us assume the oligopolist varies advertising outlay one unit at a time<sup>8</sup> which causes the demand curve facing the firm to shift to the right as in Figure 3. The demand curves are drawn so as to be consistent with Baumol's assumption "that increased advertising expenditure can always increase physical volume, though, after a point, sharply diminishing returns may be expected to set in" [1, p. 60]. The firm faces demand curve  $DD$  before advertising and, since it is not constrained from maximizing sales, a price of  $Px_3$  will be charged and  $X$  units sold. Assuming that, at this price and output, the realized profit is greater than the minimum acceptable profit, the firm will

<sup>8</sup>One unit is equal to any total dollar amount we may want to select; however, it is assumed that the advertising budget is increased in equal increments.

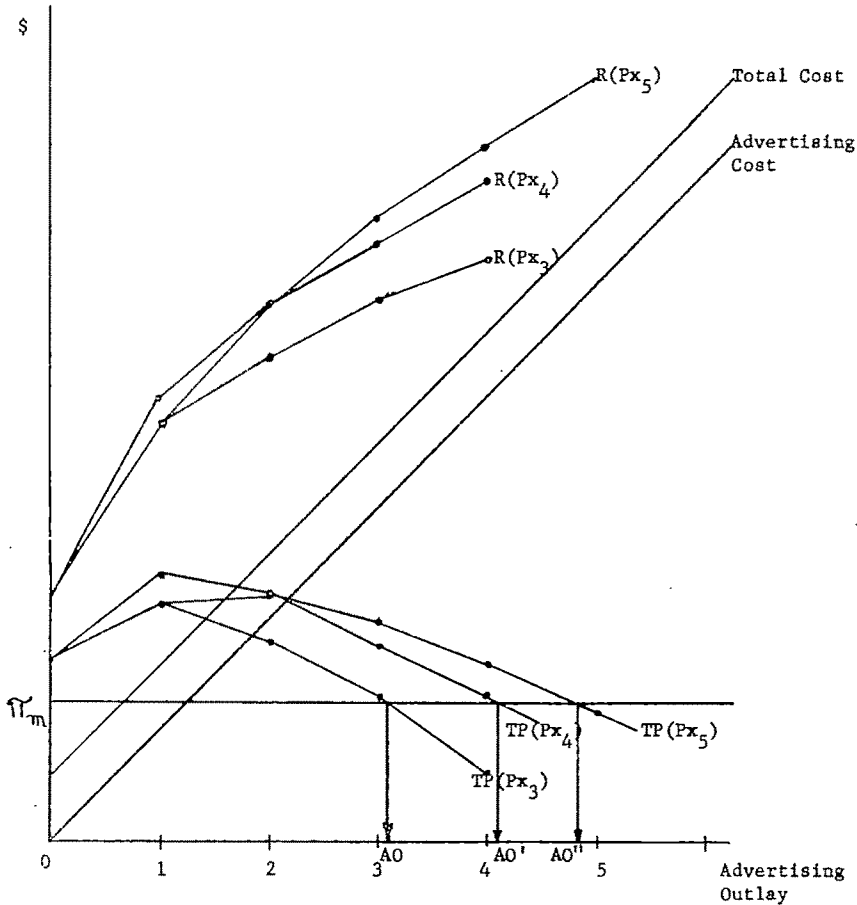


FIGURE 4

increase advertising outlay, which causes output and, given price, sales revenue to increase.

In Figure 4 advertising outlay is measured on the horizontal axis and total revenue on the vertical axis.<sup>4</sup> The revenue functions ( $R$ ) are derived by assuming a given price and then varying advertising outlay; therefore, there is a different  $R$  curve for each of the assumed prices. The advertising and total cost lines are like those Baumol uses. Given total cost, there is a different profit curve for each of the  $R$  curves.

Assuming a minimum profit level of  $\pi_m$ , it is obvious that there are several possible constrained output levels as depicted by advertising outlays of  $AO$ ,

<sup>4</sup>By starting the revenue functions at a positive intercept on the vertical axis, I have circumvented the rather unrealistic assumption, made implicitly by Baumol, that when advertising outlay is zero, output and sales revenue are also zero.

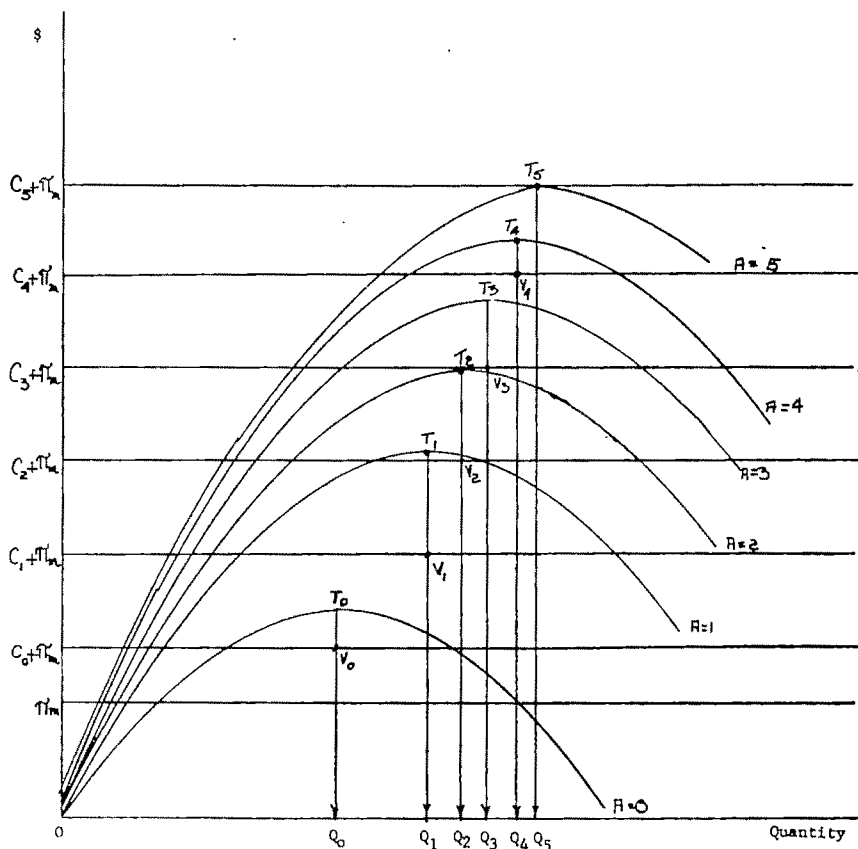


FIGURE 5

$AO'$ , and  $AO''$ .<sup>5</sup> Which of the constrained output levels is equilibrium? This model does not correctly show the constrained output equilibrium. In order to derive the constrained output equilibrium, we must develop a model which allows price, advertising outlay, and product output to change. From this model we will be able to determine the advertising budget and price which determine the output where sales revenue is at a maximum, given the fact that revenue must cover advertising and other costs plus a minimum acceptable profit.

The total revenue functions in Figure 5 correspond to the several demand curves of Figure 3. The firm maximizes sales revenue as long as the revenue covers advertising and other costs plus a minimum acceptable profit. This limitation is depicted by the lines labeled  $C + \pi_m$ . These lines will shift up-

<sup>5</sup> The number of constrained output levels depends upon several factors such as the minimum acceptable profit level, the position of the cost functions relative to the  $R$  functions, and the number and position of the  $R$  functions.

ward at a constant rate once advertising outlay is introduced.<sup>6</sup>

If the advertising outlay is zero and the cost plus minimum profit level is  $C_0 + \pi_m$ , then the firm will be able to maximize revenue by producing output  $Q_0$  at a price of  $Q_0T_0/Q_0$ . The revenue exceeds  $C_0 + \pi_m$  by the amount of  $T_0V_0$ . The firm can increase sales revenue by increasing advertising outlay and price. We now move to the total revenue curve which is the result of one unit of advertising. The cost plus minimum profit level is  $C_1 + \pi_m$ . The firm can maximize revenue by producing an output of  $Q_1$  at a price of  $Q_1T_1/Q_1$ . At output  $Q_1$  revenue exceeds  $C_1 + \pi_m$  by  $T_1V_1$ . Since profits are still in excess of the minimum required, the firm will once again increase advertising and price. With an advertising outlay of two units, the oligopolist maximizes sales revenue at  $Q_2$ , price is  $Q_2T_2/Q_2$ . Revenue is in excess of  $C_2 + \pi_m$  by an amount of  $T_2V_2$ , which means that advertising outlay and price will once again be increased. The oligopolist continues to increase his advertising budget and price until a maximum revenue is attained which is just equal to total cost (advertising and other) plus the minimum acceptable profit. In our model this happens at an output level of  $Q_5$  with a price of  $Q_5T_5/Q_5$  and an advertising outlay of five units. Therefore,  $Q_5$  is the constrained output equilibrium.<sup>7</sup>

One can now use a model similar to Baumol's (which is depicted by my Figure 2) without the confusion which exists in his exposition. The  $R$  function in Figure 6 is not drawn with the assumption of a given price. A revenue of  $R_1$  is attained with an advertising outlay of one unit, an output of  $Q_1$  (Figure 5), and a price of  $Q_1T_1/Q_1$ . Likewise a revenue of  $R_2$  is brought about by an advertising outlay of two units, an output of  $Q_2$  (Figure 5), and a price of  $Q_2T_2/Q_2$ , and so on until the revenue  $R_5$  is reached which is the maximum, given the fact that it has to cover advertising and other costs plus the minimum acceptable profit.

Contrary to received doctrine, Baumol makes the point initially that the large firm (oligopolist) does have an independent and definite price policy [1, pp. 27-32]. He then develops his sales maximization model, which is reproduced in my Figure 1. In this model the firm varies price so as to bring sales revenue to the maximum level unless constrained from doing so by the minimum acceptable profit level. Baumol then abandons his assumption of a definite price policy in favor of changing advertising budgets to describe the constrained output equilibrium; however, he does not tell us how the price is reached which brings about, along with a given advertising budget, this constrained output equilibrium. Once the constrained output equilibrium is achieved, he returns to the assumption that the firm has a definite price policy. For example, in criticizing Professor Galbraith's oligopoly model he says, "To Professor Galbraith, as to many investigators, a prime character-

<sup>6</sup>This is consistent with Baumol's assumption that the advertising cost function is a 45° line and that total cost is parallel to advertising cost.

<sup>7</sup>From the mathematical calculations on which this model is based, the equilibrium advertising outlay is 4.94948974 rather than five. This means that the equilibrium constrained output is slightly less than  $Q_5$ .

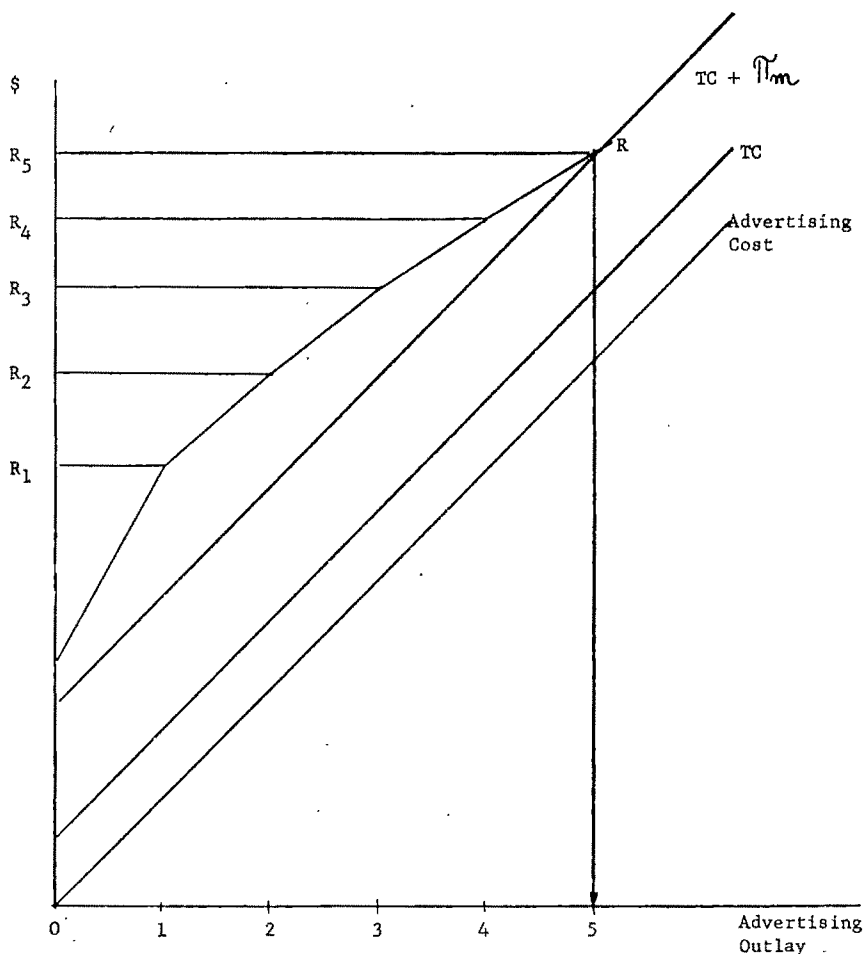


FIGURE 6

istic of the oligopolistic firm is that it does not normally set a price which maximizes profits. So far I am in agreement. But in his analysis there seems to be no alternative explanation of the price setting process" [1 p. 79]. This statement implies that Baumol has explained the price-setting process, along with determination of the advertising budget, which, as has been demonstrated in this paper, is not the case.

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\*The author, assistant professor of economics at Oklahoma State University, is indebted to E. J. R. Booth for the help which he provided and to J. Bradsher, R. Leftwich, and F. Steindl for useful and constructive criticism.

### Baumol's Sales-Maximization Model: Reply

Professor Sandmeyer has done a fine job of graphsmanship and shown ably how price and advertising outlay are to be determined simultaneously in my sales-maximization model. It is only to be regretted that he feels it necessary to allege that I failed to recognize the very obvious mutual interdependence of the firm's price and advertising decisions. He takes such a position despite the fact that this interdependence is treated in detail at several points in the chapter of my book at which he directs his criticisms.<sup>1</sup> I had thought that by now a partial analysis is at least an accepted and permissible expository simplification.

Sandmeyer should recognize that, in order for a writer to produce something which is original and correct, it is not absolutely necessary that his predecessors have been wrong.

WILLIAM J. BAUMOL\*

<sup>1</sup>See my *Business Behavior, Value and Growth*, New York 1959, Ch. 7, Section 4, and mathematical appendix. Indeed, I believe that my mathematical appendix and Sandmeyer's graphs cover largely the same ground.

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### Correction

In the June 1964 issue, the author of the communication, "The Case for Capital Controls to Relieve the U.S. Balance of Payments," should have been listed as D. A. Snider on the outside cover and in the table of contents, and not as D. A. Snyder.

## BOOK REVIEWS

### General Economics; Methodology

*Strategy and Conscience.* By ANATOL RAPOPORT. New York: Harper and Row, 1964. Pp. xxvii, 323. \$6.95.

Lincoln Steffens tells how he started a "crime wave" by filing stories while the rest of the reporters played pinochle at police headquarters. His competition broke up the game, and for several weeks the harried reporters barraged their papers with exaggerated coverage. Teddy Roosevelt had to go to New York to get the pinochle game going again and stop the crime wave, which was embarrassing the New York Crime Commission. About four years ago a wave of game theory arose, possibly at the RAND Corporation, engulfed the universities, and is lapping at the Pentagon. Even a few members of peace movements have been caught in the undertow. The wave has been tracked by James Newman, Wayland Young, P. M. S. Blackett, and dozens of others who either don't like strategy or don't like someone else's strategy.

Rapoport's is not the first full-length book to sound the alarm, but it ought to be the most authoritative. He is himself a game theorist and a creative scholar (whose contributions to economic theory were unfortunately buried for years in the *Bulletin of Mathematical Biophysics*). His book is a passionate warning to disregard the "strategists," the "cool young men," the "paper warriors" whose thinking is "psychopathic" and "utterly devoid of moral sense," who view "global conflict as a game" and consider "successful nuclear surprise" as a "winning move of a game," who decry the "illusion" that war is a dirty business and invoke the scientific outlook "in combatting the fear and the abhorrence of war." In the expressive language of Karl Deutsch's introduction, the book is "a protest against a glib and shallow fashion of contemporary thought that embodies and enhances man's inhumanity to man." In short, "When I say 'strategist,'" says Rapoport somewhat anti-climactically, "I mean someone who at the moment conceives international problems in strategic terms" (p. 176).

The book makes two points. (1) Strategic thinking is bad. (2) Game theory is to blame. Contemporary strategists express their theories in abstract and mathematical terms and assume that their problems have "solutions"; they "optimize" a "mix" to maximize expected utility, treat nations as "players" motivated by "payoffs," take "rationality" for granted, and "live in a world similar to that of a detective story, where murder is a game of reversed blind man's buff and everyone can be It." They also have an obsession with "gaming" which, Rapoport is occasionally aware, has little to do with game theory except for being equally cute and equally subject to abuse. Furthermore—and Rapoport might have emphasized this more explicitly—their game theory is *bad* game theory if he describes it correctly, dominated by a zero-sum tradition and by "games against nature," so that even in the

most conspicuously adaptive processes the adversary is rigidly assumed to be rigid in his antagonism. (Actually I was surprised that he wrote his book against game theory, rather than one against bad game theory calling for better. By any definition that Rapoport could possibly have in mind, he and Kenneth Boulding are game theorists who explicitly use game theory in elucidating foreign policy, and use it more than any "strategist" that I know of or that Rapoport knows of. So there must be "good game theory.")

Like his *Fights, Games and Debates*, this book is in three parts, and again the third is his personal exploration of what is at issue between East and West, what the capitalist and communist systems have in common and how they differ, what the prospects are for reconciliation, and what kind of dialogue is required for reconciliation. Its only relation to the first two-thirds of the book is contrast: this is a domain that strategists never explore. But since Rapoport acknowledges that hardly anyone does, that is not a characteristic of strategists.

The first third is a technical introduction to probability, zero-sum games, prisoner's dilemma games, and bargaining theory, reminiscent of the first two parts of his earlier book. It is the middle section that tells his thesis, describing what the strategists do and how they think and how they do not think, what is wrong about it, and why it all flows from their preoccupation with probabilities, utilities, and game theory as introduced in Part I. The 75 pages of game theory at the beginning of the book are evidence that I do not misconstrue the author: Rapoport is not saying that the strategists *remind* him of second-rate game theorists and share their style, but that they *are* second-rate game theorists, and this accounts for their analytical style and their foreign-policy premises.

Being one of the accused, I naturally do not care for the first hypothesis, that "strategic thinking" is bad and corrupts the people who rely on it; nor am I impressed with the second, that it all flows from game theory. The first is the one that matters, but is bound to be somewhat impressionistic and likely influenced by the policies one finds congenial. Since we do have this crime wave, and this is a review of Rapoport's book, let me try to dispose of the second hypothesis, which ought to be susceptible to evidence. First, although Deutsch in the introduction says, "The strategic thinkers whom Rapoport criticizes are clearly identifiable from his text," and Rapoport himself says, at one point, "I have now identified the strategists" (p. 186), it is a little hard to tell if he knows *whom* he is talking about even if he knows what he is talking about. The names of seven strategists occur in the book (Herman Kahn, Kaufmann, Kissinger, Knorr, R. Osgood, Schelling, G. Snyder), and they account for 12 items out of 132 in the bibliography. This is my census; they are never listed as a group, nor is the method of selection described. (I am excluding Liddell-Hart, who was a dean among strategists before *Theory of Games* ever appeared; Yarmolinsky, who is represented only by part of a speech he gave; and the Raytheon Corporation, whose mode of thinking is a little hard to personify.) Seven is a small sample, but it probably contains about half of the strategists in print who can be persuasively associated with game theory. Of the seven, four I know personally to be innocent of

game theory; and even if one thinks that William Kaufmann was implicitly infected with it during his sojourn at the RAND Corporation, it is worth noticing that the three selections attributed to him were written ten years ago, before he ever joined RAND. Kissinger has a colleague who would have to plead guilty; but even guilt by association would have to be retroactive for the work cited by Rapoport, since Kissinger wrote it before I ever met him. It boils down to three possible game theorists, against one of whom the evidence is that he included six simple matrices and one probability equation in an otherwise "literary" book on defense policy; and I can certify that his analytical style in that book is the same as in things he published before he adopted matrices for his bookkeeping. That leaves two, Herman Kahn and myself. (I hope that both Kahn and the game theorists will let me include both of us here for the sake of argument.)

Whatever else one may say of the two of us, we are bound to defy statistical analysis. Anyone who has met Kahn will surely acknowledge that he cannot be described as a man so ordinary in all other respects that his passing interest in game theory and the style of his strategic analysis have an indubitable relation of cause and effect. David Riesman once hinted that civilian strategists act tough because, not knowing at first hand the cost of war, they need to prove themselves virile. As a conjectural diagnosis, this must compete with Rapoport's. I know two outstanding strategists within the Department of Defense who are devout Catholics but, then, there are Jews among us and at least one Olympic skier, and I don't know what else. Neither does Rapoport. So his correlation coefficient relating game theory to "strategic thinking" is suspect; but it is too low to be interesting anyhow, just two out of seven in his sample.

I can raise the absolute number by adding Oscar Morgenstern, whose *Question of National Defense* is unaccountably missing from Rapoport's bibliography, and Daniel Ellsberg, as well as one or two others who might be identified both with strategic thinking and with game theory, but in a larger sample the denominator would rise at least proportionately. (The exclusion of Wohlstetter goes against recent tradition, but Rapoport treats the strategists as sufficiently interchangeable for it not to matter.) And I would have to put Rapoport himself and Boulding and a few other good guys into the sample, and I am not sure the correlation of monstrosity (his word) with game theory even comes out positive. One can also raise the question whether foreign relations is uniquely the field in which game theory leads one astray. Rapoport admires Braithwaite for his "charming and perceptive book," *Theory of Games as a Tool for the Moral Philosopher*. Karl Deutsch, who likes this book enough to introduce it, also likes game theory applied to politics and says so in his *Nerves of Government* (1963), but is uneasy about its relevance to foreign affairs. There is thus a hint that game theory stultifies only those who worry about Russians. If so, it calls for a more refined psychological analysis.

I have read one other full-length manuscript devoted to the thesis that game theory—literally game theory, the kind that got started with von Neumann and Morgenstern—accounts for the depraved quality of American stra-

tegic thinking, and one in print (Irving Louis Horowitz, *The War Game*) that, though it confuses game theory and "gaming," makes much the same point. Since statistical analysis of the open literature cannot possibly support the proposition, I wondered whether there was a reasonable premise that could make it true. There is: these few gamely theoretical strategists occupy but the top of an iceberg. They are emigrants from classified organizations whose analytical style they typify; behind them is a vast population of economists and game-theory strategists who not only dominate policy but impose their fashion on the nongame-theory publicists. This would be a reasonable conjecture, but being reasonable doesn't make it right. I have been submerged, and I did not see the iceberg underneath. No one has to take my word for it; still, an author might just ask somebody, and there is no sign that these authors did.

In fact, though Rapoport remarks on "the paucity of psychological knowledge among strategic thinkers" and says there is little evidence "that either the empirical findings or the insights of social scientists find important application in strategic thinking," and though Deutsch in his introduction agrees that the ignorance of psychology among the strategists is "appalling," I am not quite ready to accept Rapoport's book as a model of empirical research, even if he is permanently associated with the Mental Health Research Institute of the University of Michigan. He asserts that the "strategists are also exactly like other people of their social class, education, and background," but cites no evidence and does not indicate how he drew his sample or even if the seven named are his sample; he names some philosophers and novelists he is sure they do not read and some books they do not like (pp. 170-71), but did not ask them; he adverts to "decisive evidence" in their preoccupation with gaming, a preoccupation of which there is no hint in the cited works of four of his seven, but about which it would have been easy to inquire; he hints at a lack of combat experience but gives no indication he checked war records or draft status; and of course he has written a whole book about their thought processes and what motivates them. No wonder Karl Deutsch hedges his introduction by saying, "He [Rapoport] admits freely that he sometimes has overstated his points in order to ensure them attention." (Incidentally, I read the whole book and noticed no such admission.) I am reminded of Horowitz, who devoted an entire chapter of his book to the question of whether the NCMs ("New Civilian Militarists") should be embargoed or talked to, and came out favoring the strongest possible contact of right-thinking people with the strategists, but who, in writing his book about the habits and views of several strategists whom he identified by name, never once got in touch with them by phone, mail, or interview (and in consequence not only confused their policies but got several of their names wrong). Rapoport cannot be charged with such inconsistency, since he explicitly doubts that it is "worthwhile to try to bridge the chasm between strategic and conscience-inspired thinking." He considers it "imperative to establish avenues of communication between Blacks and Whites, and between East and West, because they all must either learn to live with each other or perish." But "in the case of strategy and conscience," he says,

"I am not sure. Here, I believe, is essential incompatibility, not merely a result of misunderstanding" (p. 195). It is no wonder that he knows so few of them and so little about them.

Since the book is about a style of thought and not policy, it is not surprising that he rarely identifies the bad policies that strategic thinking leads to. There might even be a hope that the wrong thinking would lead to the right answer, and the strategists would be harmless. There is one main policy orientation that he attributes to the strategists, and on this he is correct. They all take seriously the role of military force in deterring military action by foreign countries. They may not take it more seriously than most policy-oriented Americans, but among intellectuals their concentration on the military side of foreign policy is rivaled only by that of the peace movements. But as "policy" this is not very specific. The only specific issue that gets Rapoport's attention is civil defense, and on this I might hazard a little psychology of my own, after the fashion of Rapoport.

First I note that of the seven strategists represented, only four have published anything on civil defense, with the exception of Kissinger who implicitly disparaged it in the book cited. Snyder devoted a few unemotional pages to it in his book and Knorr wrote a chapter on the subject nine years ago that was cautiously favorable to taking the subject seriously. Rapoport seems to think (pp. 86-87, 94-95) that his strategists are civil-defense enthusiasts and in particular that they do not recognize the "provocative" aspect of civil defense or the feedback effect on the arms race. I believe—and this is just taking my turn at amateur psychology—that Rapoport is still twitching from the shock of Herman Kahn's book and supposes that Kahn represents the RAND Corporation, the Air Force, Princeton, Harvard, game theory, and the Kennedy Administration. (He says that "Kahn became the loudest and clearest spokesman of the 'cool young men.' " It saves reading a lot of strategy if you can rely on that.) Actually, the only game theorist I know who in print considers "defenses" innocuous is Kenneth Boulding (*Conflict and Defense*, 1962), who cannot possibly be accused of the sins of the strategists. Boulding said, "Thus, a shield, a city wall, or a bomb shelter are purely defensive armaments: they lessen the capacity of the enemy to harm but can in no way harm him" (pp. 258-59). This was just a slip on Boulding's part, but a slip I believe would not occur with Rapoport's "strategists," who typically *insist* on the interaction of civil defense with enemy responses, not neglect it. In two of the works attributed to Schelling, the implicit and explicit treatment of civil defense is exclusively concerned with the "feedback." And what aroused many people was not Kahn's treating civil defense as innocuous "insurance" but his advocacy of its "provocative" merits; it is easy to disagree with him, but not on grounds that he was silent on the very point one disagrees with!

Actually Rapoport is not quite as wrong about the strategists as I make him sound, even though his evidence is disgraceful, and all he has is a hunch based on a small biased sample. The game-theory hypothesis was not a bad guess, but a wrong one. The research technique is a bad example. There is no treatment of issues, just thought processes. There is occasional misrepre-

sentation of policies, but on the whole so little about policy that it does not matter. And there is a slightly insulting effort to cast a diverse and ill-defined group of individuals as a single type. Nevertheless, there *is* a contemporary style of thought and analysis that deserves to be characterized as "strategic thinking," and whether it is good or bad, it may deserve to be studied. It has been studied, but not by Rapoport. Robert Levine's book, *The Arms Debate* (1963), contains an excellent analysis, not of strategists but of how they reason in print. It misses the point, according to Philip Green (*World Politics*, July 1964), for not being *ad hominem* and thus neglecting some key moral issues, and according to Marcus Raskin (*The New York Review of Books*, Nov. 14, 1963) for not being *ad hominem* and thus neglecting the payroll that determines a man's point of view. What it does, though, it does well. But psychiatric examination of amenable, locally available, live subjects that confines itself to a man's published work, and not all of that, seems to me a lazy clinical practice.

The word "conscience" in the title has an important meaning for Rapoport and arises in his own interest in game theory. He has been fascinated, like many of us, with the situations characterized as "prisoner's dilemma"—the situations in which individual "rational" decisions produce inefficient outcomes. These are the situations in which "cooperative" choices by two participants could make both better off, but neither has incentive to choose "cooperatively," even if the other does. Rapoport is annoyed that a "solution" based on "rational choice" should be inefficient and wants to redefine rationality so that it is collectively rational to make cooperative choices even though individual incentives go the other way. Under a test ban without inspection, he pointed out some years ago, a country may be better off cheating if the other cheats and better off cheating if the other does not, but in the end worse off with both cheating than if both abstained. Conscience—a sense of honesty or obligation—could lead each party to eschew his individual interest with the happy result that virtue brought its own reward. (Less reward than if one cheated against a virtuous partner, but nevertheless a joint reward compared with joint cheating.)

It is hard to tell whether or not he really has an argument with those who call the noncooperative and unhappy outcome the "rational solution." I prefer to emphasize that there *are* situations in which individual rationality leads to inefficient outcomes and in which there is no sense of obligation to bring about cooperation; I prefer it because recognizing such situations raises a universal and massively important problem of social structure. Rousseau's *Social Contract* is an illustration of the need to structure obligations—through conscience if one can create conscience, otherwise through damage suits, hostages, bail, inspection schemes, synchronization of choices, or anything else—to eliminate the subversive incentives, changing the payoffs or adding new strategies. Conscience is great if it will do the trick; and it must account for an enormous amount of efficiency in social and economic arrangements where the purely material incentives could be quite disruptive. It may even explain why people vote on election day. Like Voltaire, who preferred God-fearing servants, we can attach great utilitarian value to the

implicit sense of social contract—of honesty, decency, and identification with each other—that we are brought up with. (This is in addition to the attractiveness of conscience and honesty as human qualities, but an important addition.)

Rapoport wants to derive a justification for conscience out of game theory itself, making conscience a universal part of the solution to games of the prisoner-dilemma type. As a theorist I disagree with the apotheosis of conscience; there are too many cases in which society depends on inefficient results. The Mafia have solved the prisoner's dilemma efficiently and are the better terrorists for it, as did the Secret Army Organization in Algeria, and so do the purveyor of mail-order pornography and his juvenile customers who have a serious problem of "trust" to surmount. The social value of efficiency, or its esthetic attraction, depends on who is being efficient about what.

Rapoport might agree with me as a theorist but, on matters of war and peace, he does not believe in disinterested research. He sees experimental probings as "actions which *in themselves* can induce a reorientation in thinking about international relations, that is, actions which bring about changes in the political climate." He is interested in the impact that "research *as a social activity* can have on the society which supports it" (italics his, pp 173, 174). If we start asking different questions, he says, we become different people. This explains his preference for a new definition of "rationality," one that is collective rather than individual, conscientious rather than strategic, and his strong belief that strategic thinking is bad no matter how good it is. Seduction lurks, he says, in the mental habit of rational analysis, "for this analysis requires detachment," and detachment leads one "to ignore human characteristics when dealing with human beings" (p. 109).

For sheer stimulation there are few social scientists whose work I have found more rewarding than Rapoport's. For that reason my disappointment in the present book is the more poignant. I am not a disinterested reviewer.

THOMAS C. SCHELLING

*Harvard University*

*The Nation's Economic Objectives.* Edited by EDGAR O. EDWARDS. Chicago: University of Chicago Press, 1964. Pp. ix, 167. \$4.95.

On the occasion of its fiftieth anniversary, in 1962-63, Rice University presented a series of public lectures on national economic issues by eight distinguished economists. *The Nation's Economic Objectives* is based on these lectures. Rice University and Edgar O. Edwards, editor of the volume, deserve congratulations for offering articles, not previously published, by these authors on these subjects:

Kenneth Boulding	The Dimensions of Economic Freedom
Arthur F. Burns	Economics and our Public Policy of Full Employment
Lester V. Chandler	Economic Stability
Seymour E. Harris	U. S. Welfare Programs and Policies
Simon Kuznets	Notes on the Pattern of U. S. Economic Growth

Fritz Machlup	International Economic Cooperation
Edward S. Mason	Objectives of a Mature Society
Jacob Viner	The United States as a "Welfare State"

This book will be a source of pride and pleasure to economists. We can be proud of the demonstration that some economists have something important to say to the public and can say it in an understandable and interesting way. We can enjoy the pleasure of meeting distinguished scholars, and in some cases old friends, almost face-to-face. The styles and personalities of the authors come through in these essays with amazing fidelity.

Of course, one can wish that some things were different. To devote only one of eight essays (or one-fifth of the total pages) to international questions seems to me unbalanced in today's world. Fritz Machlup's essay on international economic cooperation is a model of how much information, classification, and analysis can be compressed in 33 pages. He makes the significant and, to me, novel observation that international economic cooperation, so generally regarded as a "good" thing, has been throughout history an instrument of protection and exclusion, and is still so today to a considerable degree. Nevertheless, the volume would have benefited from more pages on our economic objectives in relation to the rest of the world, even if this had required omitting some of the pages on other subjects now included. Also, one wishes that Arthur Burns's talent and experience had been directed to a more fruitful subject than whether or not our national objective is zero unemployment, and that Seymour Harris had left his shower of statistics behind and given us his mature reflections on welfare programs. Still, what is here is well worth the price of admission.

Since space is too limited for comment on the eight essays singly, I hope that the reader will pardon the special interest of a former student in Professor Viner's essay on "The United States as a 'Welfare State.'" This is genuine Viner—urbane, sharp, and energetic. It contains this blockbuster of a sentence:

If, therefore, I nevertheless conclude that the welfare state, like Old Siwash, is really worth fighting for and even dying for as compared to any rival system, it is because, despite its imperfections in theory and practice, in the aggregate it provides more promise of preserving and enlarging human freedoms, temporal prosperity, the extinction of mass misery, and the dignity of man and his moral improvement than any other social system which has previously prevailed, which prevails elsewhere today, or which, outside utopia, the mind of man has been able to provide a blueprint for.

Some of our new federal office buildings in Washington are big enough to accommodate this inscription in one unbroken line on their lintels. But before this sentence is chiselled on many buildings, Viner's text should be carefully studied. He defines "welfare state" broadly, to include not only all the varieties of combinations of social and economic policies found in non-Communist advanced countries but also almost all the different policies seriously considered in those countries. There is no implication that we have

here in the United States that one state among all possible welfare states that is best for us. There is no suggestion whether we are on the too "welfarish" or the too "unwelfarish" side of the optimum. Thus Viner leaves ample room for the policy debate that is the bread and butter of economic discussion. What he excludes is the aggrandizement of that debate into a conflict among rival systems.

The prevailing mood of the eight essays is satisfaction. The authors do identify some problems about the U. S. economy, and there are some warnings against complacency. But the problems are not alarming or fundamental—the urban traffic problem is probably a typical example—and the warnings against complacency are required only because the general argument tends towards complacency. The theme of the volume may be in Viner's striking quotation from Robert Frost:

... How are we to write  
the Russian novel in America  
As long as life goes on so unterribly?  
[New Hampshire (1923)]

Certainly these eight professors of the dismal science have not written the Russian novel about the American economy.

I share this contented view, and yet I worry about it. Note that the quotation is from New Hampshire, not from Harlem or from Mississippi, and that the year was 1923, the year before James Baldwin was born. Perhaps the Russian novel is still to be written here. These papers give little hint of the concern, or crisis, about the related problems of poverty and race in America that was already boiling up when the lectures were delivered. Neither do they give much hint of America's possible involvement in the problems of poverty and race that exist in the "outside world." Probably it did not fall within the special competence or assignment of any one of the authors to supply this, but the reader should be aware that something may be missing.

HERBERT STEIN

*Committee for Economic Development*

*Political Economy*. Vol. 1, *General Problems*. By OSKAR LANGE. Translated from Polish by H. H. Walker. Oxford: Pergamon Press; Macmillan, distributor, 1963. Pp. xiv, 355. \$6.50.

This volume of Professor Lange's most recent major work first appeared in Polish in 1959 (it met with instant success) and has already been translated into French and Italian. It is the first installment of a three-volume work that attempts, in the light of the Marxian conception of political economy, to synthesize the author's studies in the various fields of economics. The seven chapters deal with general problems of political economy and fall into four parts. First, a philosophical part discusses the subject matter of political economy, the connection between political economy and the materialistic interpretation of history, and the economic laws. The second part is methodological, the third deals with application and economic prediction, and the last is a critical evaluation of non-Marxian trends in political econ-

omy. The next volume will be "a critical exposition of the most fundamental problems of political economy" (the theory of reproduction and accumulation, market production and the law of value). The third volume will be devoted to "a detailed analysis of the mode of operations and the 'law of motion' of the capitalist and socialist formations."

The volume under review discloses the *alter ego* of Professor Lange: the missionary. The author had at long last Truth revealed to him, and he intends to break the news to the world. Sample section headings from the last chapter: "Full development of political economy is only possible in connection with the labor movement" (p. 319); "Only the working class is interested in full scientific knowledge of economic laws" (p. 344); "Union with scientific socialism is the indispensable basis of the further development of economic science" (p. 340).

A by-product of having known truth is the discovery of the "true social welfare function." The principle of economic rationality under capitalism is subject to the private ownership of the means of production and thus involves only maximization of private profit and operates antagonistically (p. 173). The result is social diseconomies, waste of productive resources, unemployment, economic crises, etc. On the other hand, the social ownership of the means of production and the development of *social* economic rationality guarantee under socialism the maximization from the point of view of the economy as a whole.

The integration of the aims of the activity of socialist enterprises by a common end established in the social economic plan leads to a hierarchic structure of ends. . . . A hierarchic structure of ends is a peculiarity of the socialist mode of production just as a peculiarity of the capitalist mode of production is the existence of independent parallel aims of individual enterprises: their attempts to obtain the maximization of their profits. For the hierarchic structure of ends is the expression of the social economic plan and the integration of the aims of individual socialist enterprises by a chief end laid down in the social economic plan. It is at the same time the expression of the social rationality of the socialist mode of production (pp. 179-80).

Where does this leave us in terms of the discussion that Lange started with von Mises more than 25 years ago?<sup>1</sup> He then thought that to solve the economic problem three data are needed: (a) a preference scale which guides the acts of choice, (b) knowledge of the "terms on which alternatives are offered," and (c) knowledge of the amount of resources available. His preoccupation then was with (b), and he proceeded to show that the formal resource allocation is similar under both capitalism and socialism. If all conditions were satisfied, profit maximization under capitalism would have been *utility maximization*. Since all conditions are not satisfied, however, capitalism may proceed by adding, in the Pigovian way, some rules to internalize certain costs—be it progressive taxation, nationalization, fair trade commissions, etc. Lange then saw a case for socialism only on the basis of the

<sup>1</sup> Oskar Lange and Fred M. Taylor, *On the Economic Theory of Socialism*, (B. E. Lippincott, ed.), Minneapolis 1938.

*actual* resource allocation. Capitalism could certainly handle externalities by adding some constraints and equations to the system. Only socialism could do that directly. Or, as Dickinson put it, "socialism would work, as it were, in a glass house, in which all the details of its mechanism could be followed."

All this is past history. Today Lange not only has second thoughts on capitalism's knowledge of the alternative cost of resources, but he also has changed his mind about the nature of the objective function. He actually substitutes the holistic for the private welfare function.<sup>2</sup> Socialism maximizes a function that (by revelation) embodies the thinking of the whole or the Hegelian "collective spirit."

The rest should come as no surprise to the reader. Capitalism is matched to the "true" yardstick and flunks the test. We can present here only a few of the crimes that have been committed in the process.

A suitable (mis)interpretation of rationality helps if one is to show "the limited and socially warped character of private capitalist rationality" (p. 264). The conclusion could have been easily avoided if one interpreted rationality as consistent choice or transitivity of preferences rather than connecting it with deliberation or reflection. In the former sense rationality is a necessary and sufficient condition for maximization in economic activity, and it applies not only to the business world (as Lange admits) but also to household activity (which he denies). Thus, Lange holds capitalism responsible for the "derationalization" of household activity in the sense that consumers are cajoled to buy on impulse, habit, emulation, etc.

After Marx, Lange's economic laws are apocalyptical and thus are "verified" rather than confirmed or rejected (pp. 125 ff.). They are also supposed to be objective and independent from human will and consciousness. To this reviewer they appear to be dependent on the conditions of substitution between factors of production ("technical laws of production," i.e., the technical production coefficients), meaningless truisms ("balance laws of production," e.g., "no more coal can be used than can be produced plus or minus imports or exports"), or hypotheses with missing or confused necessary and sufficient conditions ("laws of interplay of human actions," e.g., the law of the disappearance of surplus profit).

The results of the capitalism-socialism matching game, although predictable, are not without interest. Some sample quotations will illustrate: "The methods of social accounting especially input-output analysis find their full application . . . only within the framework of the socialist mode of production" (p. 186); economic crises "are the result of the limited, private character and antagonistic operation of the principle of economic rationality when applied in capitalist enterprise" (p. 176); "A considerable proportion of our knowledge of programming is not used in capitalist enterprises or capitalist states because its application demands a coordination of the activities of individual enterprises, which is only possible when the means of production are socially owned" (p. 204). It is one thing to state that capitalism cannot do the job and quite another to say that at the present state it does not.

<sup>2</sup> K. R. Popper, *The Poverty of Historicism*, London 1957, pp. 76 ff.

A few concluding remarks should not be read just as perfunctory pronouncement of professional niceties. The book is certainly worth reading, notwithstanding the fact that this reviewer devoted his space to volleying at the author. The philosophical part presents a nice elaboration of Marx's ideal types. The discussion of political economy and praxiology, of marginal calculus, of programming, and a short appendix on its mathematical foundations still allow the reader a distant glimpse of the author of *Econometrics*. The attempt to write a political economy of socialism alone, given the present state of the art, is certainly a contribution. One suspects that the best is still to come in the next two volumes.

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*A Strategy of Decision—Policy Evaluation as a Social Process.* By DAVID BRAYBROOKE AND CHARLES E. LINDBLOM. New York: Free Press of Glencoe; London: Collier Macmillan, Ltd., 1963. Pp. ix, 268. \$5.95.

This is a book meant for "analysts" whose task consists of the evaluation of measures of economic policy. It recommends a "strategy" standing at variance with what the authors call "synoptic" methods, the best of which are characterized by the use of a social welfare function which is to be maximized. They charge that the latter method is impracticable and that a social welfare function has never been constructed. The method they defend is called "the strategy" or the method of "disjointed incrementalism." It consists of an evaluation of some alternative policies, usually covering only a portion of "total" policy—hence "disjointed"—and aiming at relatively small changes of the total order—hence "incrementalism." For their strategy they claim that it simplifies the analysts' task, first of all through "omission," that is, by not considering all conceivable alternatives, but only a few feasible ones; then, because it only uses the restricted knowledge available and capabilities of the people in charge of the work, no "supermen" are needed. It considers only such policies as are attractive also from the "means" side; the desired simplification also materializes through "fragmentation" by "serial" and "remedial" steps or "reconstructive features." Under the heading "omission," as a simplifying feature they also mention the possible omission of even "important consequences" of the policy measure to be analyzed. They justify it by a possible later remedial step, making up for the forgotten consequences; in the reviewer's vocabulary this is an example of the good old "trial and error" method. They describe their strategy as "meliorative" rather than "peremptory" and also characterize it as "openminded" and "flexible." Finally they feel that the strategy contributes to the solution of some ethical problems.

The reviewer's impression is that the authors are well versed in practical and detailed policy evaluation, and they rightly emphasize that in such work perfectionism is a bad thing. They are fair and modest in their claims, and many features of their "strategy" are typical for much project evaluation in development planning. In their zeal to defend their attitude they introduce lots of terms (several of them to be found in the previous paragraph of this

review in quotes). To the reviewer several of these features look like duplications; is "fragmentation" very different from "disjointed," "serial" from "incremental," "openminded" from "flexible," or is not openmindedness a feature of the analyst rather than of the strategy? The word strategy appears somewhat ambitious, by the way, in the light of the present-day, very precise meaning of this term in more perfectionistic procedures.

The reviewer also wonders whether the controversy created by the authors is somewhat imaginary in that the "synoptic" method is used for a different type of problem, mainly for the macro-stage in policy planning, where some of the main interdependencies in the economy are at stake, or for welfare-economic propositions of a very general character, meant to find the basic features of the optimal order. For the macro-stage it is characteristic that the various means or instruments used can be combined in many different ways, whereas in projects or single policy measures or schemes the means often are complementary, and the combination can be taken or left. The authors are aware that they do not always go on until the maximum of welfare has been reached, but only take a step in the right direction. This is true also for the use of a set of given projects in a development policy, but it may actually be a wrong policy, to the extent, for instance, that some good project could be carried out several times. In other words, the quantity of such projects should be considered to be unknown—a feature of mathematical programming which makes this technique really superior to that of project selection with each project considered as an individual and unique "possibility."

The reviewer is somewhat puzzled by what the authors may have in mind when they claim a contribution to the solution of ethical problems. Not being an expert on ethics, he may be wrong, but he feels that neither ethics as it is nor this book helps the economist much in solving ethical problem number one for welfare economics, namely the comparison of utilities of different persons. Here they simply remain on the unsatisfactory, agnostic level of judgment still *en vogue* in present-day economics and leave all the ethics to the politicians and the pressure groups.

While the reviewer considers the book a sound presentation of the practice of evaluation (of both policy measures and projects), he feels the authors missed the possibility of building a bridge to the methods they criticize. It is not quite true, by the way, that no social welfare functions have been constructed; Van Eijk, Frisch, Sandee and Theil may be quoted to the contrary. The point is that the synoptic method is the only one for some other classes of problems, already mentioned. The book under review has the merit, however, of reminding the reader or producer of macro-stuff of the innumerable qualitative and quantitative variables playing a role in the micro-phase of policy-making.

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*Modern Economics.* By STANLEY P. WRONSKI, FRANCIS S. DOODY AND RICHARD V. CLEMENCE. Boston: Allyn and Bacon, Inc., 1964. Pp. viii, 438. \$4.84.

This book is designed to meet an often-expressed need for a good high school textbook in economics. How well it meets this need is another question. But before proceeding to that question, perhaps it would be pertinent to ask whether what is demanded of such a text is in fact possible. Clearly such a book must do more than duplicate between hard covers the level of economics already available in newspaper editorials and magazine articles. Yet it cannot exceed the capabilities of high school students or—a more severe practical limitation—the level of difficulty which the public schools are prepared to impose on them. But even more important, given the potentially controversial nature of the material and the vigilance of interested groups, how is such a book to carry out the demolition of popular myths and the wholesale slaughter of sacred cows—which real education necessarily entails—and still expect acceptance in the public high schools? Obviously a ‘Cook’s tour of economic miscellany is much easier and safer, and that is what is too often provided. This book is no exception.

Judged for what it is, rather than what it might have been, *Modern Economics* has its merits. It is largely free of explicit moralizing and the bias of implicit value judgments, which have been severely criticized in other high school texts. Its theoretical sections are sufficiently well done that there need be no fear that the student will have to unlearn them in college. Unfortunately, the analytical segments of the book are very small, and no relationship is established between them and the numerous topics discussed. For example, supply-and-demand analysis is introduced early in the book but is totally ignored in later discussions of the farm problem, minimum wage laws, and public utility regulation. Similarly, fixed and variable costs are carefully distinguished, but no use of this distinction is made, even though there is a chapter on international trade which might have mentioned its relevance to dumping, and a chapter on government regulation which could have shown its importance in “natural monopolies.”

While there are many topics, there are few issues. Not only are controversies avoided, the very fact that there are controversies is largely ignored. Even the chapter on the role of government in the economy presents an unruffled surface simply by discussing particular government programs in terms of their avowed purposes and institutional mechanics, rather than in terms of their results or the issues they raise. It can hardly be claimed that this is preparation for informed citizenship.

As so often happens, analytical weakness is accompanied by excessive factual detail: the intricacies of labor arbitration procedures, detailed comparisons of various forms of life insurance, a list of retail prices in the Soviet Union, etc. Numerous photographs are included—no doubt a desirable feature at the high school level—but the particular pictures chosen are mediocre and insipid—as is the text too often. Some captions: “Planned saving made this family vacation possible”; “It is essential that this man study

carefully the insurance policy that he is buying"; "The search for oil is a never-ending venture that takes men to unlikely places."

There has long been a crying need for an intelligent citizen's guide to economic thinking. There still is.

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**Price and Allocation Theory; Income and Employment Theory;  
Related Empirical Studies; History of Economic Thought**

*The Theory of General Economic Equilibrium.* By R. E. KUENNE. Princeton: Princeton University Press, 1963. Pp. xv, 590. \$12.50.

This book affords a rather comprehensive review of the main body of general equilibrium theory from Walras to Arrow-Debreu. The fact that much of the material is presented nearly in the form in which it first appeared lends a historical flavor to the book. There is also a good deal of exegesis of the classics, especially Walras, and some attention to doctrinal controversy, especially in the theories of capital and money. The scope of the work is thus quite ambitious, although Part II, which is over half the book, is parallel in its development to the main chapters of Walras' *Elements*. It is an elaboration of Walras with the help, primarily, of the work of Hicks as presented in *Value and Capital*, but with the help, also, of Wickcell and Fisher on investment and of Lange and Patinkin on money.

The methodological viewpoint of Part II is that clearly put by Hicks and Samuelson, the development of comparative statics from an assumption that the model has certain stability properties. Thus quantitative results are sought, such as the direction of price change when the supply of a good is reduced, when none but very general properties of the model are used. The author asks that the theorems have operational meaning in the sense of Samuelson. Of course, he is led to observe that very few results are forthcoming in a general model. Indeed, this discovery is made many times and tends to produce a feeling of futility, so that one begins to wonder why the author bothered to write his book.

A weak defense of general equilibrium analysis is raised by introducing the value of the "vision" of the economic process which it embodies. I am not clear how this vision is effective, but in any case the author is rather apologetic about this excuse for his enterprise as well. I find his pessimism a bit surprising since it seems clear that nearly all economics nowadays is general equilibrium economics or closely allied to it. Certainly the theories of international trade, of money, of employment, and of economic growth are general equilibrium theories, in however simplified form, and are not thought by most to be entirely useless. Also the modern approach to the theory of planning is an outgrowth of general equilibrium theory. Even the emendation (or rediscovery) of Marshall's partial equilibrium theory by Friedman seems aimed chiefly at putting it in a general equilibrium setting. No doubt the usefulness of these simplified versions of the general model comes

partly from our willingness to introduce numerical quantities for the parameters or at least to hazard orders of magnitude for them. Also, the applied models are macroeconomic, and the Walrasian model is microeconomic, but this should not obscure the dependence of the former on the latter, logically, and often in the development of thought as well.

There is, I think, a major omission from the topics in general equilibrium which are discussed. This is welfare economics or, perhaps better, the theory of Pareto optimality. I think that Walras was groping for a valid optimality theory, just as he sought to prove existence, uniqueness, and stability of equilibrium. He did not succeed, but he should be commended for retaining his abortive efforts, which have now led to a valid theory. The efficiency and optimality interpretations of the price system of competitive equilibrium were brought out clearly in Koopmans' activity analysis, and in Arrow's chapter on welfare economics in the *Second Berkeley Symposium* [1]. Unfortunately, in the present book, the transition from activity analysis to competitive equilibrium and the role of the efficiency prices is not given proper emphasis, and Arrow's contribution is omitted altogether. Of course, there is much talk of shadow prices, but the context is usually linear programming for profit maximization in the firm. The absence of the efficiency interpretation of the price system is severely felt in the treatment of capital theory. The discussion could have been clarified by appealing to the fact that an efficient program of production over time or a Pareto optimal program of production and consumption over time *implies* a price system, intrinsic to the program, from which the interest rates are derivative. I would think this body of theory on the implications of Pareto optimality is at least as important as any other part of general equilibrium theory.

There are a number of inflated passages which may irritate many readers ("The ubiquity of variation, manifest in the very triviality of its assertion, forces upon man, individually and collectively, a concern for its patterns," p. 3). Also some of the word usage is in doubtful taste ("generalness" for "generality" in several places). The mathematical notation does not improve on Walras, and the use of subscripts is sometimes ambiguous. There are quite a number of places where the mathematical argument is treated casually and becomes quite hard to decipher. On the other hand, the practice in Part II of using small-scale systems with three or four independent variables in which to conduct the argument is quite helpful to the reader, and the exposition of Leontief models and of linear programming is often well done.

I will now review the chapters in turn. Chapter 2, "The Exchange Economy," develops the theory of consumer choice according to Slutsky and Hicks and applies it to the theory of exchange in the manner of Walras and Hicks. There is an unrelated section on Morgenstern-von Neumann utility as well. However, no use is made of the revealed-preference or the minimum-income approaches to demand theory [5]. The methods of proof are those of the Hicks appendix to *Value and Capital*. Theorems are derived chiefly from assuming that income effects cancel out or from using Hicks's stability arguments. The author concludes that the stability argument adds nothing. However, this conclusion seems too severe, since it seems to be based on the fact

that what is derivable from the stability assumptions can also be derived from the maximum conditions when income effects cancel. Of course, the two sets of assumptions need not be treated as alternatives. The Hahn-Negishi theorem on systems in which all goods are gross substitutes is applied to derive the Hicks perfect stability conditions. However, the argument is incomplete (p. 127). Cardinal utility is treated as a meaningful concept even in a context of choice with certainty (p. 94). However, I do not see the justification for this in the absence of a theory that assigns a distinguished role to a set of utility functions, invariant under linear transformations.

In Chapter 3, "The Production Economy," this line of reasoning is continued, again in the manner of Walras and Hicks, with, as before, a considerable amount of historical annotation. Successive models of the firm of increasing generality are examined and embedded in successive Walrasian general equilibrium models. There is also a rather nice excursus on linear programming. A zero profit condition is imposed even when the individual firm does not enjoy constant returns to scale (p. 179). This is clearly illegitimate in an exact model and, indeed, causes the model to have an excess of equations. Model III-3, and thus later models as well, have  $\sigma$  more equations than variables (p. 179). In this case counting equations and variables could have paid off, but alas they are counted wrongly. The Hicks stability conditions are again used in a brief passage to support the same comparative statics theorems as for the exchange model. The assumption of canceling income effects is also applied as earlier. This is all as in Hicks. There is a cursory treatment of decisions under uncertainty.

Chapter 4 introduces problems of investment and planning over time. The models are closest to the Walrasian statement. Only stationary equilibria are dealt with, but a finite horizon and a succession of markets are explicitly introduced. However, quite special assumptions on the generation of expectations are needed whose import is not clear. For example, expected prices are not influenced within a given market day, and each consumer plans in each market day to liquidate all his holdings by the horizon. This last assumption seems to imply that the consumer is always revising his plans, although he is never surprised by developments (p. 212). The author rejects Friedman's criticism of Walras on saving (p. 218), so far as I can see, correctly.

A large part of this chapter is devoted to raking over the coals of ancient controversy with, I should say, doubtful profit, while the basic paper of Malinvaud [6] is nowhere mentioned. Surely much of the obscurity of capital and interest theory can be dissipated by treating present and future prices as implicit in an efficient program of production and distribution in the way Malinvaud did. This view would have been especially valuable in considering Schumpeter's arguments on zero interest in the stationary state. There is an intriguing final section of Böhm-Bawerk's arguments for the technical superiority of present goods. A closely related question has been formulated and nicely settled recently by David Gale. He shows that processes which are shorter but enjoy the same output levels result in higher von Neumann growth rates in a model where reinvestment occurs [3]. Psychological discount is defined in terms of goods rather than a one-period utility function (p. 231),

although the utility interpretation had been mentioned. Koopmans' work on this topic was presumably too recent to be introduced [4].

The Walrasian edifice is completed in Chapter 5 on the money economy. Here, too, much of the chapter is devoted to a review of controversy, this time somewhat more recent, in particular, the controversy following Patinkin's charge that classical monetary theory includes an invalid dichotomy of the general equilibrium system. In the event the author pretty well exonerates Walras of this charge, and, I think, correctly. However, he convicts Walras of another, cruder charge described earlier by Lange, the charge of observing Say's Law in the sense that the demand for increases of money stocks is set identically equal to zero for every consuming unit (p. 320). I confess that I do not think it necessary to take this view if Walras is read sympathetically. The point is, consumers in this model without uncertainty demand assets in general (i.e. "*E*"), not a particular form of asset. But, if there are no new money supplies, in equilibrium the aggregate demand for asset increments must be met by new capital goods. I find the discussion in Walras at this point quite elliptical. I also find it difficult to regard the substitution of a Cambridge quantity equation for Walras' explanation of the demand for the services of money, i.e. cash balances, as an improvement. The author uses Walras' treatment of the market in money services in an effective criticism of Archibald, who has consumers build up cash balances over a sequence of weeks (p. 317).

In Chapter 6, "The Static Linear System," there begins a series of modern developments from the Walrasian system. The Leontief input-output model and activity analysis are introduced here. The general procedure is to use the ideas of linear programming to expound these subjects. The exposition is elementary and rather well done in most places. However, for a book in economics it would be appropriate to describe in more detail the analogy between activity analysis and the action of a competitive market. The consequence of neglecting this analogy is that the crucial role of the price system for efficient decentralization of economic activity is not highlighted. The principal Koopmans result on efficiency price sets is only sketched briefly. On the other hand, the earlier interpretation of the dual of a linear program as imitating the action of competitive markets was too casual to be useful (p. 156).

Models in which space is explicitly introduced are described in Chapter 7. As elsewhere most of the exposition takes the form of a review of models presented by a series of writers, Ohlin, Graham, Isard, Lefebvre, etc. However, the chapter begins with a large-scale Walrasian model embracing the complications of investment, money, and space, albeit in simplified forms. Although it may be useful to display particular parts of the technology explicitly, this elaboration of Walras may be under a misapprehension. The author required that his simple exchange market occupy a point in space. However, it is not clear why this should be. Goods are located in places, but a market is a system of communications between persons and does not, properly speaking, have a place. This is not to say, of course, that special patterns of transport cost or barriers to trade erected by states do not deserve to be studied. Only,

the general model perhaps does not merit restating with explicit, but abstract, distinctions further enriching the subscript notations. In case 6 of spatial models (p. 448) where consumption is variable, the production point is required to minimize total transport costs. Since consumption varies, it is not easy to see what significance this solution has. Surely something like Samuelson's "net social pay-off" (p. 420) should be maximized. Case 8 is also unclear on this point. I suspect that some of the models in this chapter are summarized too briefly to be understood.

The final two chapters bring us to areas where Walras essayed some guesses but made no real progress, but where recent contributions have been substantial. Chapter 8 is devoted to dynamic models, and Chapter 9 to questions of existence and uniqueness of equilibrium. The dynamic models which are discussed in Chapter 8 include the equilibrium models of von Neumann and Leontief as well as models of multimarket disequilibrium with price adjustment processes at work of the type suggested by Samuelson. Virtually all the discussion is devoted to the statement of results. Very little is offered in the way of proofs. In the case of the von Neumann model it is the balanced-growth equilibrium which is described. There is no reference to the turnpike arguments of Dorfman, Samuelson, and Solow [2] or to any asymptotic properties of other equilibrium paths in such models. However, there is a fairly careful discussion of the difficulties met by the deterministic dynamic Leontief model. In the case of disequilibrium models, most attention is given to the consequences of assuming a stable system, and the results cited are chiefly local results. Less emphasis is placed on the Arrow-Hurwicz theorems in which certain assumptions imply global stability of the system. In particular, none of these theorems is proved.

The author appears to think that the presence of flows between processes in the economy, as described in the Leontief models, creates some difficulty for the von Neumann model, so that one must choose a sufficiently short period to justify adding flows to initial or terminal stocks (pp. 469-70). However, the spirit of the von Neumann model is that the length of the period is not relevant. The processes of the model describe possibilities of transforming stocks at one point of time into stocks at another point of time. These possibilities are well defined, whatever the flows that must occur in between and whatever the time that elapses. What the flows do is to involve a great many stocks in any single process (thus to lend some credence to the von Neumann assumption that every stock is involved in every process).

The dynamic Leontief model is not presented with complete rigor. The work of Solow [9] and Morishima [7], which would have been helpful on this topic, is not mentioned. It is not very clear what the author thinks occurs in a closed, deterministic, and dynamic Leontief model. He seems to suggest that there will be an approach to "optimal" balanced growth, although interruptions may occur along the way when rules governing disinvestment possibilities would be violated (p. 487). However, neither the optimality nor the approach seem defensible. In particular, the discussion on dominant roots is muddled. There is no reason to expect the root with largest real part to be positive, or if it is positive, it may not have a nonnegative character-

istic vector. The relevant matrix is  $[(I - A)^{-1}B]^{-1}$ . If this matrix is irreducible and has two positive roots, the positive root with a positive characteristic vector must be the smaller of the two (p. 483).

A model of dynamic behavior for the individual consumer is also introduced, but some of its features may be seriously questioned. A strategic variable is something corresponding to the Lagrange multiplier when disequilibrium prevails. It is called "marginal preference of income," but it is not defined (p. 490). The variable is needed because it is not assumed that expenditure equals income in disequilibrium. It may be possible to carry such a model through in an intuitively appealing way, but I cannot see that it is done here.

In the discussion of both the consumer and the multimarket disequilibrium models appeals are made directly to the (quite involved) Routhian stability conditions. So far as I can see, these appeals make no contribution. They are used to derive the result that stability implies that the determinant of the differential equation system has the sign of  $(-1)^n$  where  $n$  is the order of the system. However, this is obvious from the fact that this determinant equals the product of the roots, which must have negative real parts for stability.

In general, the literature of the three or four years prior to the date (June, 1962) of the preface is not used in the book. An example of the hiatus is the view that competitive exchange models may prove to be stable in all cases (p. 508) despite the publication of Scarf's counterexample in 1960 [8].

The final chapter considers the rather esoteric theorems proved in the mid-fifties on existence of equilibrium. By far the most elaborate treatment is given to the Arrow-Debreu article of 1954. Indeed, this is much the most meticulous examination of any substantial mathematical proof in the book. Moreover, most of it is well done. On the other hand, the other existence arguments (chiefly mine and DOSSO's [2]) are not clearly presented. Indeed, I fear they may be incomprehensible ("consider the set  $Y'$  defined as  $\phi([X'])$  such that when  $[X']$  lies on the boundary of feasible production,  $[Y'] = [X']$ ," p. 560).

It is said that the Arrow-Debreu Model 2 rules out fixed coefficients of production (p. 522). This is a misinterpretation. It is even possible to have universal fixed proportions in this model if labor services always have some direct utility. Also it is not true that the economic import of convexity derives only from its implying nonincreasing returns to scale (p. 524). A second important consequence is divisibility of goods. Also the nonnegativity of the price vectors is based on free disposal for goods, not just free disposal of factor services (p. 535). Finally, I do not assume in my model that consumers hold zero initial stocks of goods (p. 554). Of course, slips like these are bound to occur in so bulky a compendium. I mention them less to blame the author, than to assist the reader.

There are some errors of historical reporting in this chapter. Along with many other people, the author evidently thinks Wald only used his assumption of the weak axiom of revealed preference to get uniqueness in his existence theorem (p. 520). This is incorrect. The axiom is used also in the deriva-

tion of the existence result in the production model. Since this axiom very nearly eliminates the social feature of the demand functions, a much larger gap is left between his theorem and the recent theorems than otherwise would be the case. In Wald's theorem on exchange equilibrium greater generality is achieved, but quite special assumptions like independence of the utilities of different goods are still needed.

In commenting on the generalizations of my Graham-model theorem, the author implies that this argument is also restricted to a one-consumer economy (p. 564). This, of course, is not true. The demand function can be anything you like, so long as it is continuous and observes Walras' law. Indeed, the further statement that my proof is a special case of that of Arrow-Debreu is also wrong, since Arrow-Debreu only deal with demand functions derived from utility maximization, while mine does not require that choices be consistent, and, of course, the proofs themselves are along entirely different lines.

It may be worth while to say a word more about the method of equation-counting to lend plausibility to a general equilibrium model. This ritual is performed many times, indeed, after every statement of a model in Part II. However, this procedure does not correspond to the structure of competitive equilibrium, and in any case it is entirely indecisive. A competitive equilibrium is defined in terms of maximization of utility by consumers, maximization of profit by firms, and equality of supply and demand in the presence of a list of market prices for all goods. The basic variables are the prices. The only intrinsic set of equations are those of supply and demand, although even here it is preferable to speak in other terms. To a list of prices there corresponds a set of demand vectors, any one of which may be distributed to consumers so that no consumer can increase his utility by trading at the market prices. Call this set the demand set. Correspondingly, there is a supply set which is consistent with profit maximization by firms at these prices. Then we mean by equality of supply and demand that the intersection of the demand set and the supply set is not empty.

Whether, for example, it is possible to write down a set of equations whose solutions are the points of the demand set is quite irrelevant to the problem of whether the demand set is empty. This question should be decided prior to the attempt to construct an equation system to help in locating the set. In general, of course, no such equation system will exist, unless in some trivial sense that requires the solution to be known. But if it does, the appropriate move is to consider the plausibility of the subset of equations corresponding to an individual consumer's maximization problem in isolation from the rest of the system.

The problem on the production side is equally illuminating. It is best realized in a linear-activities model of production, which is also closest to the model of Walras. Here the maximization problem is expressed by a set of equalities and inequalities. Moreover, the inequalities are intrinsic and cannot be eliminated. These basic inequalities are, first, the nonnegativity of activity levels for the production processes, and, second, the nonnegativity of profits on all processes. There is no way of converting them into equalities without creating new inequalities. This is analogous to the presence, even

in simple interior maxima, of second-order conditions which must appear as inequalities. However the inequalities here have more than a local significance.

Thus in considering whether the system is soluble, the structure of the competitive equilibrium, implicit in its definition, suggests that one should first determine that the demand set and the supply set are well defined over some set of price vectors, and then see if there will be some price vector in this set which leads to a nonempty intersection of the demand set and the supply set. This is consistent with the approach of Chapter 9, but the methods of this chapter (on existence) and of other parts of the book where linear programming and activity analysis are discussed are not allowed to influence the development of the classical theory. Analysis along these lines would have quickly led to a revision of the system III-3 without one's needing to count equations correctly.

In the system III-3, profit-maximizing firms are subject to a 0-profit condition although they do not have constant returns to scale. This is an attempt to represent an economy of Marshallian industries which are competitive and where profits tend to be eliminated by entry and exit from an industry. The only proper way to build such a model is either to ignore the firms and deal with linear-production processes, as Walras did, or to allow the numbers of firms to vary and thus achieve approximate constant returns to scale for the group of firms of each type. Then the 0-profit condition applies to each type of firm, and the number of firms of each type is a new variable, corresponding exactly to the activity level in the model with linear processes.

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*A Study of Cost and Demand Inflation.* By J. D. PITCHFORD. Contributions to Economic Analysis XXXIII. Amsterdam: North-Holland Publishing Co., 1963. Pp. 167. \$4.20.

The purpose of this book is the examination of cost and demand inflation. To achieve this purpose, J. D. Pitchford has devoted the bulk of its pages to the working out of the implications of several simple models of inflation.

He first develops a "basic model" of an aggregated and closed economy in the very short run. The main characteristics of this model are: (1) Wages depend upon both demand and "costs." The change in wages is a function of lagged excess demand and lagged prices. (2) Prices depend upon both demand and wages. The change in prices is a function of lagged excess demand and lagged wages. (3) A change in prices and wages leads to a less than proportionate change in money aggregate demand. This ensures that the price level is determinate.

In examining the implications of this model, the author develops the concept of income claims. These are the proportions of national income implicit in the markups on costs in the wage and price equations. If the wage and profit claims are consistent (i.e., they do not add up to more than one), the stationary value of the price level will also be an equilibrium one in the sense that excess demand and unemployment will be zero. If the claims are excessive, prices and wages will come to rest only if enough excess supply and unemployment have been created to dampen the cost pressures.

In this model the relative "bargaining power" of wage and profit receivers does influence the share of income each receives. When income claims are inconsistent, labor's share will increase as cost-determined wages become more important or as demand-determined prices become more important.

After nine variants of this basic model (involving different combinations of pure demand, pure cost, and cost and demand wages and prices) are examined, a disaggregated model is constructed. This model has two commodities and two groups of workers. One price and one wage are demand-determined, the other two are cost-determined. Once again excessive income claims result in a stationary price level above the equilibrium level, but deficient demand and unemployment will be concentrated in the cost-determined sectors.

Further elaborations of the aggregative model involve introducing some flexibility in income claims—i.e., making them responsive to aggregate demand and enlarging the model to include foreign trade.

The main contribution of this book is its clarification of the role of cost-plus pricing and cost-of-living wage adjustments in inflation. Pitchford has demonstrated that the existence of such price and wage behavior alone does not create incompatibility between price-level stability and full employment. Whether the income claims are consistent is of crucial importance. In his own words:

If too much attention is devoted to methods of determining prices and wages the paramount issue of excessive or consistent income claims may be

neglected. It is not the type of price fixing that matters, but whether or not the claims of the two recipients can be mutually satisfied (p. 57).

Beyond that, the book is somewhat disappointing. While the dynamic aspects of the inflationary process are not completely ignored, comparisons of stationary positions receive heavy emphasis. The speed of the inflation is surely at least as important as the eventual stationary price level, especially from a public policy standpoint. Since the response of prices to changes in demand will depend upon the extent of cost-determined pricing, emphasizing the effects upon the stationary value of the price level can be misleading.

The whole study is completely divorced from the business cycle, thereby ignoring factors of importance for inflation, such as the cyclical behavior of productivity and the late-recovery profit squeeze. It would be interesting to examine the behavior of Pitchford's model with excessive income claims in a cyclical context.

Since excessive income claims account for the incompatibility of full employment and price stability in his models, the author devotes one chapter to an examination of the sources of these claims. Unfortunately, nothing particularly novel emerges from this discussion. The highly aggregative approach embodied in most of the simple models probably accounts for the inadequate treatment of structural problems arising from excessive sectoral demands and price and wage rigidities. The interactions between product market conditions and labor market conditions which Levinson found to be important are not dealt with at all.

The chapter on policy provides no new recommendations for dealing with cost inflation. However, the author correctly points out that an "incomes policy" necessarily involves implementing a particular income distribution, a point which some supporters of the Council of Economic Advisers' "guideposts" appear to have overlooked.

THOMAS A. WILSON

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*Models of Income Determination.* National Bureau of Economic Research, Studies in Income and Wealth, Vol. 28. Princeton; Princeton University Press, 1964. Pp. ix, 427. \$10.00.

The focus of this income and wealth conference was on models intended to explain major national income components. As seems inevitable with conference proceedings, the papers vary greatly in scope, technical level, and quality.

The most ambitious and comprehensive model is presented in the first paper, "A Postwar Quarterly Model: Description and Applications," by Lawrence Klein. Klein's model contains 29 endogenous variables and is estimated from quarterly data for the years 1953-58. This model is clearly a descendent of earlier models estimated by Klein and his collaborators and improves on earlier work in a number of ways. It is impressive how much he has been able to incorporate the best recent work on production functions, inventory accum-

ulation, etc., in the present model. A major part of Klein's paper is concerned with a detailed analysis of forecasts, completed in April, 1961, for the second and third quarters of 1961. The model correctly predicted the upturn in the second quarter of 1961 and predicted most GNP components with considerable accuracy.

The second paper, "A Forecast Determination of National Product, Employment, and Price Level in Canada from an Econometric Model," by T. M. Brown, gives the first full presentation of the econometric model of the Canadian economy developed at the Department of Trade and Commerce. This is an annual model estimated from the years 1926-41 and 1946-56. The model contains 40 endogenous variables but, as Christ shows in his comments on Brown's paper, can be reduced to eight independent stochastic structural equations in eight endogenous variables. An impressive amount of work has gone into the construction and estimation of the model, but it leaves a strong over-all impression of being less up-to-date than Klein's model in that it incorporates much less of what has been learned from earlier postwar econometric models. Like Klein, Brown focuses on the forecasting accuracy of his model, using the recession year of 1958 as the forecasting period.

The third paper, "Income and Asset Effects on Consumption: Aggregate and Cross Section," by Jean Crockett, is a study of the determinants of household expenditure patterns. Attention is focused on three sets of considerations: (1) separating the effects of permanent and transitory components of income; (2) measuring the interaction effects of income and assets; and (3) taking account of different tastes that presumably exist among different families. The paper contains a wealth of suggestive results obtained from cross-sectional and time-series data. The thorniest problems arise under (3), in trying to measure the degree of thriftiness of different families. Causation does not flow uniformly from income to saving and consumption. Instead, income, consumption, and the forms of asset accumulation should all be endogenous variables in a model that contains the demographic, economic, and sociological variables that measure tastes for and availability of those endogenous variables.

The next paper, "Capital Expenditures, Profits, and the Acceleration Principle," by Robert Eisner, is a contribution to the empirical estimation of accelerator models of capital investment decisions. Eisner makes use of his well-known distributed lag investment function and employs a large sample of capital expenditure data collected by the McGraw-Hill Publishing Company. Eisner's model performs well with these data in comparison with several alternatives, but the multiple correlation coefficients are distressingly small. A notable feature of Eisner's paper is his discussion of the relation between accelerator and profit-maximization models. The discussion between Bert Hickman and Eisner is enlightening on this point.

"Determinants of Inventory Investment," by Michael Lovell, is a thorough and useful survey of the extensive research now available on the determinants of inventory investment. Attention is rightly focused on the role of sales expectations in the making of production and inventory decisions, and

Lovell evaluates the major contributions to this problem, including his own important work.

In "The Federal Sector in National Income Models," Wilfred Lewis, Jr. discusses a variety of topics related to the federal sector. Relationships are estimated between corporate income tax receipts and corporate profits, between personal income tax receipts and disposable income, between excise tax receipts and GNP, and between social insurance contributions and wage and salary payments. Projections of total receipts in the federal sector are made to 1970. Several projections of federal expenditures are compared and evaluated. The theoretical discussion of the determinants of federal spending, based on the "displacement effect" of wars, is unrevealing. Some of Lewis' comments, especially on page 248, sound dated in view of the 1964 tax cut. Finally, Lewis attempts to measure the effects of the built-in stabilizers in the federal fiscal system. Hickman shows in his comment that Lewis' measures are marred by biased estimates of marginal consumption propensities.

"Short-Run Forecasting Models Incorporating Anticipatory Data," by Irwin Friend and Robert Jones, is an attempt to persuade the reader that short-term forecasting is better attempted with a carefully chosen small-scale model than with one of the large-scale models that have been presented to the profession. Their evidence in support of this proposition is a comparison between the forecasting properties of their four-equation model and those of several larger models, in particular Klein's quarterly model discussed above. Their argument makes frequent use of innuendo (for example, their false contrast on page 280 between models which concentrate on a small number of basic variables and those that contain a large number of marginal variables). Furthermore, their substantive procedure is inadequate. They compare their *ex post* estimates and forecasts with Klein's *ex ante* estimates and forecasts. Neither we nor, possibly, the authors know how much their knowledge of events during the forecast period affected their choice of models and estimates. Comparing the forecasting accuracy of econometric models is a subtle and complicated problem which has been studied by Theil and others. Friend and Jones have not done justice to this problem.

"An Empirical Model of the United States Economic Growth: An Exploratory Study in Applied Capital Theory," by Albert Ando presents a disaggregated neoclassical growth model. One consumption good and an arbitrary number of capital goods are assumed to be produced with Cobb-Douglas production functions in each of which all the capital goods appear as arguments. Disembodied technical change is assumed in each sector. The generalized model permits Ando to obtain richer results, particularly regarding price relationships, than is possible in a one-commodity model. The discussion of measures of the aggregate capital stock (pp. 347-54) is particularly interesting. Estimation from data for periods 1900-1928 and 1951-58 is attempted for the special case of one capital good and one consumer good. Although the empiricism appears to be somewhat casual, the results are plausible and suggestive.

In "Notes on the Measurement of Price and Quality Changes," Zvi Gri-

liches surveys and extends his earlier research on the measurement of quality changes, particularly in postwar U.S. automobiles. Price differences at a point in time between cars with different quality combinations permit him to estimate the cost of changes in various qualities such as length, horsepower, etc. Using these estimated quality change costs, he can correct year-to-year price increases for quality improvements. The method is ingenious and appealing, but the ability to identify relevant qualities is crucial for practical application. A strong case can be made in principle for concentrating on performance characteristics (such as the number of trouble-free miles, fuel consumption per mile, etc.) rather than on physical characteristics. Presumably, for example, it is not the weight of the car that provides utility, but the resulting safety, comfort, etc. Some physical characteristics may be good proxies for the more basic performance characteristics, and some may not. Griliches has opened up an area in which a great deal of useful work can and should be done.

One concluding remark may be in order. It is incredible how much of the effort and trouble in short-term forecasting models (including the papers in this volume by Klein, Brown, Lovell, and Friend and Jones) is related to inventories and inventory investment. Perhaps the American Economic Association should offer a \$1,000 prize to the first economist who submits a model that will accurately forecast the inventory component of the national income accounts.

EDWIN S. MILLS

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*Intermediate Price Theory.* By SIDNEY WEINTRAUB. Philadelphia and New York: Chilton Books, Publishers, 1964. Pp. xiv, 378. \$8.00.

*Intermediate Price Theory* is a revision of Sidney Weintraub's book, *Price Theory*, first published in 1949 by the Pitman Publishing Corporation. The intent of the earlier work was to provide a treatment of price theory for relatively advanced students. In the current edition, as suggested by the new title, the avowed purpose is to simplify the exposition and so to serve a wider audience. To this end what was needed was a careful, chapter-by-chapter revision; what is achieved falls short.

Briefly, the revision entails the deletion of a number of chapter sections, the deletion of the bibliography at the end of each chapter, some shuffling of the remaining chapters and sections, the addition of exercises throughout the text, the addition of short expositions of topics of recent interest, and the addition of some exposition of welfare economics. Little rewriting is attempted; a close examination of the original text reveals only minor changes here and there, introduced largely to facilitate the deletions and the shifts in organization in the new text.

More particularly, the bulk of the deletions occurs in reducing Part IV, "Dynamic Analysis," from five chapters to two chapters and, similarly, in reducing four of the chapters in Part III, "Market Behavior of the Firm," to two chapters. Apart from transposing a number of chapter sections, the major change in organization is to locate the exposition of consumer behavior after,

rather than before, chapters on market demand and production and costs. Revealed preference, von Neumann-Morgenstern utility, general equilibrium, input-output analysis, and linear programming are introduced as new topics. Each of these treatments, however, is too brief to be of much service. For example, about three pages each are devoted to expositions and evaluations of revealed preference and von Neumann-Morganstern utility. Similarly, about nine pages are now allotted to sketching the equations of general equilibrium and a few pages each to input-output analysis and linear programming. Of these short digressions, the expositions of input-output analysis and linear programming are hopelessly opaque. More favorably, a concise, reasonably clear exposition of Pareto Optimality in the guise of two-person, two-commodity exchange and production is also added and used subsequently in drawing welfare implications of various types of competition. The net result is that most of the original volume is reproduced unchanged, with additions being negligible except for the exposition of welfare economics.

The extent of the revision, consequently, is minor, and the objective of simplifying the exposition has not been realized. The suitability of the new edition as an intermediate price theory text thus hinges on the content of the original volume.

Several reasons support the contention that a careful, chapter-by-chapter revision was needed to provide a useful intermediate price-theory text. One reason is to be found in the author's intent in 1949 to present an advanced statement of price theory. While the exposition itself is not especially difficult, relying heavily on geometric and literary devices, an effort has been made to canvass much of the literature of the 1930's and 1940's on imperfect competition, and the outcome has been an exposition which treats too many special cases and too many fine points to serve the needs of students with only a limited background in economics. Another important drawback is the author's tendency to intermingle his own personal critique of price theory along with generally accepted reservations. Too often the reasons for criticisms, their degree of consensus, and their force, after taking into account developments in economic theory not discussed in the text, are neglected. Blatant examples are the author's outright biases against general equilibrium analysis and the use of mathematics in economics, discernible in the earlier text but now asserted even more baldly. (Cf., particularly, pp. 155-56 and 162-64.) Only a good background in the literature, not only of the 1930's and 1940's but of earlier and later periods, would enable the student to thread his way through the exposition and to isolate and give due weight to the author's own evaluation of price theory. To increase the difficulty for the student, a number of technical errors and ambiguities and dated modes of exposition have been carried over unchanged from the original volume or introduced here for the first time. Some examples are the following: the definitions of homogeneous and nonhomogeneous production functions are incorrect; the distinction between cardinal and ordinal is never clearly drawn; the short section on instability in Chapter 6 is exceptionally ambiguous; the mistaken impression is given that indifference-curve analysis is limited to choices

between two goods while holding the consumption of other goods constant; substitutes and complements are discussed in several ways, but the definition in terms of the substitution effect of a price change is not mentioned; the inequality condition that price equal or exceed average variable cost is confused with the second-order condition for profit maximization; an incorrect bit of mathematical legerdemain is used to show that the marginal rate of product transformation equals the ratio of marginal products. The conclusion I am forced to draw is that, on a variety of grounds, the new edition is not the book to be offered to an intermediate price-theory student.

EDWARD ZABEL

*The University of Rochester*

*Income: Analysis and Policy.* By MYRON H. ROSS. New York: McGraw-Hill Book Co., 1964. Pp. viii, 431. \$7.50.

This is a textbook whose chief use will be in undergraduate macroeconomic courses just beyond introductory economics. There have already been many other textbooks written to serve this use; no fewer than eleven—those by Ackley, Bailey, Dernburg and McDougall, Greenhut and Jackson, Henderson, Peterson, Powelson, Rosen, Siegel, Sirkin, and Walker—have been reviewed in this journal during and since 1961. And much of the subject matter included in these books is also treated in the numerous recent texts on introductory economics, money and banking, growth, public finance, and business fluctuations. What then is the justification for the appearance of still another book dealing with this same subject matter? What is there about this new book which should induce a teacher to choose it rather than any of its competitors? Are the earlier books so quickly outmoded by the swift onslaught of events and thought? Are they so deficient in exposition? Does each of them include so inappropriate a compendium of topics? Is it so important to provide a wide variety of books from which teachers may choose?

The topical sequence of this text does not differ significantly from many of the others in the field. The first two chapters (11 per cent of the book) define and discuss the concept of income—money, real and psychic—and explain how it is accounted for in the United States. Chapters 3 through 9 (40 per cent) present the accepted Keynesian, classical, and Hicksian neo-Keynesian (part of which Ross unfortunately chooses to label "Klassical") theories of consumption, investment, interest, income, and employment. Chapters 10 through 12 (20 per cent) are primarily concerned with theories of growth, cycles, and inflation. Chapters 13 through 17 (20 per cent) are about the use of monetary and fiscal policy to achieve macroeconomic goals. Chapters 18 through 20 (9 per cent) appear as afterthoughts concerning personal income distribution, international economic relations, and the history of economic ideas other than those Keynesian and classical.

Perhaps the chief merit of the book is its clear exposition. Students will find it easy to grasp quickly most of the points which are made as they are made. There are excellent summaries at the end of each chapter and an above-average consistency of terminology and symbolism throughout. The

expository quality would have been even better if there were set out explicitly somewhere a complete table of symbols and their definitions; as it is, the reader must, for example, frequently ponder and/or search for the meaning of  $t$ . The generally good exposition occasionally breaks down as in the explanation (pp. 86-87) of the inverse relationship between the amount of investment and the expected marginal rate of return on investment (that is, the explanation of the negative slope of the MEC curve). In this explanation there is multiple confusion for the reader partly because Ross, having previously defined  $r$  as the rate of profit, now seems to treat it as the rate of cost of borrowed funds and partly because there is in no place a complete enumeration of the causes of shifts in the curve as opposed to causes of movements along the curve. Another example of poor exposition appears in the discussion of the Duesenberry effect and the short-run or cyclical marginal propensity to consume (pp. 56-57); it is left unclear whether Ross means that in a recession consumption will decrease only slightly relative to the decrease in income because "families are willing to sacrifice savings in order to protect their living standards" or rather that he means that consumption will decrease greatly relative to the decrease in income because "In recession, consumers are generally in a defensive state of mind. . . ."

One may also quibble with Ross about both his sequential order of topics and his exclusion of some topics. As a means of exciting interest among most students, an opening with two chapters of definitions and conceptual abstractions is perhaps not the happiest choice. Even at the intermediate college level it seems more motivationally effective to begin by describing the pressing social problems whose character and solution should provide the *raison d'être* both for the book and for the study of it. And in general the book seems to display too much concern with income itself and too little with those problems—unemployment, inflation, and growth—for the study of which income analysis is merely a tool. Moreover, throughout the book there is too little empiricism and history, and what there is, is too often segregated out and presented as merely incidental. Symptomatic of this de-emphasis of empiricism is the persistent use of hypothetical rather than historical data in numerical illustrations. In addition, and not surprisingly, the book seems to place a disproportionately small emphasis upon programs to increase income, employment, and growth by means other than conventional fiscal and monetary policy. Very little of the current antipoverty program involving such things as retraining, regional redevelopment, and geographical mobility of resources is discussed. Another significant failing is the omission of a discussion of the determinants of state and local government spending in the theoretical chapters dealing with the determinants of aggregate effective demand. These spending units, as well as households and businesses, deserve treatment. It may even be that federal government, whose spending is determined in considerable part by factors other than the desire to achieve full employment, price stability, and growth, should also have been treated in the theoretical chapters.

The adverse criticisms of the previous paragraphs should not be permitted

to obscure the fact that this reviewer regards Ross's text as a good one. Ross obviously knows what he is writing about; his discussions are scholarly and, for the most part, his opinions well balanced. His text achieves a greater degree of integration of material than do most; the reader is considerably helped to see how things fit together. All in all, one can with a clear conscience recommend the book as worthy of consideration for use.

JAMES NORDYKE

*New Mexico State University*

*Rich Man, Poor Man—The Distribution of Income in America.* By HERMAN P. MILLER. New York: Thomas Y. Crowell Co., 1964. Pp. xxi, 260. \$4.95.

This book contains an analysis of factors determining the distribution of income in the United States largely using data obtained in the U.S. Census of 1960. Chapters are devoted to discussions of income classified by color, occupation, education, and wives' work experience. An analysis of the poorest fifth of U.S. families is made by considering the categories of farmers, aged, broken families, nonwhite families, and other groups among the 9.6 million families making less than \$3,000 in 1959. The problems of single males or young single males are not stressed, presumably because the unit examined was the family.

A strong emphasis is placed on the effect of discrimination. The author shows in an important table that the median income of Negroes was 52 per cent of the median income of whites in 1959 and that this ratio was the same as it was in 1949. A classification by state shows that the Negro-white median income ratio actually decreased from 1949 to 1959 in most of the states having large numbers of Negroes. Another table shows that the non-white-white income ratio for males, classified by amount of education, decreases as the amount of education increases. In 1959 the average nonwhite with four years of college could expect to earn less during a lifetime than the white who did not go beyond the eighth grade. One wishes these educational classifications had also been cross-classified by region. A special plea is made in the final chapter for the elimination of discrimination in employment, housing, and education. The author states, however, that the elimination of discrimination would still leave economic hardship since one of every five Negroes grows up in a fatherless home.

The most important thesis of the book is that when one considers inequality of income in the United States, "There has been no appreciable change in income shares for nearly twenty years." The author offers as evidence one table giving the share of total personal family income received by families in each quintile range in the years 1929, 1935, 1941, 1944, and 1961. The percentages by quintile range are the same in 1944 and 1961. However, these results are somewhat puzzling when one compares them to the data of another recent publication, *Trends in the Income of Families and Persons in the United States* by Herman P. Miller. In this publication, the income concept is that used in the Current Population Survey of the Bureau of the Census. Here the published Gini coefficient of income inequality for families de-

creased from .378 in 1947 to .351 in 1957.<sup>1</sup> This is a 7 per cent movement toward a Gini coefficient of zero or perfect equality. The increase in the coefficient from .351 in 1957 to .369 in 1960 is consistent with the increased unemployment rate after 1957. In evaluating the impact of unemployment, the 11 million unrelated individuals not included in families should be considered. A perhaps more adequate distribution is that for males 14 years old and over with income. In this case the published Gini coefficient increases from .413 in 1947 to .438 in 1960.<sup>2</sup>

What is even more important is that when the lowest 20 to 40 per cent of males or families are eliminated from their respective distributions, inequality decreases from 1947 to 1960.<sup>3</sup> Thus, decreasing inequality may have continued in the last 15 years except for low-income groups. This decrease focuses the problem even more on the plight of the low-income group. It is more than a problem that the relative poor are with us. It is, rather, a problem of the share of total income received by the relative poor.

The author is clearly in sympathy with the plight of the poor man. He suggests in general terms that the unemployed must be aided so their training and mobility are strengthened. Cases of individuals with economic problems are described in detail in several parts of the book. The author argues that the individual in a democratic society can demand the chance to develop his talents and to have a floor to his income so as to minimize his suffering.

This easily read book is written at a level where it should have wide circulation in publicizing the problem of income distribution.

LEE SOLTOW

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<sup>1</sup> U.S. Bureau of the Census, *Trends in the Income of Families and Persons in the United States: 1947 to 1960*, Technical Paper No. 8, U.S. Government Printing Office, Washington, D.C. 1963, pp. 36-44.

<sup>2</sup> *Ibid.*, pp. 212-51.

<sup>3</sup> *Ibid.*, Calculations from pp. 36-44, 212-51.

*The Origins of Scientific Economics.* By WILLIAM LETWIN. London: Methuen and Co., 1963. Pp. x, 316.

According to the author, scientific economics is deductive economic theory, operating with a small number of principles and capable of explaining many diverse economic phenomena. This scientific economics did not grow by "mere idle accumulation," but was "invented" in the late seventeenth century. However, it was not a product of deliberate scientific intention—Sir William Petty's claims for his political arithmetic notwithstanding—but rather a by-product of men's efforts to convince others of the desirability of certain economic policies. "The needs of rhetoric brought forth the method of economic theory." As William Letwin says in the preface, his purpose is to show how "practical, and often mercenary objectives led certain men to build a new science, the first social science."

The declining authority of the Church and of theological pronouncements

on economic questions had opened the door to an economics that was independent of accepted religious and ethical considerations. In seventeenth-century England, especially after the Restoration, writers on economic problems confronted an audience that was intensely suspicious of economic advocacy in terms of the general interest. The group of men best suited by experience and inclination to write on economic questions were likely to be men with business interests, and these men were especially vulnerable to the accusation of special pleading and mercenary motives.

How to disarm this seemingly ever-present and intractable skepticism of the reading public was a major problem. Ardent professions of preferring the public over one's private interests could be expected to meet with scornful rejection. Recourse to anonymous publication was frequently practiced, but it was more likely to stimulate than to allay suspicion. Thomas Mun was prominent among the merchants who sought to strengthen their credibility and authority by arguing that the King's interests and the interests of the merchants usually coincide, and hence that the public interest was most likely to be promoted by attending to the counsel of businessmen. The difficulty with this approach was that merchants so frequently disagreed on the policies they recommended and on what they held to be important. Who among them could be trusted, and whose voice was the voice of authority?

The dilemma was resolved as economic writers evolved an impersonal and objective form of argument and analysis. Dudley North best illustrates the new style, and his brief "Discourses"—almost unknown to contemporaries—are the peak of economic thought during the seventeenth century, according to Letwin. It was his younger brother and biographer, Roger North, who called attention to the great differences between the style and temper of Dudley North's economic analysis and that of his merchant colleagues. No one escapes partiality to his own interests, but by building the analysis upon "clear and evident truths" and reasoning from these "principles indisputably true," we can arrive at correct conclusions. A taut deductive system that reaches its conclusions from a simple set of principles was Roger North's prescription for the necessary objectivity with which to dispel the charge of special pleading, and it is this method of analysis that gains recognition as scientific economics.

Not all readers will be prepared to accept the author's definition of scientific economics or the particular explanation he advances for the rise of this scientific economics, but most of them will agree that he has written a stimulating and informative book. He confines himself almost entirely to British experience, and within this area he concentrates his analysis and discussion largely on Child, Barbon, Locke, Collins, Petty, and North. The first two represent the "old style" economics, and the others illustrate the "new style." The book is a happy combination of thorough scholarship and imaginative writing.

MAX GIDEONSE

*Rutgers—The State University*

*Economic Growth—An American Problem.* Edited by PETER M. GUTMANN. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1964. Pp. 181. \$4.50.

This volume in Prentice-Hall's "Modern Economics Issues" series, under the general editorship of Otto Eckstein, is ostensibly "aimed at everyone who is concerned about the pace of American economic growth as compared to that of other countries." The readership target, as thus defined, is blurred; the actual pitch of the book, to the extent it can be said to possess a uniform level of sophistication, is in the reviewer's judgment a bit over the head of the educated layman. It will be useful for the second term of a first-class university introductory economics course or for U.S. economic history courses in which introductory economics is a prerequisite.

The other countries with which the United States is compared turn out to be almost exclusively the Soviet Union. Of doctrinal and theoretical background selections, the volume is unfortunately bereft. There is a moderate amount of duplication. Regrettably, there is no index.

Slightly over one-fourth of this brief volume is accounted for by material very familiar to economists: a condensation by Edward F. Denison of his own *Sources of Economic Growth in the United States*, Theodore Schultz's December 1962 presidential address before the AEA on "Investment in Human Capital," and sixteen pages from the 1962 *Annual Report* of the Council of Economic Advisers. Another fourth of the volume is accounted for by a long, uninspiring, but helpful, panoramic lecture on growth anatomy by Peter Gutmann. An informative 22-page essay by Stanley H. Cohn on U.S.-Soviet growth comparisons carries us up to almost two-thirds of the book. For most of the rest, there is a somewhat chatty and frequently incisive piece by Robert M. Solow; some descriptive material on research and development; a vignette on fiscal policy by Otto Eckstein; what may come to be known as "Samuelson's 22 Points" on fiscal and financial policies (for growth, of course); and a significant essay on the postwar Kuznets cycle by Edward M. Bernstein. The impediments to growth are presented by the Chamber of Commerce of the United States and are, of course, alleged to be heavily governmental in origin.

The Bernstein discussion supplies this book's explanation of the slowdown in U.S. economic growth (or, more properly, in intermediate period expansion) after the mid-1950's. The slowdown is presented as a normal Kuznets downswing. For a period still too short to give us decent answers to trend questions, the Kuznets cycle apparently does just as handsomely or poorly as the Kondratieff long cycle did for Schumpeter in rebutting the stagnation hypothesis as applied to the Great Depression of the 1930's. The impact of Bernstein's analysis is given a coda, in the very last selection, by Eckstein's confession that he sees "no reason to accept the most pessimistic of the theories, that of secular stagnation" (p. 177). If we are going thus to write off a hypothesis of secular stagnation in the private market mechanism, insofar as it is possible any more to disengage analytically that mechanism from the public budgets, shouldn't we do it explicitly, and for some attempted

good reasons? Similarly for Schumpeter's theory of crumbling walls, also ignored in this work.

This criticism of the handling of secular decline is underscored by the book's lack of treatment of the possible implications of the apparent upward secular trend in the output-capital and the incremental output-investment ratios. The book's only treatment of the prospects for private capital formation as a growth determinant is a superficial substitution of statistical projections for economic theory. The related question of the future role in economic growth of government expenditure and debt is by-passed in Eckstein's essay, which presumably should have dealt with it. Even Samuelson's 22 Points, while scarcely revealing any emphasis, suggest several stimulants to *private capital formation*, even while noting, somewhat disparagingly, that the classicals "tended to place all their emphasis on capital formation as the sole source of progress." Many other propositions, some of them refreshingly succulent, can be found in Samuelson's list.

In general, the book gets a rather low excitement rating because it skirts fundamental theoretical controversy regarding U.S. growth problems and prospects in favor of description. A gentle exception to this criticism is the entertaining treatment by Solow of the differing opinions between the three-percenters and the four-percenters. (He concludes the argument is not solidly based.) On the other hand, if one wants largely description and suggestions as to the many aspects of growth, these pages yield a fairly generous portion. "Everyone" who is getting into theoretical questions on this subject will at least wish to be familiar with the sort of material contained in this book, if he is not already.

HAROLD G. VATTER

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### **Economic History; Economic Development; National Economies**

*Economic Growth in France and Britain, 1851-1950.* By CHARLES P. KINDLEBERGER. Cambridge: Harvard University Press, 1964. Pp. xii, 378. \$8.95.

Economics, said Cannan, is a serious subject. Can the same be said of economic history? Charles Kindleberger thinks not: "Economic history, like all history, is absorbing, beguiling, great fun. But, for scientific problems, can it be taken seriously?" (p. 332).

In essence his book is an essay in criticism. Kindleberger explicitly disavows credentials as an economic historian. He relies exclusively on secondary sources, which he subjects to detailed logical analysis and compares with one another in an attempt to evaluate their explanations of French and British economic history of the past century. He devotes the greater part of the book to what he calls "partial equilibrium" analysis, with chapters on each of the potential determinants of the level and rate of change of economic development: resources, capital, population, social factors (values, religion, family and class structures, etc.), entrepreneurship, technology, and the role of government, discussing its probable effects on economic growth. He also deals

in separate chapters with some specific sectors and structural features: agriculture, foreign trade, scale and competition, urbanization and regional balance, leading and lagging sectors and industries. Finally he considers the extent to which the patterns of growth may have been determined or influenced by such random exogeneous factors as harvest variations and wars, and concludes with a brief, summarizing "search for a method."

At the level of the individual chapters Kindleberger's treatment is superb. Although he eschews primary sources, he has digested an enormous amount of secondary literature, in the French case especially. Factual errors are few and mostly unimportant, although it is certainly incorrect to say that French agriculture in general is backward today (p. 217; see Gordon Wright, *Rural Revolution in France* [Stanford 1964]), or that "after World War I Britain thought that defeat of . . . Japan had removed its major international competition" (pp. 319-20). Kindleberger's logic is usually impeccable on matters of detail, although he gets himself involved in circular reasoning in his discussion of French coal resources (pp. 17-23). Not least important, Kindleberger writes with verve and force, if not always with fairness to the authors he criticizes. In short, economists with little prior knowledge of French and British economic history will learn a great deal from the book and will enjoy reading it as well—especially if they have an innate bias against economic historians.

In spite of these substantial merits, the book as a whole is not only disappointing but obscurant, not merely "negative and paradoxical" as Kindleberger himself describes it (p. 3). One suspects, in fact, that he set out to write a somewhat different book. Internal evidence as well as a letter from the author to the reviewer in 1958 suggest that Kindleberger originally intended to set economic historians straight on the causes of the relatively slow growth of the French economy over the last century or so. (The inclusion of Britain as a "control" may or may not have been an afterthought, but the discussions of the British experience occupy far less space than those on France and are based on a much narrower selection from the relevant literature.) If so, the task proved more difficult than his presuppositions led him to expect. In Kindleberger's view none of the explanations that have been offered to account for the patterns of French and British economic growth and stagnation is satisfactory, but he can provide no general explanation of his own. Instead he is led to a blanket indictment of economic history in general (or is it economic historians?). "What has been revealed is the virtual impossibility of proving anything positive about theories of growth through the use of history, and the propensity of economic historians, with rare exceptions, to overgeneralize" (p. 324).

The indictment will not stick, however. What theory could possibly be "proved" by historical evidence? Most economic historians, insofar as they are competently trained in their profession (but of course not all are), use theory to *interpret* the evidence. Moreover, as Kindleberger himself points out, "The difficulty is that there is no single theory of economic growth . . ." (*ibid.*). Are economic historians to blame for that? One is reminded of the fable of the hedgehog and the fox: the fox knew many things, but the hedge-

hog knew one BIG thing. Economic historians are foxes: they know many things. Kindleberger wants to know that one big thing, and, in effect, blames economic historians for hiding it from him. Most economic historians would agree that a "single theory of economic growth" not only does not, but cannot, exist in any meaningful sense. Kindleberger, however, accuses them of using "partial theories as if they had total validity" (p. 325). This is surely an example of the kind of overgeneralization for which he stigmatizes economic historians. The more usual criticism of economic historians is for failure to generalize at all. In spite of abundant footnote documentation throughout the volume, the section entitled "Single-Valued Explanations of the Alleged Slowness of French Growth" (pp. 8-10) does not contain the name of even one author of such a "single-valued explanation"—nor does any other page. Those anonymous authors are, in fact, straw men.

This is not to say that Kindleberger might not have named the names of some authors of highly oversimplified or self-contradictory generalizations about French or British economic growth. (But if he had, not all of them would have been economic historians.) Before issuing his blanket indictment, however, he might have been more careful to distinguish those who, being neither good economists nor good historians, fail to recognize the complexity of their subject matter and deal with it as if it were a simple algebraic model or a tale to be told. Serious economic historians have long since given up the search for "single-valued" explanations of economic change. If Kindleberger's excursion into historical analysis has brought him around to that position, he is to be congratulated on having proved that history is not entirely useless.

The discipline of economic history today stands in much the same relationship to economics as social science generally does to natural science. The number of relevant variables is enormous, the manner of their interaction extremely complex; the difficulties of accurate measurement are formidable, and the cost of gathering data is great. Moreover, the economic historian has a dual task: he must both gather his own data and attempt to organize them in a coherent manner. Given the brevity of life, the scarcity of financial and institutional support for economic history, and the variety of interesting problems that beckon, it is not surprising that some individual scholars either never generalize or attempt to generalize too soon on the basis of inadequate data—the latter failing is not unknown to other branches of the profession with less difficult data problems. This is not meant to exempt economic historians from criticism: their work should be judged and criticized by the same criteria that apply to that of other economists. But to castigate its practitioners because their subject matter is difficult is not only irrelevant but ridiculous. As a critic of economic history, can Kindleberger be taken seriously?

RONDO CAMERON

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*Economic Growth in the West—Comparative Experience in Europe and North America.* By ANGUS MADDISON. New York: Twentieth Century Fund, 1964. Pp. 246. \$4.50.

There is much food for thought in this volume by an author who has been

a staff member of the OEEC-OECD for the past 11 years. Angus Maddison attempts to explain why and how the European economy accelerated its growth in the 1950's; why the United States has not shared in this acceleration; and why the performance of some European countries has been "better" than others. His study "did not arise from any desire to theorize about growth, but out of a practical interest in forecasting what the future development of the Western industrial economies would be." His theoretical framework is extremely simple and is based upon the Harrod-Domar identity: the rate of growth of output is the ratio of investment to output divided by the marginal capital-output ratio.

To explain economic growth in the West, one must explain the growth of output per man-hour. From 1870-1913, 1913-50, and 1950-60 the growth of productivity accounted for 71 per cent, 90 per cent, and 83 per cent of the growth of output.

His thesis is as follows:

1. The maintenance of high and stable levels of demand has been the major reason for the improved performance of the European economies as compared with the past and is part of the reason why they have grown faster than the United States.

- (a) The investment-output ratio is negatively related to the risks of investment. Confidence in the maintenance of full employment has induced businessmen to maintain higher investment-output ratios than prevailed in the past.

- (b) The impact of technical progress depends largely upon the age of equipment and the rate of investment. With the adoption of new and improved technologies as a result of investment which lowers the average age of equipment, the marginal capital-output ratio need not increase. On the other hand, investment is more likely to produce capital-deepening and a rise in the marginal capital-output ratio in countries which already have the most up-to-date technologies.

- (c) The rate of increase in employment was higher in Europe in the 1950's than in earlier periods; and it was particularly high in the fast-growing countries. When there is an ample growth in the supply of labor, output could be increased without a rise in the marginal capital-output ratio. A high level of aggregate demand was necessary to convert the growth of the supply of labor into the growth of employment.

2. Under conditions of full employment, the author believes that the European economies will grow at higher rates than the U.S. economy, because the U.S. productivity level is twice that of Europe. Most of this productivity gap is due to missed investment opportunities: the European countries are working below the fringe of best-practice technology, and they also have lower capital-labor ratios than the United States. For many years Europe had the potential for high growth, but the failure of an adequate level of aggregate demand prevented the economy from growing at its capacity rate during the earlier periods.

3. Success in the 1950's was often due to luck rather than to policy. "For most of the 1950's, European governments made their policy decisions with-

out any quarterly estimates of national income, and without benefit of seasonally adjusted figures for other key indicators. In many cases, forward indicators, such as order figures were not collected. There was little effort to set up the budgetary accounts in such a way as to quantify the impact of discretionary policy measures as distinct from the impact exerted by the existing tax structure. There is such a heterogeneity in government accounts that it is not possible to compare the effect of policy in different countries in any quantitative way." But nearly all European countries sustained a buoyant level of demand, whereas in the United States policy was lacking in this respect.

4. Maddison feels that governments should have a growth policy that would manage over-all demand and the price level to ensure full employment. He specifically advocates "an articulate effort to define the potential of the economy and to see that resource allocation, particularly investment, is adequate to reach this. These ends are complex and cannot be achieved by getting a tax structure that automatically provides built-in growth or built-in stability. Policy will have to be pretty continuously active, although the need for intervention will vary between countries with different problems and institutions."

There are many questions posed by the author which are not satisfactorily answered.

(a) Why were there intra-European differences in the rate of growth of labor productivity? Why did German productivity from 1950-60 grow at 6 per cent (per annum) while the rate of growth of productivity was 4 per cent in France, Italy, Norway, Switzerland, and the Netherlands?

The author claimed that Italian productivity was clearly below that of the other European countries. Since he believes that the level of productivity is negatively related to the long-term growth potential, Italian productivity should have grown at the highest rate. Why did not Italy grow more rapidly than the others, in view of the fact that there was no more deflationary pressure in Italy than in the other countries?

(b) Although the author desires to forecast future growth rates and to distinguish between the long-term and the transitory forces of growth, he has not succeeded in providing an objective basis for his quantitative results.

We have given reasons for thinking that the productivity achievements of some countries, such as Germany, France and Italy, were due to special factors, and have also suggested that the productivity growth potential of European countries is better than that of the United States. To go further than this and pick on a particular rate as a yardstick, of course, involves some guesswork, but, after so much statistical analysis, we should be willing to venture a judgment. For European countries an average productivity growth rate of less than 3 per cent a year can be taken as evidence of deficient policy. . . . For the United States and Canada it is difficult to fix a norm, but by the standards of the early 1950's, their productivity performance from 1955 onwards was very poor.

No European economy achieved a 3 per cent growth rate of labor productivity from 1870-1913 or from 1913-50; and only Sweden reached that rate from 1938-60. Maddison's 3 per cent long-term growth rate was arrived at subjectively, and he admits that it involved guesswork. Nevertheless, he is an ardent advocate of growthmanship: the policy (summarized in number 4 above) of actively managing the economy to see that investment is adequate to achieve a certain target rate of growth. On what grounds can one advocate managing an economy to achieve a capacity growth rate that is founded on guesswork and exceeds all long-term trends? Particular attention should be given to number 3 above: that policy decisions were based upon ignorance of the facts. His policy conclusions are not grounded in a quantitatively objective analysis.

This volume is interesting and useful in two respects. First, it is a serious study of the remarkable growth rates achieved by the Common Market countries relative to the experiences of the United States and the United Kingdom. Second, it demonstrates the need for using a much more sophisticated tool of analysis than the Harrod-Domar identity in explaining this empirical phenomenon.

JEROME L. STEIN

*Brown University*

*Experiments on a Simulated Underdeveloped Economy: Development Plans and Balance-of-Payments Policies.* By EDWARD P. HOLLAND WITH ROBERT W. GILLESPIE. Cambridge: The M.I.T. Press, 1963. Pp. xviii, 289. \$8.00.

As the author states in his preface, this book "is a report on some exploratory experiments in economic dynamics performed on a simulated underdeveloped economy." A 250-equation dynamic model was used to represent the economic structure of a typical underdeveloped society. Data on the economy of India provided a guide to intuition in the selection of the relevant parameter magnitudes. Sensitivity studies were undertaken to investigate the nature of desirable adjustments to two important economic problems facing most underdeveloped nations today: inflation and the maintenance of a satisfactory balance of payments. The merits of alternative development programs, as represented by different levels and sectoral allocations of investment, were then investigated in this context. Finally, several trade policies were studied in a similar manner.

The one major criticism of Dr. Holland's procedure which must be made is that the nature of the compromise between flexibility in dynamic specification and disaggregation within a single time period is not especially appropriate for the investigation of long-run development. On the one hand, a very short time-step—one-twentieth of a year—was used, a choice which permits the relationships in the economy to include many modes of feedback and to vary with time in other ways in as complex a manner as desired. For example, the supply functions were made to shift through time in response to new

investment, changes in prices of intermediate products, and variations in wage rates. Similarly, a kink was built into the short-run supply functions, and a ratchet effect into the wage-rate equations.

On the other hand, even though a simple equation count indicates that the simulation effort was quite detailed, the actual number of behavioral relationships included in the model is in fact rather small (of the order of 30). This means that many compromises in the degree of disaggregation had to be made because of computer time and computer memory limitations, and therefore that the normal economic relationships were treated relatively crudely. Specifically, the productive activities of the economy were aggregated into a mere six sectors, and the effects of income distribution upon consumption patterns, as well as most of the economic manifestations of dualism, were entirely ignored. This set of compromises appears to this reviewer to be much more relevant to the investigation of short-run stabilization in an industrialized society than it is to long-run planning of economic development.

In spite of this shortcoming, Holland's investigation demonstrates forcefully the great potential of simulation techniques in the construction of practical development programs. If a model is chosen which constitutes a reasonable representation of the economic interactions of the real world, simulations of this type offer a promising (and humane!) alternative to direct experimentation with an existing economic system. Simulation, in other words, can be used to estimate the effect of different planning objectives upon the time paths of the important economic variables. Similarly, parametric studies of policy-induced changes in such characteristics of the system as its behavioral propensities, its instrument variables, and the inertia of its institutions can be analyzed before policy decisions are made. It must, of course, be recognized by the analyst that the virtue of the use of computer simulation techniques in this manner is not that one can get accurate predictions of the effects of specific policies, but rather that one can acquire at least a basic understanding of and a qualitative feeling for the reactions of the economy to various events.

The usefulness of the technique depends crucially upon the validity of the representation of the system to be simulated. Unfortunately, it is extremely difficult to determine, before experimenting upon the economy in question, just how appropriate the model employed is to the problem at hand. It is in this area, the construction of suitable tests to determine the degree of correspondence of the computer model to the real economy, that new work and new ideas are especially needed. In his simulation, Holland did not have to face this problem, since he was analyzing a synthetic, albeit realistic, economy. Clearly, however, successful application of simulation techniques to existing economies will require considerable attention to the correspondence between model and country.

IRMA ADELMAN

*The Johns Hopkins University*

*Capital Formation and Investment in Industry—A Report of the International Conference on Problems of Capital Formation and Investment in Industry Held at Istanbul, August 6-25, 1962.* Istanbul: Economic and Social Studies Conference Board, 1963. Pp. xvii, 467.

This volume is the report of a three-week conference held in Istanbul in August, 1962. One week each was devoted to domestic sources of capital and manpower, foreign aid, and planning, as each of these affects the industrialization process. For the most part, the format of the book is a sort of "club sandwich"—a layer of foreign expertise, a layer of Turkish experience, a second layer of foreign expertise, etc. Most of the speakers were economists, but both private enterprise and public administration were also represented.

When one scans a table of contents and sees such names as Bert Hoselitz, Basil Yamey, John Adler, Roy Harrod, Hollis Chenery, Arthur Lewis, Pitambar Pant—and others all distinguished in one way or another—one's appetite is naturally whetted. On the whole, however, the volume is disappointing. The Turkish contributions add up to a useful picture of Turkish problems and policies, but the foreign contributions add up, at best, to a rather dull textbook. There is a general *de haut en bas* flavor about these chapters; the foreigners clearly did not reserve the more brilliant of their recent ideas for this occasion. W. A. Lewis, for example, provides a scant five pages in which he says that agricultural improvement is essential for industrial development. Hollis Chenery, in five and one-half pages, describes foreign aid programs, particularly the U.S. one, and offers a mild criticism of the "project approach" to foreign aid. Some of the other foreign papers were somewhat longer, but few were more original.

There are some bright spots. The reviewer particularly enjoyed the neat demonstration by Professor El. Naggat and Sir Roy Harrod that only a fraction of PL 480 commodity surplus disposal is really "aid," and that recipient countries are usually better off if counterpart funds are not spent at all. Mr. Cihat Iren provides a splendid example of how governments of developing countries discourage foreign investment while passing laws ostensibly designed to encourage it. Readers who have not encountered it elsewhere will find Pitambar Pant's description of perspective planning in India interesting, although some may share the reviewer's feeling that it is slightly curious for an avowedly socialist country to take the Pareto curve as given and use it as the foundation for long-run planning. Sir Roy Harrod's arithmetic on ICOR and assisted take-off is useful, although not altogether new. There are also some very low spots, such as Maxwell Stamp's condemnation of foreign aid as "charity" and his stern rebuke to the peoples of underdeveloped countries for dissipating savings for ceremonial purposes (does he feel the same way about support to opera and ballet, I wonder?).

More interesting to the reviewer than anything clearly said is the thread of between-the-lines bewilderment that runs throughout the volume. There is a general agreement that, in meeting the problems of developing countries, marginal analysis and reliance on free market adjustments alone are not

enough; and there is general discomfiture over the uncertainty as to just what should be put in their place. In this respect, of course, the authors merely reflect the current condition of the economics profession. (The nineteenth-century cohesion of economic and political philosophy is gone.) The volume opens with a foreword by Ismet İnönü, Prime Minister of Turkey. There are only two systems of economic development, he says; Turkey has chosen the "democratic system." But "democracy" is not a system of economic development. It neither guarantees nor prevents development in itself and is consistent with a wide range of ratios of public to total investment, with varying degrees of control over the private sector, and with various types of planning. *Laissez faire* or economic liberalism may be a "system" of economic development, but it is one Turkey has renounced in favor of a mixed and managed economy. Moreover, it is clear from this volume that the Turks are no closer than anyone else to a clearly articulated political philosophy to justify and guide the mixed and managed economy. Indeed, as Osmar Okyar makes clear in his chapter, Turkey has had trouble enough in formulating the economic rules of the game in such a society. Similarly, Professor Ülgener displays the typical uneasiness of the economist who sees a necessity of substituting some shadow prices for some market prices (although he does not use these terms) but does not know exactly how shadow prices should be determined.

Clearest in his own mind on this range of subjects is Sir Roy Harrod. He comes out squarely for perspective planning as well as short-run management and (in sharp contrast to Dr. Willmann) denies any conflict between planning and freedom: "Nobody is compelled to do anything except perhaps the state enterprises. . . . The success depends on a confidence that the different sectors will play the game in their own interest." Nor is the price mechanism inconsistent with planning: "On the contrary, if the price system is working smoothly, it is a sign that the planning itself has been efficient." Harrod also makes the significant distinction between taking savings as given, while planning total investment so as to achieve stated objectives, and treating savings as a policy variable.

The book is, unfortunately, all too typical of recent publications in the field of economic development and reflects the generally low state of the art of development planning. We badly need two things: facts concerning the whole vast array of parameters governing economic development and a new, systematic social philosophy to fit societies which no longer accept the free market as the sole guide to economic policy and the sole mechanism for economic growth, but which have not embraced the Communist faith.

BENJAMIN HIGGINS

*University of Texas*

*Hungarian National Income and Product in 1955.* By T. P. ALTON, L. CZIRJAK, L. SMOLINSKY, AND G. PALL. New York and London: Columbia University Press, 1963. Pp. xv, 254. \$5.00.

This study not only provides the necessary material for the analysis and

understanding of the Hungarian economy in the prerevolutionary era but, with its analytical methods, sets an example of how to make a successful estimation of national income in a Soviet-type economy. Moreover, the description and evaluation of Hungary's economy serve to characterize the general pattern of structural changes in the whole East Central European area.

The Soviet-type economies of East Central Europe, with a population 45 per cent that of the Soviet Union, have an output roughly equal to the same proportion of the total USSR production. This represents an important element in the world's economy. Although an increasing amount of economic information is being published by these countries, the concepts in which most of the data are reported, the peculiarities and distortions of the existing price system in which the values are expressed, require a separate analysis. As is well known, official income statistics coming from Soviet-type economies are given in prices which represent the preference of the Central Planning Bureaus, and the price of products will neither reflect their relative scarcities nor represent the real factor costs. Thus the official figures should be modified especially for international comparisons.

The authors in this study recalculate Hungary's national income and its components in terms of their real factor costs. The price of a good or service is re-evaluated on the basis of the real costs of inputs, and, by this method, the price distortions are ironed out. On this basis, the authors represent the national accounts in accordance with standard methodology and concepts of the West, showing the sources and final uses of gross national product. The authors in doing this enormous research (recalculating the real costs of all hired factors and deriving the national income) obviously faced many difficulties, and it is natural, in spite of the very important contribution and material presented in the study, that they were unable to overcome all the pitfalls.

It is known that in the Soviet-type economies the prices of consumer goods tend to deviate considerably from the real factor costs due to higher rates of turnover taxes imposed on such goods. To evaluate an undistorted consumer-good price, the real burden on the households (the aggregate sales tax, turnover tax levy) should be counted. The authors do this, and they claim that the turnover taxes represent 62 per cent of the state revenues (p. 90). This huge sum is the basic source for government services and investments, but it is also probable that these turnover taxes were used for financing the losses that Hungary suffered in foreign trade. However, the study does not indicate clearly the latter use of available government funds. Moreover, it deducts the loss in foreign trade from the national income. This method is completely justified in a Western price-directed economy, but not in a Soviet-type economy where the state firms are compensated in internal prices for their products, whatever may be the prices in the international market.

From this it follows that the Hungarian national income, which is given as around 112 billion in 1955 forints (p. 222), is probably underestimated. This consideration will demand caution of anyone using the results of this study for international comparisons.

The volume also provides the framework for a forthcoming study of Hungary's economic growth in the period 1938-60.

PAUL JONAS

*Brooklyn College of the  
City University of New York*

*Large Economic Regions of the U.S.S.R.—Some Problems in Territorial Organization of the Economy.* By L. N. TELEPKO. Moscow: Institute of Economics, Academy of Science, USSR, 1963. Pp. 198. Rbl. 0.53.

The book under review consists of three parts: (1) the regional organization of economy; (2) some problems of specialization of large economic regions; and (3) problems of complex development for large economic regions.

Economic division into districts in the Soviet Union began more than 40 years ago. By the end of 1957 the Soviet Government recognized that the existing departmental branch structure for the management of industry did not meet the requirements for national economic development. In an effort to improve managerial controls over industry, many ministries were abolished and 105 (subsequently reduced to 47) economic-administrative regions were established.

Because of continuing economic problems, the Soviet Union in recent years has undertaken reorganizations in the administration of its industry. In establishing the Councils of National Economy at different administrative levels, it has paid particular attention to the large economic regions. In 1961, 17 large economic regions were established instead of the 13 created at the end of the 1940's.

It is not difficult to determine the chief reasons for these rapid changes. As the Soviet economy became more complex, the problems of its management appeared more difficult. The Soviet Government sincerely believed that all shortcomings, which are in fact an organic part of the Soviet economic system, could be removed by the creation of these regions.

Among the data presented by L. N. Telepko are the predominant specialization of large economic regions in the USSR on the basis of specific weight of industrial branches to all-union production, and major branches of all-union specialization of industry for the 17 large economic regions except Belorussian and Moldavian SSR.

The range of problems considered by the author is wider and includes such questions as: correlation between regional and branch organization of the national economy; territorial division of labor and specialization of economic regions; economic efficiency of the specialization of production and its most important indices; large economic regions and the planning of territorial specialization of production; basic index of the specialization of large economic regions, and some other problems.

It would seem that the author's method of determining the specialization of large economic regions on the basis of the number of workers in the separate branches of industry gives a very inaccurate picture, since he does not take into consideration the differences existing between the branches in terms of

great expenditures required for labor, electric power, mechanization, and the training of labor.

As any book written from a Marxist point of view, this book should be read critically. Once the propaganda is sifted out, however, there remains a considerable volume of interesting and informative material which can be very useful to scholars of the Soviet economic system.

TIMOTHY SOSNOVY

*The Library of Congress*

*Mensen en Achtergronden.* By J. G. VAN DILLEN. Groningen: J. B. Wolters, 1964. Pp. 571. Fl. 25.

On the occasion of his 80th birthday a group of colleagues and former students decided to honor Professor J. G. van Dillen of the University of Utrecht with the publication of this volume. It consists of a series of 21 essays by van Dillen himself. Some of these have not been published before, and others are reprints of articles which have retained their importance over the years and are not always easily accessible to interested readers. Van Dillen's scholarly contribution has been a formidable one, and is still expanding. The bibliography of his published writings at the end of the book has over three hundred entries, and we are told that his eagerly awaited, comprehensive work on the economic history of the Dutch Republic in the seventeenth and eighteenth centuries is now in an advanced stage of preparation.

Students of the social sciences who may not already have become familiar with van Dillen's writings will have in the present volume a good opportunity to become acquainted with the range and quality of his work. His time-consuming activities in the archives as editor of source materials on Dutch social and economic history, such as on the Amsterdam Wisselbank and the Amsterdam guilds, had long since earned him among specialists a reputation for meticulous scholarship. As principal editor of the *Tijdschrift voor Geschiedenis* over a period of 40 years, his interests ranged widely beyond the narrow limits of regional specialization, and this breadth of scholarship is also reflected in the selections that have been made for the volume under review. They are grouped under three general headings, namely, (1) nine articles of general interest, (2) five articles on the Russian revolution, and (3) seven articles on the economic and social history of Amsterdam.

In a short review of a publication such as this one, it is a problem to decide what to select for emphasis. Certainly the three articles on the history of the Amsterdam Wisselbank from 1609 to 1820 are a valuable addition to our knowledge of this institution. Only the first of these three articles has hitherto been printed. Together they give us the first authoritative, connected account by the foremost student of the subject of the vicissitudes of the Amsterdam bank from its beginning to its demise. In the article on the size and composition of Amsterdam's population in the seventeenth and eighteenth centuries, his analysis is notable not only for its skill in handling the imperfect source materials, but also for its substantive demonstration that the population of Amsterdam was steadily rebuilt and reinforced in these centuries by people from the surrounding Dutch countryside as well as by massive influxes from

the Southern Netherlands, France, Germany, and Scandinavia and from areas as distant as the Levant and Portugal.

It is likely that the interest of economists, sociologists, and historians will tend to concentrate itself on what Professor van Dillen has to say on broad and general subjects, such as the economic interpretation of imperialism, the significance of the concept of mercantilism in contemporary economic and political thought, the gild system, the relation of economic history to economic theory and sociology, or his discussion of the relative importance of economic and psychological factors in the economic development and stagnation of nations, and the suggestive historical parallels in the economic changes of the Netherlands and England during the last few centuries. The over-all impression conveyed by his discussion of these general topics, as well as by his contributions to regional economic history, underlines once more that contemporary Dutch scholarship in economic history reflects a more thorough utilization and understanding of foreign contributions in this field than can be said to be the case the other way around.

MAX GIDEONSE

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### Statistical Methods; Econometrics; Social Accounting

*A Priori Information and Time Series Analysis: Essays in Economic Theory and Measurement.* By FRANKLIN M. FISHER. Contributions to Economic Analysis XXVI. Amsterdam: North-Holland Publishing Co., 1962. Pp. xii, 168. Paper, \$5.00.

Although this book contributes primarily to econometric methodology, the empirical chapters not only illustrate the method, but also include results of interest to students of the wheat, aluminum, and railroad passenger markets. In addition, Franklin Fisher presents a theory of expectation formation, which he tests by experiments with college students and by estimation of the United Kingdom's demand for wheat from 1867 to 1914. Incidentally, this work establishes Fisher as an economic historian and experimental psychologist.

Notwithstanding the term "a priori" in the title, no use is made of formal Bayesian analysis. Instead Fisher, by precept and exhortation, continuously points to the moral that mechanical estimation and hypothesis-testing make little sense in economics and that even apparently simple problems require a mixture of good judgment, statistical knowledge, and economic theory. No doubt some mechanical curve-fitting still goes on in the back rooms of economics departments, so that Fisher's sermon is well taken. Given the nature of computer regression programs, the danger of naïve estimation is ever present, and even the virtuous need to be reminded of this now and then.

In line with the author's prejudice against mechanical procedures, the book starts with a plea for selective estimation. This leads to procedures for grouping together data for certain kinds of years and discarding certain observations. Fisher develops an ingenious method that can estimate both short- and long-term responses based somewhat on his expectations theory.

Fisher develops a novel theory of expectations. According to this theory, crucial information is provided by the turning points of a time series. Briefly, this theory says that firms and households pay particular attention to those turning points that are neither too close together nor too far apart. However, they do not pay attention to the *size* of the change, although the size of their response is assumed to depend on the size of the stimulus.

Fisher does provide a rationale for his expectations and response theory. He argues that it is reasonable to stress turning points of *nonstationary* series. Moreover, since decision-making and the gathering and evaluating of information are costly, it follows that responses are not continuous functions of stimuli and that changes of expectations are not continuous functions of the discrepancy between observations and predictions.

Unfortunately, Fisher's evidence in support of his expectations model is, in his own words, "mild" since it depends on the way that college students forecast the U.K. wheat price series. Moreover, although Fisher cites E. J. Working's expectations model and the Koyck adjustment model, he does not test how well these compare with his own method of estimating long- and short-term responses. The Cagan expectations model used successfully by Friedman, Nerlove, Meiselman, and others provides an alternative to Fisher. It is now known that there is a sound statistical theory for the Cagan model, which had been developed much earlier by Weiner, Wold, and Kolmogorov. For a well-specified class of time series, it can be shown that Cagan's formulation provides good predictions in the least-squares sense, and some writers have developed a theory of rational expectations to exploit these properties. It would be desirable to compare these models with Fisher's expectations theory to see which gives better predictions of behavior. For instance, Fisher summarily rejects the Koyck model without presenting any evidence for doing so.

Two chapters in the book, "The Demand for Aluminum" and "The Demand for Railroad Passenger Traffic," are well worth assignment to advanced undergraduates and beginning graduates. There is an example in the former of Fisher's occasionally cryptic style. Without explanation Fisher deflates the aluminum price by the average metal's price instead of a general price index. It is a tricky exercise for the reader to show why this may avoid biasing downward the estimates of the aluminum price elasticity (see p. 99). The passenger-traffic chapter includes an intricate argument for buttressing Fisher's estimates of the long-run effects of changes in the stock of cars on rail passenger traffic. Fisher supposes that a decrease in the stock of cars is associated with a decrease in rail passenger traffic if it results from a decrease in real income which occurred from 1929-34. However, a rise in the stock of cars also reduces passenger traffic because the two are substitutes. Despite the fact that the basic regression contains an income variable, and despite a paucity of observations that prevents direct tests, Fisher makes out an ingenious, but not entirely convincing, case in support of his hypothesis.

On every page this book displays the brilliance and virtuosity of its author. The arguments are complicated but essentially nonmathematical. Nev-

ertheless, a close reading is rewarding, and attempts by the reader to supply the gaps in the argument will pay handsomely. So much is packed into this book, which is derived from Fisher's Ph.D. dissertation, that a brief review cannot do it justice. The econometrician must read this book; the general economist should at least skim it.

LESTER G. TELSER

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*Essays on the Structure of Social Science Models.* By ALBERT ANDO, FRANKLIN M. FISHER, AND HERBERT A. SIMON. Cambridge: The M.I.T. Press, 1963. Pp. iv, 172. \$5.00.

The most prestigious and perhaps the most prevalent type of current econometric activity appears to involve estimating parameters of models in which the variables are interdependent, so that simultaneous estimation is in principle required, but in which the restraints on the model are sufficient to identify the coefficients, and in many cases to overidentify them. This practice has been attacked on the one hand by Herman Wold, who maintains that in economic reality the underlying cause must precede the effect, whatever may be the case for the observations taken as proxies for the underlying variables, and that therefore any faithful representation of the underlying reality must be in terms of causal chains in which interdependence is absent and elaborate simultaneous estimation procedures are uncalled for. From the other side, T. C. Liu has asserted that all observable variables are inherently interdependent, if not directly, then through pervasive or indirect relations too diverse to be fully represented in any model, so that any attempt at identification of parameters in a specific model by a priori restrictions on some subset of the parameters is necessarily erroneous, and even a separation of variables into exogenous and endogenous is of dubious validity. This collection of essays attempts, among other objectives, to rehabilitate the prevailing practice by examining the mathematical properties of systems in which the specifications are "almost" satisfied.

The general procedure running through these essays is to show that under appropriate conditions the behavior of a system having specified coefficients exactly equal to zero is approached asymptotically by the behavior of sequences of systems for which the corresponding coefficients merely approach zero asymptotically. Three types of systems are discussed: general identified or overidentified systems, block-diagonal or "completely decomposable" systems with nonzero coefficients restricted to square blocks along the diagonal, and block-triangular or "decomposable" systems in which the nonzero coefficients may appear in the square blocks along the diagonal and anywhere above the diagonal. In each case it is shown that when the characteristics of the model are estimated by assuming that the restricted coefficients are all exactly zero when in fact they are nonzero but small, the errors thus made approach zero as the actual coefficients are made to approach zero, provided some reasonably general conditions are met. The demonstration is not trivial, since counterexamples exist where the convergence fails; unfortunately it

does not appear easy to give a nontechnical description of the conditions under which convergence is assured. However, the general tenor of the results is to offer some reassurance to those who, while recognizing that certain coefficients in their models cannot be exactly zero in actuality, nevertheless from convenience or necessity proceed on the assumption that they are exactly zero.

Matrices that are "almost block diagonal," in the sense that the coefficients outside the blocks are small, have a bearing on the aggregation of the variables within the blocks. An important distinction is made between aggregation for short-term analysis and for long-term analysis. In the long run, the variables within each block of a nearly block-diagonal system tend to move proportionately, so that each such set of variables can be represented by an index with good results. In the short run, each block tends to behave as a separate system relatively little affected by development within other blocks. This would seem to be almost reversing the traditional division of labor in which microeconomics is concerned with allocative efficiency in the long run, while macroeconomics is concerned with cyclical phenomena in the short run. If I understand the implications correctly, this type of result will be cold comfort to the analyst who is under pressure to produce short-term results from highly aggregated variables.

Indeed a disturbing feature of the dynamic models under discussion, which comes out especially in the last two essays, is that the convergence upon which so much depends often occurs only in a long run, as to the length of which we are given no very definite indication. Fascinating and elegant as these models may be as mathematical exercises, one has an uneasy feeling that, in a long run long enough for a satisfactory degree of approximation to the required proportionality to be achieved, technological change will have produced fundamental and significant changes in the basic coefficients, and the mathematical demonstration that all would be well if we could only follow the model long enough may be of little practical avail. Even in the short run, while it is some consolation to be told that in the limit the errors vanish, there is a world of difference between this mathematical property of a system and a practical judgment as to whether, if a small but finite error of specification is made or an aggregative shortcut is taken, the resulting error in the results will be within tolerable limits.

As is perhaps inevitable with this subject matter, these essays are not easy reading, even for fairly good mathematical economists. It is to be hoped that econometricians will not, as a result of hasty reading, relax their vigilance against errors of specification or aggregation, and that growthmen, whether under- or overdeveloped, will not be led to seek turnpikes with renewed assiduity. For those prepared to tackle these essays seriously, it is recommended that they first read the excellent introduction with great care, and then turn next to the nonmathematical description of the results in terms of armament races and voting contained in the article drawn from the *American Political Science Review*. Indeed, only the introduction and the last essay on von Neumann rays had not previously been published.

WILLIAM VICKREY

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*Econometric Methods.* By J. JOHNSTON. New York: McGraw-Hill Book Co., 1963. Pp. xiii, 300. \$7.50.

*Econometric Theory.* By ARTHUR S. GOLDBERGER. New York: John Wiley & Sons, 1964. Pp. xi, 399. \$10.95.

These two recently published books are the first of several new econometrics texts which will probably be published over the next few years. They represent the first basically new texts suitable for basic classroom use in econometrics since L. R. Klein's well-known *A Textbook of Econometrics* of a decade ago. They both embody much recent work and both offer systematic coverage to a greater or less extent of most of the major topics usually covered in a good graduate econometrics course. They will both doubtless receive heavy use in such courses.

In an effort to make the material reasonably self-contained, both authors devote a chapter to matrix algebra. Arthur Goldberger, in addition, spends over a hundred pages on the elements (and more) of statistical inference, whereas J. Johnston assumes some familiarity with the basic notions and introduces more difficult material when and if needed. While the desire for self-containment is quite understandable and, to some extent, laudable, Goldberger seems to me to have heavily overdone it in this respect. No student without a previous course in statistics will be able to acquire the necessary material from Goldberger alone without a great deal of difficulty, and there are far more suitable texts for statistics-teaching. Goldberger's treatment (as throughout his book) is rigorous without being particularly clear or—in this part—always particularly motivated. To some extent, the same is true of his treatment of matrices. Such chapters in econometrics texts can usefully serve as a convenient summary of results already known to the student or can usefully present those variations on basic results which will be needed, although these can be incorporated in the main text as required. In essentially attempting to provide entire courses in matrices and mathematical statistics, Goldberger goes beyond such useful and perhaps necessary functions to an extent that is likely to weary the average student before he ever gets to the econometrics sections of the book.

As for those sections, which after all are the ones which really count in an econometrics text, the difference in the titles of the two books well describes the basic difference in approach. While roughly the same subjects are covered in both volumes, Goldberger's treatment is heavily theoretical, while Johnston, who does not neglect theory by any means, is largely concerned to convey a feeling for the techniques as used in practice. As a result, Johnston is likely to prove a rather more satisfactory textbook for training students in the techniques of empirical econometric research, even though Goldberger may have more for that minority of students whose basic interest is the theory as opposed to the practice of econometrics. A student trained on Johnston should be able to do good empirical research, while one trained on Goldberger will be somewhat better able to cope with the theoretical literature if he has fully digested Goldberger's not always easy but certainly rigorous treatment. I should hesitate, however, to turn the latter student loose on an empirical project.

As stated, both books cover roughly the same areas and treat most of those topics which would generally be covered in a basic course. Such coverage, however, is by no means complete,<sup>1</sup> and the two works differ greatly in the amount of attention paid to subtopics within the main headings. Thus, for example, neither author adequately discusses such topics as aggregation or specification error to any extent, although one would have thought that some discussion of these would be appropriate. Johnston devotes an entire chapter to the problem of autocorrelated disturbances, while Goldberger gives that topic only very formal treatment as one way in which the classical assumptions of least squares can be violated.

The latter difference leads to another. In treating regressions with lagged dependent variables, Johnston is careful to point out the major problems that arise when lagged variables appear on the right-hand side of a regression and the disturbances are autocorrelated. Goldberger mentions that problem in the context of a particular distributed-lag model, but fails to generalize the point. He also assumes that the Koyck transformation leads to autocorrelated disturbances without pointing out that such a result depends on the autocorrelation properties of the disturbances of the original model. Johnston does point this out, but neither author suggests that in some versions of the model (Nerlove's adjustment model) the "natural" zero autocorrelation assumption is in fact on the disturbances after the Koyck transform. Whether the transformed disturbances are autocorrelated does not depend on the way the model is written; yet both authors write the model before the Koyck transformation and indicate that autocorrelation is probably introduced by that transformation. Incidentally, neither author points out that the Durbin-Watson test is inapplicable in the presence of lagged dependent variables among the regressors.

Turning to a different example of difference in coverage, Johnston gives a somewhat more lengthy discussion of multicollinearity than does Goldberger. The latter, however, gives an elegant treatment of the use of extraneous estimators, a closely related problem only mentioned in Johnston. Unfortunately, Goldberger fails even to mention the difficulties of specification and interpretation that arise in practice when combining different types of data, which problems have been discussed by several authors in the literature. Johnston at least adverts to such problems but also gives no discussion of them.

Such failure adequately to signal the reader that there is danger in incautious application turns up elsewhere in Goldberger. He spends two pages discussing stepwise regression—the practice of regressing residuals from a fitted regression on a new variable to see if it should be included. While he displays the formulae which give the relation between the true coefficients and the ones so estimated, he fails to point out specifically the clear implication that the practice involved is not generally a good one. The space spent on the topic

<sup>1</sup>Both books are dominated in this and other respects by E. Malinvaud's recently published magnificent work, *Méthodes Statistiques de l'Econométrie* (Paris: Dunod, 1964), which is not yet available in an English translation.

and the reference to stepwise regression as a "common practice" are likely to mislead all but the very careful reader in this respect.

Turning to another area, neither author's treatment of identification seems fully satisfactory, although here again Johnston's discussion seems better than Goldberger's. Both authors state the rank condition for identifiability in terms of the rank of a particular submatrix of reduced form coefficients—that corresponding to the included endogenous and excluded exogenous variables. While it is easy to see why the condition in this form is necessary and sufficient for the obtaining of estimates by indirect least squares or one of the other usual methods, it is both clumsy to work with and difficult to interpret. In particular, it bears no intuitive relation to the discussion of the identification problem in terms of admissible linear transformations of the original structure which the student probably finds easy to understand (and which is given by Goldberger but not by Johnston). The classical form of the rank condition in terms directly of the structural parameters, on the other hand, is easy to work with and to interpret, as well as being readily generalizable to cases other than those of exclusion restrictions. That form is given by Johnston but not by Goldberger. Even Johnston, however, adds no intuitive discussion, but presents the classical form for reasons of convenience of handling.

In their treatment of simultaneous equation estimation itself, both books are rather good. Johnston presents a very valuable detailed summary of the Monte Carlo literature, while Goldberger (who also discusses that literature at less length) presents a good discussion of economy-wide models and the implications of dynamic structure. I would disagree, however, with his statement (p. 369) that "economic hypotheses often refer to reduced-form, rather than structural, coefficients. . . ." Hypotheses almost invariably refer to structural coefficients; it is the implications of those hypotheses for policy which refer to the reduced form and which justify Goldberger's discussion.

While both authors appropriately devote considerable space to simultaneous equations, that subject plays a more important role in Goldberger than in Johnston. This doubtless reflects a difference in reasonable men's opinions as to the relative importance of various subjects, yet one wishes that Goldberger had given the same extended treatment to many of the single-equation problems which he discusses at less length than does Johnston.

In general, perhaps no teacher of econometrics is ever fully satisfied with another's treatment. Of the textbooks now available, that by Malinvaud, cited above, seems to me to be entirely the best in terms of completeness of coverage and depth of understanding and discussion. Of the books available in English, however, Johnston's is clearly the most satisfactory for classroom work. Its quality is well above that of the average text, and it provides a useful base for econometrics courses. Such deficiencies as it possesses are easily made up in lectures.

I cannot say as much for Goldberger's book. There are several places in which Goldberger's treatment will provide a valuable adjunct to another text, particularly for advanced students. As a basic text for an econometrics course, however, it seems to me to be too lacking in clarity, ease of presentation, and

intuitive discussion to be really satisfactory in a course in which a major objective is the training of students in the techniques of empirical research.

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### **Economic Systems; Planning and Reform; Cooperation**

*Soviet Strategy for Economic Growth.* By NICOLAS SPULBER. Bloomington: Indiana University Press, 1964. Pp. 175. \$4.50.

*Foundations of Soviet Strategy for Economic Growth—Selected Soviet Essays, 1924-1930.* Edited by NICOLAS SPULBER. Bloomington: Indiana University Press, 1964. Pp. xii, 530. \$12.50; both volumes, \$15.00.

The 1920's were a golden age in Soviet history. Following the successful defense of the Revolution against civil war and foreign intervention, Russia entered upon the years of the New Economic Policy, an exhilarating period of reconstruction and change, fraught with hope for the new world in the making. In contrast to the totalitarian age that would shortly stifle Russia under a ruthless Stalin, it was a time of great freedom. Though political liberties were severely circumscribed, there nonetheless was open and intense discussion, both inside and outside the Communist Party, of the myriad issues facing the new state. Among these, none attracted so much attention nor drew into the lists such a star-studded cast of debaters as industrialization and planning. For while most everyone agreed that industrialization was essential for the security of Russian socialism, there was very little agreement about what planning entailed and what role it should perform in achieving industrialization. This was hardly surprising, for in the 1920's Russia was pioneering new forms of economic institutions, and the guideposts to be followed were not self-evident. Some sought these in the hallowed pages of *Capital*, others in bourgeois economic analysis, and still others in the principles of engineering. And always there to remind Marxists, economists, and engineers alike of the urgency of the issues were the Party leaders—Trotsky, Stalin, and Bukharin, to name the most prominent—who themselves participated in the discussion and so made it part of the bitter struggle for the succession of Lenin.

The debate thus generated stands with the greatest in the history of economics. There was, in the first place, something to debate about. A crisis confronted the Soviet economy. In the latter part of the 1920's the perennial Russian agricultural problem was exacerbated as the terms of trade moved against the peasant, prompting him to reduce the goods he exchanged for manufactured output. The revolutionary chickens had come home to roost. For in the institutional changes brought about by the Bolsheviks, the peasants had acquired greater freedom in the disposal of their crops. At the same time the recovery of industrial output during the New Economic Policy was achieved at the cost of the depreciation of the capital stock inherited from the Tsarist regime. Now the period of socialist reconstruction was at hand. With the source of accumulation declining even as the needs of the economy

for net investment were expanding, how was the bill for industrialization to be paid? In the second place, while seeking planning and socialist solutions to these problems, the Russians debated issues of wide general interest and in fact anticipated many that have subsequently come to absorb the attention of Western economists. Here one finds discussions of input-output analysis, national income accounting, investment efficiency, growth models, sectoral priorities in development, balanced versus unbalanced growth. Had not Stalin choked off the debate in the purges and repressions of the 1930's, it is not unreasonable to conjecture that Russia might have established itself at the forefront of the economics of growth and development. But in the event the opportunity passed, and Soviet economics succumbed to a dreary litany of Marxian platitudes.

In view of the fact that these early concerns of Soviet economics have become central to much contemporary work in Western economics, it is surprising that so little attention has been paid to them in the Anglo-American world. Aside from contributions by Maurice Dobb, Evsey Domar, and especially Alexander Erlich, there is little to go by. The two volumes under review, therefore, are all the more welcome. Professor Spulber has written a long essay on the issues in the debate and the positions taken by the *dramatis personae* which can serve as an introduction and guide to a companion volume of translated papers drawn from economic sources—journals, reports, books—published in Russia at the time. Now for the first time the English reader can witness the debate from the inside.

It should be said at once that the debate thus seen is not always easy going. The papers are occasionally dull, sometimes pompous ("But to the mathematical logic of the S-curve, the proletariat has opposed the dialectical logic of socialist growth" [*Foundations* . . . , p. 303]), and too often polemical. But these are shortcomings that Western economists can hardly disclaim. More difficult is the Marxian medium in which much of the debate is conducted. Not only do the reproduction tables in the second volume of *Capital* serve as the point of departure for the discussions of growth, but the papers are laced throughout with the jargon of Marxian economics. Fortunately for those economists whose knowledge of Marx is rusty, Spulber's excellent essay is ready at hand. Here in familiar idiom one may first become acquainted with the economic problems confronting the Soviet Union in the 1920's and the various solutions suggested by the participants in the debate.

The translated papers are grouped under four sections (Macro-Economic Models; Economic Growth: Strategies of Development; Economic Growth: Pace and Efficiency; and Planning Theories and Methods) and in each one readers will find selections of exceptional interest. This reviewer found particularly stimulating the contributions of E. A. Preobrazhenski, a member of the left opposition and a strong advocate of forced industrialization; V. Bazarov, a general-equilibrium critic of so-called teleological planning; and N. D. Kondrat'ev, a supporter of growth through agricultural development. Economists who like to play with growth models will find much substance in the papers of G. A. Fel'dman. Also of more than passing interest is a short

critical note by W. Leontief on "The Balance of the Economy of the USSR" (first published in *Planovoe khoziaistvo* in 1925). N. I. Bukharin and J. Stalin are represented, and though their pieces are polemical, they are an appropriate reminder of the rugged political milieu in which the debate was carried on.

One over-all impression strikes the reader of these papers forcibly: none of the contributors (Stalin's paper was dated November 24, 1928) anticipated the drastic changes that were shortly to take place in Russia with the collectivization of agriculture. Whatever their view of planning, however much they thought the pace of industrialization could or could not be hastened, they assumed that socialist society in Russia should maximize consumer satisfaction over the coming 10-15 year period. Because this premise was widely accepted, there could be a fruitful dialogue. By the same token, however, it lends an air of unreality to the proceedings simply because we view them from the present day with the full knowledge that Stalin made capital accumulation a domineering end seemingly unrelated to consumption. Yet the economic issues themselves are universal and indeed are being debated today the world around. Spulber is to be congratulated for giving wider currency to the Russian contribution to the discussion of these issues.

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*Contemporary Economic Systems—A Comparative Analysis.* By CARL LANDAUER. Philadelphia and New York: J. B. Lippincott Co., 1964. Pp. x, 560. \$8.95; text, \$7.00.

The object of comparative economics is to study the *modus operandi* of contemporary economic systems, i.e., the ways in which they solve the problems of how, what, and for whom to produce, and to show how these systems interact, i.e., in which ways their differences lead to conflicts or to mutual accommodations.

Unfortunately, many of the available textbooks tend to dwell at length on the *modus operandi* of capitalism in a sort of sanitized, purely competitive setting and fail to point out how, when, why, and in which ways diversity arises under capitalism. Further, some authors tend to classify systems by the main instrument they use, e.g., reliance or nonreliance on the market for key decision-making, rather than by the form of ownership on which each system rests and which determines its respective dominant drive, e.g., the profit motive under capitalism. Others stress—and this reviewer inclines toward this school—that the differences among systems arise from the predominant form of ownership prevailing in them and the drive it generates, while differences within each system arise under a variety of impacts from the ways in which the instrumentalities—market mechanisms or planning and administrative devices—are combined with one another. But, since there is no consensus on definitions, many textbooks tend to be obscure on these crucial points, or worse, tend to change criteria from one chapter to the next. Finally, while the economics of free enterprise is presented within the simplified

standard frame suitable for instruction in economics, in which historical or geographical material need not be called in to the rescue, the economics of communism is usually presented in the historical perspective of the Soviet Union's economic evolution. The tendency to identify any system with its preferential instruments rather than with the forms of ownership on which it rests, and the pursuit of analysis on different theoretical planes, leads to a static and disjointed analysis, in which the systems analyzed are presented in an orderly fashion like cake layers, each of a different substance and of a different consistency. But one can hardly visualize in all this either the patterns of convergence of these systems or the ways in which these systems interact. The main object of the whole effort is thus somehow lost in the process.

In terms of space devoted to each system, Professor Landauer attempts to strike a fair balance: to capitalism, identified by him with the U.S. economy in its changing aspects, he devotes 135 pages, and to communism, centered on the USSR and Yugoslavia's evolution, he allots some 170 pages. Capitalism is defined primarily as a system of "relatively unrestricted rights of private property"; Soviet-type communism, as a system of primarily collective ownership. Between the two systems, Landauer places an 85-page section which he calls "socialism." The latter is identified by him, though not quite explicitly, by a sort of capitalism in which nationalized or cooperative enterprises are established in a more or less extensive way. "Socialism" is further deemed to be closely associated with tendencies toward planning and welfare policies. These definitions cover roughly, but not exactly, what the French usually call "capitalisme dirigé," that is, guided capitalism, or "étatisme," that is, broad state interventions in the management of the economy as a whole. Landauer ranges in his "socialist" section from Great Britain for both expansion of state-owned enterprises and of government control over them during the Labour government to present-day France more for its planning than for the extent of its nationalizations, to Sweden primarily for its cooperatives and its welfare policies, and then to Israel for its *kibbutz* collective agriculture. Landauer's "socialism" is situated in a sort of limbo: it is not clearly included under capitalism and not truly related to the Soviet Union's communism, as the latter is characterized by comprehensive public ownership and by nonreliance on market signals. One wonders why socialism so defined includes only developed countries: it could just as well apply to Mexico, or Turkey, India, or the U.A.R., Ghana or many others—some of which are placed by Landauer for not very compelling reasons into a short section appended after "Communism" titled "Primitive and Underdeveloped Areas." This separate "socialist" section obscures rather than illuminates the wide capitalist and communist spectra. Further, the treatment of capitalism and communism is pursued on different planes, with the Soviet and Yugoslav communisms treated in historical perspective, with the usual negative results as previously mentioned.

From the outset Landauer rightly stresses that, as far as instrumentalities are concerned, all systems are reducible to the same components: all coordinate economic activity by command and/or by exchange; all are laced by

ties of solidarity and self-interest; all are tinted in various ways by traditions and customs. As in architecture, there are many differences among systems in style and design but little variety from brick to brick. Actually certain combinations of basic building stuffs can be erected on the soil of either a predominantly private or predominantly collectivized economy.

Currently the Soviet-type economies tend toward diversifying their "brick arrangements." With respect to the management of state property, there is a variety of tendencies toward decentralization. Regarding planning strategy and procedure, there are many tendencies toward wider utilization of market mechanisms and rationalization of prices. Concerning technological options, there are strong departures from the traditional Soviet models. While in respect to property relations the capitalist and the Soviet-type communist economies remain very different, in terms of instrumentalities they tend often toward convergent solutions. The same types of problems appear, and roughly the same types of solutions are envisaged and implemented in both systems. On this basis, Landauer suggests that capitalism and communism "will eventually meet under the sign of socialism—provided the present tendencies are not stopped or reversed before the meeting takes place" (p. 537). This reviewer feels that the two evolutions often take place along parallel lines, but two such structurally diverse species will continue to remain entirely apart, though they tend to develop similar "protective" adjustments to a changing environment.

Although I differ with Landauer's structural emphasis, there can be no question concerning his handling of the basic concepts which he presents with precision and authority.

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### **Business Fluctuations**

*Pricing and Employment in the Trade Cycle—A Study of British Manufacturing Industry, 1950-61.* By R. R. NEILD. National Institute of Economic and Social Research Occasional Paper 21. New York: Cambridge University Press, 1963. Pp. viii, 73. \$3.00.

Robert Neild has produced a compact, high-quality econometric study on the cyclical determinants of prices, profits, and employment in the manufacturing sector of the United Kingdom. He presents a short comparison of U.K. and U.S. cyclical profit behavior. The least-squares price equations are consistent with the view that prices are fundamentally a markup on long-run unit labor and material costs. Prices do not respond to short-run variations in unit labor costs as they do to long-run trend movements. A measure of demand pressures turns out to be statistically insignificant. While this result could, as usual, arise from data imperfections, reaffirmation of the overwhelmingly cost-oriented response of price surely reflects strong elements of non-competitiveness in manufacturing.

Once strong statistical support had been found for a theory based on long-

run price decisions, however, the dynamic response of prices to wage and raw-material costs (for which U.K. indexes appear distinctly superior to those for the U.S.) is similar: two quarters after either cost change, prices have increased by about seven-tenths of their total response. Long-run implications for profits are not immediately obvious, although parameter estimates imply that an increase in unit labor and unit material costs will be reflected in an equiproportional increase in prices, suggesting a price policy consistent with the constancy of long-run factor shares.

The labor-employment relations for manufacturing provide further illumination about the short-run determinants of labor productivity, a matter of genuine importance, because:

If in a recession productivity falls below the trend level, profits per unit will be squeezed. How far this will happen will depend on the extent to which firms adjust employment and working hours when output changes. If, as an extreme example, employment and hours are not changed at all, output per man will change in proportion to output; labour will be in the nature of an overhead cost. In that event, labour incomes will be constant and an initial fluctuation in demand (say, in exports) will lead simply to a change in profits. Since short run variations in profits are reflected mainly in changes in company savings and in tax payments, the effect on consumer spending will be small: the multiplier will be low. If, on the other hand, firms quickly adjust employment and hours, an initial movement in demand will impinge more strongly on labour incomes and less on profit margins: the multiplier will be higher. Thus the extent to which employers adjust their employment and hours governs the value of the short run multiplier (pp. 28-29).

Generally speaking, theoretical implications and statistical quality of the employment and man-hour demand equations, as a function of lagged outputs and trend, are quite different. The explained fraction of variance of the quarter-to-quarter change is .74 for employment and only .60 for man-hours. The impact of output in the man-hours equation is more heavily concentrated in the current period than in the employment equation since hours are more rapidly adjusted than employment. A disquieting implication of these estimates, recognized by Neild, is that the long-run employment increase is only about one-third to two-fifths per cent in response to a maintained 1 per cent increase in output according to the preferred sets of estimated parameters. This seems much too low if similar analyses of U.S. manufacturing have the slightest relevance. The implied extent of increasing returns to scale (a scale factor between two and three for a homogeneous function) appears implausible. Explicit recognition of the distinction between the dynamic employment response of production and nonproduction workers ought to improve conformity to theoretically more acceptable parameters.

Neild compares the cyclical sensitivity of profits in the United Kingdom and the United States. He concludes that the sharper cyclical variations in U.S. gross corporate profits (measured as deviations from trend) are more closely related to price-flexibility than to labor-productivity variations, be-

MAN-HOUR INPUTS, OUTPUT, AND PROFITS IN U.S. MANUFACTURING DURING  
POSTWAR RECESSIONS

(Quarterly Data—Per Cent Changes)

	Peak-Trough <sup>a</sup>		Rapid Output Decline <sup>b</sup>	
	(1) Production Worker Hour Basis	(2) Weighted Average Production and Nonproduction Worker Hour Basis	(3) Production Worker Hour Basis	(4) Weighted Average Production and Nonproduction Worker Hour Basis
	1953-II:1954-I		1953-III:1953-IV	
Output	- 9.7	- 9.7	- 6.1	- 6.1
Man-hours	- 9.2	- 8.6	- 2.9	- 2.9
Employment	- 6.2	- 6.2	- 2.9	- 2.9
Hours	- 3.1	- 2.5	0.0	0.0
Net Profit	28.1	28.1	32.0	32.0
Gross Profit	21.9	21.9	24.8	24.8
	1957-I:1958-II		1957-III:1958-I	
Output	-13.0	-13.0	-12.3	-12.3
Man-hours	-13.4	12.7	-10.8	-10.0
Employment	-10.3	-10.3	- 7.7	- 7.7
Hours	- 3.5	- 2.7	- 3.3	- 2.5
Net Profit	34.4	34.4	31.5	31.5
Gross Profit	25.0	25.0	23.2	23.2
	1960-I:1961-I		1960-III:1961-I	
Output	- 8.5	- 8.5	- 5.5	- 5.5
Man-hours	- 7.8	- 7.2	- 5.0	- 4.6
Employment	- 5.4	- 5.4	- 3.6	- 3.6
Hours	- 2.6	- 2.0	- 1.5	- 1.1
Net Profit	-28.6	-28.6	-16.9	-16.9
Gross Profit	-19.8	-19.8	-11.1	-11.1

<sup>a</sup> According to text, p. 46.<sup>b</sup> Periods based on visual inspection of graphs.

## Sources:

Row (1) Federal Reserve Board Index of Manufacturing Production: Per Cent Changes using quarterly averages of monthly data. The denominator is the initial value for which the numerator difference is calculated.

Row (2) The product of BLS data described in rows (3) and (4) below.

Row (3) Employees in manufacturing, data collected monthly by the U.S. Department of Labor, Bureau of Labor Statistics, and published in various issues of biennial *Business Statistics*, a supplement to the *Survey of Current Business*.

Row (4) Columns (1), (3)—Average weekly hours per production worker in manufacturing; data collected monthly by the U.S. Department of Labor, Bureau of Labor Statistics, and published in various issues of biennial *Business Statistics*, a supplement to the *Survey of Current Business*. Columns (2), (4)—Same for production worker hours as columns (1), (3), and constant 40 hours per week for nonproduction workers.

Rows (5)–(6) *Quarterly Financial Report for Manufacturing*, FTC-SEC. Net profit—before tax, with depreciation deducted. Gross profit—before tax, but depreciation not deducted.

cause the United States had larger per cent deviations from trend in profits and smaller variations in productivity than in the United Kingdom. "In the [U.S.] downswing there is usually some temporary decline in output per man-hour because manhours are not cut quite as fast as output. But by the time the trough in output is reached, manhours have generally been reduced in almost the same proportion as output . . ." (p. 46). These inferences are open to question. First, the "temporary decline" in productivity during periods of rapid output decline often is substantial, as columns (3)-(4) of the table indicate. These quarter-to-quarter calculations relating to profit and productivity provide more powerful evidence than the annual profit data used by Neild. His choice of peak-trough comparisons obscures characteristic profit-productivity relations since slight changes in the end points make a drastic difference in the reported comparisons, as careful examination of the table reveals. The cogency of these observations cannot be fully established until an analogous treatment of British data is available.

Second, Neild estimates all variations in hours worked from the one available series, that for production workers. While nonproduction-worker-hours are not available on a convenient basis, a more defensible assumption is that nonproduction employees work a constant 40 hours per week. Thus the total drop in hours has been overestimated, which makes a modest difference, as reported in columns (2) and (4) of the table. Since comparable U.K. nonproduction labor is only slightly less, differential U.K.-U.S. behavior would be only slightly affected by this adjustment.

The main purpose of this study, to improve national income forecasting procedures, has been well served. Of more lasting value, however, are the quantitative and qualitative insights into the short-run dynamics of an advanced industrial economy.

EDWIN KUH

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*American Growth and the Balance of Payments 1820-1913—A Study of the Long Swing.* By JEFFREY G. WILLIAMSON. Chapel Hill: University of North Carolina Press, 1964. Pp. xviii, 298. \$7.50.

The increased willingness of economists to accept the view that long swings or Kuznets cycles did exist in the nineteenth-century U.S. domestic economy has raised the question whether such long swings also existed in the foreign sector? Were the flows of people, commodities, securities, and gold across U.S. borders also subject to long swings, and were these swings dominated by the U.S. economy or by economic fluctuations in other parts of the world, particularly in the United Kingdom? A decade ago Brinley Thomas examined the relationship of the long swing and the flow of European migrants to the United States. Jeffrey Williamson, using data relating to nineteenth-century U.S. foreign trade and capital flows developed by North, Simon, and others has now extended this investigation to the existence and source of long swings in the flow of commodities, net capital, and gold over the period 1820-1913. Williamson finds that the major items in the balance of payments were subject to long swings; long swings in domestic development were the principal

cause of the fluctuations in the trade balance and net capital flows; the flow of specie acted only to complete the real transfer already initiated; and the Kuznets cycle in the United Kingdom, insofar as it is identifiable, likely had its source in the United States.

Although nineteenth-century data relating to price changes, gold flows, and net capital flows have their limitations, and despite the adjustments needed to isolate the long swing, Williamson offers a convincing case for the existence of Kuznets cycles in imports and net capital flows through most of the century and the idea that these fluctuations were related to the domestic Kuznets cycle as represented by transport-building, construction, and other indexes of industrial output. Since the long-run state of the balance of payments depended on the growth of U.S. output along with the rates of return on capital and changes in the price level in Europe and the United States, given the structure of comparative advantage in the United States and Europe up to the 1890's, one is not surprised to find that such a relationship existed. What is less certain, despite the regression-analysis presented here, is the lag of imports and net capital inflows behind domestic activity, especially at the lower turning point. The evidence seems to support the independence of the U.S. swing, but it is not overwhelming. For an earlier period, the impact of the American colonies on the upswing in British economy activity in the period 1740-60 has yet to be settled. For the period 1820-60, Williamson's analysis adds little to North's explanation of Anglo-American economic relations, and the comments of Hughes and Rosenberg still hold good (see *The Economic History Review*, Vol. XV, No. 3, April 1963). Finally, the timing of net capital exports from Great Britain, the most obvious evidence of the Kuznets cycle, requires a more detailed analysis of the existence of long swings in other countries if the source of the British swing after 1860 is to be found in the United States.

The major shortcoming of Williamson's thesis is his exclusion of short-term capital flows, particularly as these are influenced by long swings in domestic output. It is unlikely that a satisfactory explanation of the long swing will be found without a detailed study of short-term capital flows within the whole Atlantic economy. Such a study might indicate that short-term capital flows financed the flow of imports into the United States in the initial stages of the upswing and that long-term capital exports from Great Britain did not take place until later. Under these circumstances British exports of capital to the United States to a substantial extent would be independent of U.S. transport development, though not of the long-term growth of U.S. exports. Williamson's limited statistical test of the U.S. "pull" and the British "push" as they affected capital flows over the period 1871-1914 tends to support this view. The imperfect knowledge available to the British saver, who was essentially averse to risk, produced a lagged recognition of the profitability of U.S. transportation investment both in the upswing and the downswing. The long-term growth of exports, which did not undergo long swings after 1845, enabled hope to spring eternal in the hearts of disappointed British investors. The consequences of such behavior for a capital-stock adjustment theory of the long swing are evident.

What then can be said about Williamson's contention that the concept of the long cycle is useful not only for analysis of the historical past but for the formulation of current economic policy, since the author implies that, if the United States followed a policy of achieving rapid growth and full employment, the balance of payments problem would tend to disappear in the long run? Some support for this thesis can be found in the Report of the Royal Commission on Banking and Finance recently issued in Canada. The Commissioners seem to endorse the usefulness of the long cycle as a guide to economic analysis and policy formulation within the Canadian economy and imply that a high level of economic growth has tended to ease Canada's balance of payments problem for much the same reasons as those outlined by Williamson.

It is evident that the experience of the Canadian economy in the postwar period, while tending to support an explanation of U.S. economic behavior in the nineteenth century, may be of little relevance to the United States today. However, it is significant that the Commissioners also failed to integrate adequately the long-run and short-run aspects of economic growth. When the United States was on the gold standard in the nineteenth century, Williamson finds a positive correlation between the rate of economic development and the rate of gold flow. Periods of rapid economic growth were characterized by net capital inflows that not only permitted maximum growth to continue over long periods of time but added to domestic growth by supplying a widening basis for the expansion of liquidity in the upswing. How far the movements of short-term capital contributed to this adjustment and the extent to which such movements were a function of domestic economic growth remain unexplained. Whatever may be the case for the twentieth century, the analysis of nineteenth-century short-term capital flows will not be easy. For example, to track down the short-term capital held by Americans in foreign banks is a task that will challenge the most astute economic detective. He would be well advised, however, to begin his search by reading this excellent guide book.

JOHN J. MADDEN

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### **Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit**

*Money, Capital, and Prices.* By GEORGE HORWICH. Homewood, Illinois: Richard D. Irwin, Inc., 1964. Pp. xiv, 556. \$12.50.

Professor Horwich's volume is the first in a new monograph series sponsored by Purdue's Krannert Graduate School of Industrial Administration and Richard D. Irwin, Inc. According to the publisher, additional volumes in the series are anticipated which will report on experimental studies of economic behavior, econometric studies of the American economy, new managerial and production techniques, and studies of economic development.

The first volume sets a very high standard for those which are to follow. It

is not a book to be merely read, however. Instead, it needs to be studied most carefully, and then restudied, for one to be at all reasonably sure of achieving full comprehension of the author's argument and its implications.

Let us not fault Horwich for this, though, but view it as the inevitable result of his having worked his analysis out most carefully and elaborately and at a very high level of abstraction. Also, he is dealing with a complicated subject that simply does not lend itself to treatment in an uncomplicated way. Thus, as he notes:

. . . the analysis is highly aggregative and contains little more than the minimum number of variables needed for a complete stock-flow system. But even this number creates an analytical network whose inter-relationships are sometimes both numerous and complex.

The volume has been long in preparation (Horwich indicates that it had its beginnings in his doctoral dissertation, submitted in 1954). What has finally emerged is a closely reasoned and very detailed description of, first, the impact of externally induced monetary change on an aggregative model of a closed economy; and, second, the resultant process of adjustment within the model to the monetary "disturbances" introduced. Throughout, the theoretical methodology employed is that of general equilibrium analysis.

All this is contained in ten lengthy chapters, accompanied by seven appendices. The first chapter provides an overview of the whole analysis. The second builds the model into and within which, in Chapters 3-6, monetary disturbances (resulting from central bank open-market operations) are introduced, and the adjustment processes flowing from them are depicted and analyzed in great detail. Then, in Chapters 7-9, the preceding detailed analysis is generalized in various ways: by altering the model (e.g., a two-security instead of a one-security model, to create opportunity for viewing monetary disturbances against the backdrop of the interest structure); by assuming *continuous* instead of "once-for-all" open-market operations as the source of monetary disturbance; by assuming other sources of monetary disturbance than open-market operations—*independent shifts in savings and investment, government budgetary deficits and surpluses, independent changes in active velocity, shifts in existing-asset demand, and autonomous movements between idle and active balances; and by introducing a sector of financial intermediaries.*

The concluding chapter provides a restatement and critique of interest and price theory. The writings of Patinkin and Keynes are viewed as providing the major alternatives to the analysis developed by Horwich. But to Horwich, whose very first paragraph stipulates that "In a sense, stock-flow analysis and fidelity to stock-flow relationships of the monetary system are the central themes of this volume," Patinkin's use of the "real-balance effect" blurs, rather than clarifies, stock-flow concepts. Other shortcomings of the "Patinkinian" approach include:

Denial of the medium-of-exchange function of money and the traditional income-expenditures relationship (as a consequence of Patinkin's attack

on the "dichotomy" of the real and monetary sectors, as entailing an indeterminate price level); and

Disregard of the store-of-value function of money (as a consequence of Patinkin's claim that a system in which saving and investment are exclusively a demand for and supply of securities also has an indeterminate price level).

Keynes and, among his latter-day interpreters, Lloyd Metzler fare no better than Patinkin at Horwich's hands. The liquidity-preference propositions bear the brunt of the buffeting here. For example, Horwich concludes, from his own lengthy and detailed analysis, that the liquidity-preference theory (1) is incompatible with the view that saving and investment cannot directly affect the rate of interest, and (2) involves a confusion between the allocation of additional saving to cash balances and the holding of additional cash.

Amidst such clashing views on topics in which the reviewer himself has more than a passing interest, it is difficult indeed to resist the temptation to take up the cudgels oneself. But when a review is all that has been requested, a review *article* should not be permitted to result. Thus the temptation will be resisted, and further comment limited. It should be said before closing, however, that Horwich ought to be highly commended for making a substantial contribution to the literature. His analysis is complex and quite difficult to assimilate; but it is also frequently illuminating and often extremely provocative. Not the least of its merits, moreover, is the fact that it provides us with just the solid basis for healthy controversy through which we may hope that the present condition of monetary theory will be most rapidly improved.

CLIFTON H. KREPS, JR.

*University of North Carolina*

*Impacts of Monetary Policy.* A Series of Research Studies Prepared for the Commission on Money and Credit. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1964. Pp. 688. \$7.50.

Each of the contributors to this collection of CMC supporting papers (Suits, Eisner and Strotz, Kuh and Meyer, Grebler and Maisel, Phelps, and Friend) was charged with the task of summarizing and appraising the present state of knowledge of the factors determining spending in a major economic sector. Special attention was to be accorded to the question—is spending in a given sector responsive to general monetary policies? Most of the contributors to this volume would apparently answer, "Who knows?" or words to that effect. Considering the thoroughly Keynesian tenor in which the question was generally interpreted, this uncertain response would have to be rated as a concession to the claims of resurgent monetary theory.

Daniel Suits and Irwin Friend, in separate papers, scrutinize two sides of virtually the same coin—the determinants of consumption (Suits) and savings (Friend). There are, of course, important differences of emphasis. Suits examines the factors influencing consumer spending on durables, nondurables, and services, while Friend is concerned with the composition of savings and with business and governmental as well as consumer savings. But in

their discussion of aggregative consumer behavior, they cover similar ground, and share a common reluctance to admit that consumption may be a more stable function of wealth or permanent income than of current income, a common affinity for the distribution of income as an important, albeit unverified, influence on saving, and a common skepticism that monetary forces have much effect on consumer expenditure behavior. Friend looks for the effect primarily in the relation between interest rates and savings and finds practically none; Suits looks for it mainly in relations between liquid assets or consumer credit terms and consumer expenditures on durables and finds very little. One wonders if Suits, in the light of the Friedman-Meiselman study, would still say that "none of the studies of the effects of monetary factors on consumer behavior has succeeded in showing an unambiguous, statistically significant, important direct effect." Surveyors of knowledge are of course entitled to a few preoccupations with pet ideas. For Suits it is the interesting suggestion that family consumption is more autonomous and family income less autonomous than is generally supposed; for Friend it is the shopworn suggestion that an increase in desired savings may be self-defeating, for the usual "paradox of thrift" reasons.

Robert Eisner and Robert Strotz assay the determinants of business investment in a joint paper. The result is an important contribution to the literature on investment, thanks largely to the excellent chapter on the theory of investment of the firm by Strotz. Among other things, it is shown by a straightforward application of the calculus of variations that the distinction between fixed and variable factors of production is based on an economic consideration, namely that of maximizing a discounted net profit functional in which the cost of plant expansion depends on the time rate of expansion and the promptness with which the planned expansion is completed. Formulating the investment problem in this manner enables Strotz to provide a theoretical justification for the commonly used distributed-lag investment function in which the weights decline geometrically. Also, it becomes possible to provide a case demonstration that higher interest rates should tend to slow down the rate of plant expansion (i.e., investment). This dynamic or "interequilibrium" approach is distinguished from the Fisherian intertemporal theory of optimal investment. Regarding Fisher's approach, it is shown that multiple or non-existent "internal rates of return" have satisfactory economic interpretations although the internal rate of return is defective as a general investment criterion.

The bulk of the Eisner-Strotz paper consists of summaries and evaluations of the principal quantitative investigations of investment in plant and equipment and inventories. (George Post's useful bibliography of articles on investment, 1945-61, is appended to the paper.) One lesson to be learned from these investigations is to beware of the proxy variable—it may wander away from any close relation to the variable it purports to represent. Eisner and Strotz prefer modified capacity and acceleration models of business investment to those which stress profits and liquidity because the latter have no support in a profit-maximizing motivation of investment behavior and be-

cause profits may be merely a proxy for a missing accelerator mechanism. As to the effectiveness of monetary policy, the authors are of at least two minds. Eisner sees monetary policy as unable to affect the long-term rate of interest owing to the unresponsiveness of expectations of future short-term rates to the tentative reversible open-market operations that characterize anticyclical monetary policy. This is said to rule out any direct effect of monetary policy on long-term investment. Strotz is less adamant on this score, more willing to consider the possible indirect influence of monetary policy on investment via its influence on final demand, and more willing to admit that "changes in the terms of credit may be poorly represented by the tabulated changes in interest rates." He places considerable emphasis on the possible role of trade credit and other private sources of liquidity in affecting inventory investment.

Edwin Kuh and John Meyer search for new reasons why monetary policy need not have much influence on plant and equipment expenditures. One motive for the search is that they recently unearthed somewhat stronger evidence of an interest elasticity in investment demand among manufacturing industries than most previous studies, including their own, had shown. The most important new reason discovered is that even if business investment demand is interest elastic, variations in the level of interest rates might not affect the level of business fixed investment, owing to peculiarities of the investment funds supply schedule. If investment is financed more or less entirely from internally generated funds and the supply price of such funds is not responsive to market interest rates, then monetary policy is said to be ineffective. Meyer and Kuh's examination of the cyclical flow of funds in manufacturing suggests to them that, except near the cyclical peak in business, there is very little reliance on external financing of investment. Even this dependence has dwindled with the steady postwar increase in the portion of internal cash flow contributed by depreciation allowances. And in public utilities, heavy reliance on external financing does not mean monetary policy has an effect, because for various special reasons, the investment demand schedule of public utilities is said to be interest inelastic.

Despite their admission that "business liquidity and business financing can be influenced by monetary policy," Meyer and Kuh continually ignore the inference that monetary policy can affect investment via business liquidity. Repeatedly they equate monetary policy with manipulation of market interest rates and find this to be an ineffective stimulus to business investment. But the process whereby excessive business liquidity stimulates increased business expenditures—for inventories, plant and equipment, and the like—is a standard example of the equilibrating mechanism of the quantity theory of money. The quantity theory is, after all, most fundamentally a theory of the relation between money and expenditures, not between a small set of interest rates and expenditures or between money and a small set of interest rates. If they are properly interpreted, ten more such studies by Meyer and Kuh and the future of monetary economics in Cambridge, Massachusetts, might be assured.

Leo Grebler and Sherman Maisel offer a lengthy review of existing knowledge (or ignorance) of the determinants of residential construction. Between

incomplete models and faulty data the authors find much to discuss and little to praise. Verbal or qualitative models of construction activity have suffered from arithmetic triviality, untested or untestable propositions, and vague assumptions. Econometric models have not lived up to their promise, owing to the usual grab bag of statistical flaws (multicollinearity, serial correlation, errors in data, nonlinearity, least-squares bias) and ill-conceived specifications of underlying economic relationships. Worst of all, as Grebler and Maisel show in their useful series of tests on new data, the econometric models of construction generally forecast poorly by comparison with "naive" methods. However most of the models tested seem clumsy and unimpressive. A serious omission from the survey is Richard Muth's study of the demand for residential housing, which assimilates housing demand with capital theory, as it should be. One can agree with Grebler and Maisel's conclusion that more thorough models and better data are needed. It might also help if researchers in this field concentrated their attention on a few topics whose fundamental importance warrants the effort to improve both the theory and the data.

Charlotte Phelps examines the impact of higher interest rates on municipal capital expenditures in the 1955-57 period. She found that "from 4 to 7 per cent of municipal capital expenditures was postponed and/or cut back due to tightening credit." This seems like a trifling effect, but Mrs. Phelps apparently concludes that monetary policy has a significant influence on municipal investment.

GEORGE R. MORRISON

*Cornell University*

*The Term Structure of Interest Rates.* By DAVID MEISELMAN. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1962. Pp. xii, 75. \$4.50.

The lack of independent evidence on expectations of future interest rates has been the chief obstacle to effective empirical testing of the expectations theory of the term structure of interest rates. Previous empirical work assumed that, if expectations did determine the term structure, one ought to be able to observe a correspondence between the forward short-term rates implied by the yield curve at a given moment of time and the short-term rates which later were actually observed. Such studies usually found that investors had been doing such a bad job of forecasting that it seemed doubtful that any forecasts had been made at all. In this Ford Foundation award-winning dissertation, David Meiselman argues that perfect-foresight approaches do not constitute valid tests of the expectations theory: "Anticipations may not be realized yet still determine the structure of rates in the manner asserted by the theory" (p. 12). Meiselman's contribution is the construction of an error-learning model which describes how expectations change over time with the receipt of new information. With this model, he is able to perform tests of the expectations theory which do not depend upon accurate forecasting for their validity.

Meiselman's conceptual framework is easily grasped with the aid of a simple example. Assume that as of the beginning of 1965 the one-year market

rate is 2 per cent, the two-year rate, 3 per cent, and the three-year rate, 4 per cent. Through the Hicks-Lutz averaging mechanism, this term structure may be considered to imply that the one-year rate expected for next year (1966) is 4 per cent, and the one-year rate expected for the following year (1967) is 6 per cent. Now let one year pass, and assume that the actual one-year rate turns out to be 3 per cent (100 basis points lower than had been forecast). An error-learning approach has investors respond to their error by lowering their forecasts of still-unrealized forward rates. For example, the expected rate for 1967, which had been forecast at 6 per cent during 1965, will now be reduced. Since, under the expectations theory, longer rates are averages of present and forward short rates, forecasting errors cause the whole structure of rates to fall.

Formally, the error-learning hypothesis may be written:<sup>1</sup>

$${}_{t+n}r_{1,t} - {}_{t+n}r_{1,t-1} = f(R_{1,t} - {}_{t+1}r_{1,t-1}).$$

Assuming the relationship to be linear, Meiselman estimated the equation

$$\Delta {}_{t+n}r_{1,t} = a + b(R_{1,t} - {}_{t+1}r_{1,t-1})$$

employing the annual Durand data. He found that changes in forward one-year rates were highly correlated with the forecasting errors and concluded that his tests provided impressive support for a modified expectations theory, one buttressed with an error-correction mechanism.

Difficulties are just now coming to light. Both Kessel and Wood have shown independently that it is possible to reinterpret the

$$(R_{1,t} - {}_{t+1}r_{1,t-1})$$

term as composed of a forecasting error and a liquidity premium. As a result, Meiselman's estimates do not discriminate between the pure and liquidity-premium versions of the expectations theory. Other critics have suggested that certain characteristics of the Durand data call the tests seriously into question. Applying the model to differently-constructed British data, Grant finds that Meiselman's hypothesis cannot be relied upon as an efficient explanation of the term structure. Grant's results seem to indicate that the construction of the Durand yield curves (particularly the methods of smoothing) may be partly responsible for Meiselman's surprising results. I have recently argued that behind Meiselman's work lies a certain view of the mechanics of the securities market which is in conflict with the actual workings of the market. Once one drops the untenable assumptions of costless arbitrage and identical expectations, relative supplies do affect the term structure even if market participants are not segmented. From a policy standpoint, the matter is an important one since a pure expectations theory suggests that any attempts deliberately to alter the rate structure would prove nugatory.

In sum, Meiselman claims too much for (his interpretation of) the expectations theory by suggesting that it can offer a complete explanation for the

<sup>1</sup> Capital  $R$ 's represent actual rates, lower case  $r$ 's are expected or implied rates. The prescript gives the time period when the rates become applicable. The first subscript stands for the maturity of the bond, and the second subscript tells when the rate is observed in the market.

term structure. Kessel's recent work, showing that short-term yields have on the average been significantly below long-term yields, indicates, at the very least, that factors other than expectations must be accorded an important role in a complete explanation. A realistic theory requires that we take a more eclectic view of the determination of the term structure.

To express skepticism concerning Meiselman's major conclusion in its most unqualified form is not to deny that his study has been immensely valuable. In an adroit opening chapter he has succeeded in clarifying several theoretical issues, particularly those concerning the Hicksian liquidity-premium analysis. In addition, he has devised and executed an ingeniously simple empirical test which has helped dispel a cloud of suspicion from the expectations theory. I hope that Meiselman's model will be applied to other interest-rate data and that it will be adapted to other financial instruments. An error-learning approach has much to offer in helping to explain price relationships in the equity market. No serious student of financial markets can afford to disregard this study.

BURTON G. MALKIEL

*Princeton University*

*Full Employment or Stagnation?* By J. M. CULBERTSON. New York: McGraw-Hill Book Co., 1964. Pp. vii, 252. \$5.95.

In his preface, Professor Culbertson states that "The high unemployment level and weak economic growth of the United States in recent years are . . . the cost of misguided government economic policies. The errors of policy, in turn, are not accidental, but rest upon certain fallacious economic ideas." The "fallacious economic ideas" which the author angrily attacks are those of the "traditionalist," the related "banking school," and the "Keynesian."

The traditionalist approach is one which finds virtue in economic discipline and privation. Its proponents call for belt-tightening and for fiscal responsibility. A recession is considered to be a needed and desirable dose of salts for the economy. For the traditionalist, an increase in the quantity of money invariably produces a rise in the price level. The banking school, on the other hand, accepts monetary expansion as both desirable and necessary so long as it "meets the needs of trade." The members of this school consider the volume of credit, rather than the quantity of money, to be the relevant target variable for monetary policy. According to the author, "Keynesians" are fellow travelers. Their doctrine is just as objectionable as the other two. In tacit accord with the traditionalists, the "Keynesians" accept both monetary restraint during periods of economic slack and procyclical changes in the maturity of the federal debt because they believe these factors to be unimportant. They do not refute the "needs of credit" concept because they do not believe that it does any harm.

Culbertson discusses these three doctrines throughout his book, and he adequately demonstrates the fallacies in their arguments. His most effective weapon against the traditionalist and banking-school doctrines is the liberal use of quotations taken from statements made by public officials. The pri-

mary target of the attack is Chairman Martin of the Federal Reserve System who is fond of using such phrases as "leaning against the wind," "proper flow of funds," and "redundant money" to characterize monetary policy.

Culbertson's refutation of the "Keynesians" rests on the observation that the velocity of money cannot become infinite. The economy cannot expand indefinitely with a fixed stock of money. If this proposition is true, then the quantity of money must make a difference, the author argues.

Culbertson discusses the policies which he believes resulted from the three doctrines. Among these are (a) the absolute decline in the quantity of money (demand deposits plus coin and currency) which occurred in 1959; (b) the slow rate of growth in the money supply during the 1960-62 period; (c) the attempts to lengthen the average maturity of the outstanding debt in 1960 and again in early 1963; and (d) the lack of increase in federal expenditures from late 1958 to 1960. The author argues that these policies were sufficient to produce the 1960-61 recession and they have produced the economic slack which has existed since then.

Culbertson presents ample evidence that economic policies were indeed misguided. His presentation is well constructed, and it should reach the lay audience for which the book is partially intended. Unfortunately, the author slips badly in his theoretical analysis and, therefore, does the same audience a disservice. In attempting to teach his audience that the quantity of money is *the* important economic variable, and in straining to knock down his Keynesian straw man, Culbertson slips into the same sort of empty theorizing for which he criticizes Martin and others.

According to the author, the quantity of money enjoys its place of prominence by virtue of the "cash ratio" and the "web of financial constraint." The former is the ratio of the quantity of money to GNP; a "determinant and predictable" variable in the short run and, apparently, a "constant" in the long run. Culbertson's web is best described by the following revealing quotation: "What governs total demands is a web of financial constraint. Its component parts are a determinate money supply and those customs and practices that determine people's behavior toward their cash ratio. These customs and practices, in turn, depend upon motives of caution and responsibility set against the background of a complex of ideas and planning assumptions as to the general shape of future economic developments." This is the only description of the concept which he offers.

An injection of new money into the economy fights its way through the web in the following manner: "Since the injection is independently determined and does not, in itself, cause anybody in the economy to want to hold more money idle, the presumption is that, through channels straight or devious, the money finds its way into expenditures."

This sort of theorizing leads the author to the following conclusions: (a) monetary policy is sufficiently powerful to achieve economic stability and growth; (b) we need only to look at the behavior of the quantity of money to judge the performance of monetary policy; and (c) time deposits are not a medium of exchange and, therefore, are unimportant.

Culbertson's primary domestic policy suggestion is that a tax cut is (was) not needed to get the economy out of the doldrums. What we need is a substantial increase in the quantity of money. A cut in tax rates which is not accompanied by an increase in the money supply is self-defeating; the web of financial constraint sees to this. An increase in the quantity of money, with constant federal expenditures and fixed tax rates, is not self-defeating; the predictable "money multiplier" sees to this.

Culbertson adds little or nothing to informed policy-thinking; he provides a distorted and confused analysis of policy for the lay reader. If he were content to argue that there is an obvious fallacy in believing that a tax cut not accompanied by an increase in the quantity of money is as expansionary as one associated with monetary expansion, few would disagree. To argue that the quantity of money is the only significant policy variable is to be unnecessarily dogmatic. Even the confirmed quantity theorist would admit such variables as interest rates, income, and stocks of wealth into his theoretical system. If pressed, the theorist would probably agree that, while the relationship between monetary variables and the level of GNP is potentially predictable, no one has been able to demonstrate empirically the extent of the influence. Such knowledge will never be obtained by using anachronistic concepts like the cash ratio and the web of financial constraint.

JAMES L. PIERCE

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### Public Finance; Fiscal Policy

*Public Finance in Under-Developed Countries.* By RAM N. TRIPATHY. Calcutta: World Press Private Ltd., 1964. Pp. x, 288. Rs. 15.00.

The financing of government activities in developing economies has been considered in numerous studies of particular countries by International Bank and other tax missions, by U.N. experts, by royal commissions, and by individuals. There have, however, been very few general analyses of the question, those of A. R. Prest<sup>1</sup> and R. J. Chelliah<sup>2</sup> being the best known. The Tripathy volume has the same general coverage as these, but with greater reference to the literature of economic development. The task of preparing a study of this type is a very difficult one, given the wide variety of conditions in the underdeveloped areas of the world and the lack of first-hand contact by any one person with a large portion of the spectrum. Any such volume almost of necessity suffers from the operation of the elephant theorem, and in this instance, as with Chelliah, the side of the elephant is India.

Ram Tripathy's basic philosophy is that governments must play a major role in bringing about rapid economic development, in light of the inadequacy of market forces in doing so, and that taxation and other methods of financing constitute important potential instruments for shaping economic development along desired lines. He analyzes in detail various alternatives for

<sup>1</sup> *Public Finance in Underdeveloped Countries*, London 1962.

<sup>2</sup> *Fiscal Policy in Underdeveloped Countries*, London 1960.

financing in terms of the usual principles of government financing, with particular stress on the importance of the goal of development. The study is largely analytical and normative, with limited reference to experience in the field.

The first portion of the book is devoted to a review of the theory of economic development of underdeveloped areas and of the general role of fiscal policy in economic development, with stress on the inadequacy for such areas of the Keynesian-based fiscal policy doctrines of the developed economies. The basic source of unemployment in the less developed areas is lack of capital, not inadequate demand, and fiscal policy must be shaped accordingly.

Nearly a third of the book is concerned with various tax measures, a discussion which is neither particularly novel nor illuminating. The merits and disadvantages of each type of tax are developed at length, and the reader not infrequently is left with the conclusion that there are no taxes which are suitable for the conditions. Tripathy has considerable preference for indirect taxes, particularly excises, but argues for the use of income taxation as well, largely on equity grounds. He unfortunately ignores African experience with the graduated personal tax. The importance of land taxation is stressed, a problem of greater importance in India than in many underdeveloped areas. While rejecting an expenditure tax on administrative grounds, he favors excess-profits taxation. In the field of income taxation, particularly corporate taxation, he endorses the principle of a high rate of taxation coupled with numerous incentive devices to aid investment and channel it along the lines regarded as most important for economic development.

The last three chapters deal, respectively, with government borrowing and debt management, inflation as an instrument to facilitate economic growth, and profits of public enterprise as a source of funds for development. The difficulties in the way of effective debt management are stressed. Tripathy is not sympathetic to deliberate use of inflation to aid growth, but regards profits of government enterprises as a highly desirable source of funds, largely because adverse incentive effects are avoided.

Running throughout the discussion are several assumptions of major importance for the conclusions: the existence of a high degree of inequality of income and wealth (which is not true in some underdeveloped areas, including most of Africa); the failure of the high income groups to contribute significantly to economic development; and the relatively limited dangers of adverse effects on incentives from high taxation.

Most of the analysis, hardly novel, conforms with the conclusions of what may be regarded as "orthodox" thinking in the field. There are a few exceptions. He stresses the need for reducing luxury consumption, without noting the potential dangers of such a program for incentives. The ultimate fate of savings used for purchasing land is not analyzed. His conclusion that the retail sales tax is the simplest form of sales tax to operate in underdeveloped economies would be challenged by many. His discussion of the effects of income taxation on risk-taking ignores recent contributions to the literature. His statement that developed countries have been unable to develop satisfactory

expenditure taxes ignores the fact that they have not seriously tried to do so. His discussion of how an increase in the supply of money affects consumption rather than investment (p. 204) is not clear, while that of the role of the interest rate in developing economies largely neglects its rationing function.

The volume, on the whole, makes little contribution to the subject, and such contribution as it might have made is greatly reduced by two considerations. First, the analysis is extremely loose and careless, with constant failure to clarify assumptions, and a tendency to treat assumptions as having demonstrated relevance—which frequently they do not have. The other problem is the form of presentation and method of writing. The reader cannot escape the feeling that he is wallowing along through a sea of words, constantly pulled back by the undertow of repetition, impeded by lack of systematic organization, and plagued by awkward and ungrammatical sentences. There are endless quotations and references to what other writers have said and rehash of old controversies not relevant to his primary subject matter (e.g., excess burden, marginal cost pricing). Frequently the reader can distinguish quotations from original writing only by the better grammar of the former. If the book were half the length, with the repetition eliminated and the quotations reduced, the usefulness would be greatly increased.

JOHN F. DUE

*University of Illinois*

*Tax Incentives for Industry in Less Developed Countries.* By JACK HELLER AND KENNETH M. KAUFFMAN. Cambridge: The Law School of Harvard University, 1963. Pp. xii, 288. Paper, \$4.50.

Developing nations now realize that the encouragement of private investment is a practical problem of the first importance. For the most part, however, the problem is still discussed in such superficial terms as "favorable climate" and the channeling of investment resources to "necessary" or "fundamental" activities. The need certainly remains for a more thoroughgoing comparative analysis of the variety of investment encouragement measures that might be utilized and for an assessment of the efficacy of incentive devices that have actually been used. This study attempts to fulfill part of this need with respect to tax-incentive laws, and it does so in a competent fashion that should be of particular benefit to those planning or administering a tax-incentive program. It is a worth-while addition to the series of publications emanating from the Harvard Law School's International Program in Taxation.

The discussion is most relevant for those countries in which tax incentives represent the major legislative effort yet undertaken to promote private investment—by way of partial or complete exemptions from taxes and special allowances under individual and corporation income taxes for accelerated depreciation or reinvestment. While this type of legislation is intended to induce domestic or foreign investors to initiate activities which they would not otherwise undertake or to increase their investments in already existing enterprises, it imposes at the same time an opportunity cost in foregone govern-

ment revenues, new differentials in tax-burden distribution, and additional burdens upon the tax administration. The central issue is whether these revenue, equity, and administrative costs outweigh the advantages that the use of tax incentives may afford.

The authors begin with a chapter that surveys the discretionary or selective statutory patterns and the more automatically operating nondiscretionary patterns of tax incentive laws. Not surprisingly, this chapter concludes that where an incentive statute calls for selective discrimination in the designation of individual benefit grantees and in the tailoring of benefits to their individual circumstances, a higher degree of administrative proficiency is required than is ordinarily available in less-developed countries. If tax incentives are used at all, it is therefore more appropriate to rely on a more automatically operating statutory pattern of providing for accelerated depreciation or special reinvestment allowances in the income tax law.

The next chapter confronts explicitly the problem of balancing the potential gains of incentives against their costs. It is rightly noted that for many countries the tax system is relatively unimportant, compared with other factors impeding investment, and that tax relief seems to be of only secondary importance in creating conditions conducive to industrial growth, especially when the objective is one of increasing the level of new investment instead of merely promoting reinvestment. Given the low utility of tax incentives, the analysis suggests that for most countries the costs outweigh the advantages.

Remaining chapters concentrate on the role of income tax exemption, procedures for evaluating the effect of incentives on profitability, and accelerated depreciation policies. Regarding income tax exemptions, the authors conclude that, in practice, the benefits are generally less than would be indicated by theoretical considerations alone because of the desire of governments to attain a certain kind of development and because of the nature of the administration of the laws. On the basis of an empirical survey of the results of exemption laws in Mexico, the Philippines, and Puerto Rico, it is also submitted that income tax exemption is unlikely to be a desirable method of stimulating investment: it is difficult to administer, makes the tax system inequitable, and increases the probability that the government will be unable to finance needed expenditures without inflation. To evaluate the effects of incentives on the prospective rate of return, a compound-interest method for quantifying the effects is applied to the various cases of no incentives, exemption from import duties, exemption from income tax for a limited time, and straight-line depreciation for the same time period. Working through these cases entails some tedious "turning of the crank," but it is recognized that this type of quantitative evaluation is not a substitute for judgment and general qualitative evaluation. The discussion of accelerated depreciation and investment allowances is also based on a set of theoretical calculations, primarily designed to analyze the influence on the rate of return, risk and financial resources. Special attention is given to the cost of such incentives in lost revenues. Investment allowances that offer a deduction for income tax purposes of a proportion of the cost of an asset in the year of acquisition with-

out altering the basis for ordinary depreciation are judged the better alternative to accelerated-depreciation procedures for countries wishing to purchase the greater incentive per unit of revenue loss during the first 10 or 20 years of an incentive program.

The tenor of the entire study is workmanlike, and the authors present no exaggerated claims for their theoretical insights or quantitative illustrations. They recognize that the tax factor is but one factor in the investment decision, and they give no support to any notion that investment-incentive laws can be designed as though the types, quality, and amount of investments could be directly influenced by precise, statutorily prescribed and oftentimes small variations in the amount of tax reduction granted (p. 57).

Much of the discussion, however, is based on an administrative and legal approach, and development economists will feel that more could have been made out of the economics of the subject without sacrificing—but instead strengthening—the essential noneconomic aspects. The problem of promoting private investment cannot now really be separated from the problems of development planning. Yet this relationship is not pursued with much insight; accordingly, the economic analysis remains oversimplified and only tangential to some central issues. No substantial differentiation, for instance, is made between domestic and foreign investment. Inadequate attention is given to the use of tax incentives to channel investment to fit the objectives of a development plan. Nor is there little more than a hint of the important interrelations between development planning, import-substitution, and industrial investment. Indeed the entire argument appears exceedingly narrow in comparison with a study that would have sufficient sweep to place the particular problem of tax incentives into the more general context of a wide array of alternative devices for promoting and allocating investment in a development program.

GERALD M. MEIER

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### International Economics

*A Study of Economic Growth and International Trade.* By BO SÖDERSTEN. Stockholm Economic Studies, New Series, V. Stockholm: Almqvist & Wiksell, 1964. Pp. 190. SKr. 38; paper, SKr. 28.

For better or for worse, what is usually referred to as the Heckscher-Ohlin approach now dominates the theory of international trade and investment. The modern graduate student is familiar with the standard two-by-two-by-two model (two countries, two factors, two products) and thoroughly drilled in the comparative statical manipulation of it. If he cannot tell you offhand just what will be the effect on wages at home of an increase in population abroad, at least he knows how to find the answer—the Heckscher-Ohlin answer.

But of course it takes time to calculate: it would be easier to consult a card index. Bo Södersten has provided us with such an index. For a clearly defined class of comparative statical exercises he has provided us with a well-

nigh complete compendium of answers. Given the disturbing parameter, say, population abroad, and the variable of interest, say, the home wage rate, one has only to turn a few pages to discover the required total derivative.

The parameters in which Södersten is interested all have something to do with growth; they include the labor force at home and abroad, the stock of capital at home and abroad, and an index of technological efficiency, one for each industry, at home and abroad. The variables, the responses of which form the subject of Södersten's calculations, include the terms of trade, the level of output of each commodity in each of the two countries, the real national product, real income, and the distribution of income.

As a bonus Södersten has provided a brief penultimate chapter in which he abandons neoclassical production functions and reworks some of his problems under the assumption that coefficients of production are fixed, but demand is such as to assure the full employment of both factors. There is also a final retrospective chapter in which Södersten sketches the history of some of his problems and which is notable for a crushing dissection of Dr. Raoul Prebisch's theories.

The job was worth doing and Södersten has done it well. His style is as graceful as the subject matter permits, and his notation is happily chosen. Only on two rather minor matters would I wish to quibble. Thus Södersten makes a good deal of the generality of his treatment, in particular of his refusal to follow tradition in assuming constant returns to scale. But he does assume a competitive equilibrium, with the price ratio equal to the slope of the production possibility curve. How this equality may be reconciled with variable returns to scale, however, is not made clear. It can be done, by resort to externalities, for example. But the externalities must be of a very special kind indeed, and one wonders if Södersten has realized how limited is his additional generality. My second quibble concerns an unguarded passage on page 83. Södersten is there discussing the impact on factor rewards of a change in the factor endowment, with production functions which are unspecified but which might be homogeneous. He appears to deny that, if the terms of trade are constant, if production is incompletely specialized, and if production functions are homogeneous, factor rewards will be insensitive to the change in endowment.

MURRAY C. KEMP

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*The Dollar Crisis—The United States Balance of Payments and Dollar Stability.* By NASROLLAH S. FATEMI, THIBAUT DE SAINT PHALLE, AND GRACE M. KEEFE. New York: Fairleigh Dickinson University Press, 1963. Pp. xvii, 317. \$8.95.

This book, the first in a projected series from the Institute of Foreign Economic Studies of Fairleigh Dickinson University, is a disturbing example of a tract that fails to give plausible support to its assertions, while at the same time claiming the standing of a research study. The trouble is not the conclu-

sions, nor the strength of their expression, but the quality of their substantiation; in fact, the analysis is superficial.

Some of this ambivalence is evident from the Foreword, which speaks with the multiple voice of the Committee on Economic Research. "It is the thesis of this study that the present situation [of the U.S. balance of payments] has been brought about by a number of complex and interrelating factors . . . and that there is no single cause at the root of the problem" (p. ii). Yet (p. iii), "the problem, we believe, is being rendered chronic by the policies and spending programs of the United States Government in relation to economic and military aid as well as the maintenance of an excessively large military establishment abroad." The reader will not be expected to take very seriously the concluding statement that "it is not the purpose of this study to defend any particular thesis or to cast any blame for the unfortunate condition which exists" (p. iv). The didactic intent of the book is reinforced by the Introduction, of which a series of policy prescriptions accounts for two-thirds of the space.

The method employed by the authors is to take the categorization of the main tables of the U.S. balance of payments and to devote separate chapters to each of the principal components. The text is supplemented by no less than 103 tables, many of which are derived from the Department of Commerce statistics. Partly as a result of this, the problem is seen as almost exclusively a U.S. problem rather than a matter of international economic relations. Since the reference is mainly to the period 1950-62, the problem is the cumulative effect of the over-all deficits in U.S. transactions. Thus the policy prescriptions are for what we should do about it, or rather what the U.S. government should do about its own affairs, since "the . . . deficits since 1957 have not resulted from any serious imbalance within the private sector" (p. 293).

Not all of the deficiencies of the analysis are attributable to the complexity of the subject. There is only the slightest attempt, for example, to estimate what part of U.S. merchandise exports (nonmilitary) is associated with U.S. government grants or loans. With the exception of brief references to the Commodity Credit Corporation and the Export-Import Bank, these exports are regarded as credits to the private sector account. There is, moreover, a completely arbitrary exclusion from the category of service imports of U.S. military expenditures abroad. The subsuming of these expenditures under "unilateral transfers" is not even argued; the authors blandly discuss (p. 241-43) how much of them were absorbed by offshore procurement, troop outlays, contractual services, and so forth. It is difficult to avoid the inference that the intent was to magnify the aggregate of U.S. Government Transfers Abroad (military expenditures, economic aid grants and net government loans) in Table 12.

On the other hand, the complexity of the subject appears to be admissible only when the discussion centers on private transactions. "The effects of direct investment on this country's balance of payments do not lend themselves to facile or definitive measurement. . . . The variables and inter-relationships

involved in this process are numerous and complex" (p. 166). The apologetic attitude reappears in the discussion (p. 175-76) of possible conflict between profit-seeking and the best interest of the United States; the displacement of domestic output by imports from foreign subsidiaries is styled "the path of least resistance."

Since the subject is complex, there is reason to be critical of some of the statistical juxtaposition used by the authors. In Table 91, for example, there are compilations, for each of six countries, of merchandise trade with the United States, military expenditures by the United States, holdings of dollar assets by their nationals, and official gold holdings. There is no mention here that these countries have in the past few years been making sizable net repayments on U.S. government loans; the emphasis is all on "the accumulation of large dollar surpluses which have been used quite freely to build up gold reserves" (p. 247). Moreover, it is not apposite to quote the total of military grant aid received by certain countries in 1950-61 and follow this with the prescription, "In view of the serious and continuing deterioration in this country's balance-of-payments position and the rising budgetary deficits, it would seem that one of the most obvious economies the Administration could make would be to discontinue forthwith military assistance grants to the dollar-surplus countries of Western Europe and Japan" (p. 261).

Nowhere, however, does the nationalist and bilateralist attitude of the authors appear more plainly than in the fact that the reader is within 25 pages of the end of the text before there is a mention that the dollar is an international reserve currency. By that time, it is too late for a serious treatment of international reserve adequacy. The authors, in any event, are more concerned about gold movements.

The basic question raised by this book is not whether there is a problem in our international economic balance, nor how to deal with it. It is whether there is any excuse for an academic research institute's issuing a political pamphlet superimposed on compilations of official statistics.

JOHN B. HENDERSON

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*Domestic Interests and International Obligations—Safeguards in International Trade Organizations.* By IRVING B. KRAVIS, Philadelphia: University of Philadelphia Press, 1963. Pp. 448. \$8.50.

Professor Kravis opens this timely and useful book by observing that cooperation among countries to ensure the orderly marketing of traded products creates conflicts between obligations under international commitments and interest groups. The major remedy is to include safeguards against injury, balance-of-payments difficulties, failure to receive anticipated benefits, or inconsistencies with other government programs in the treaties and agreements setting up international trading organizations. Such safeguards usually provide for renegotiations of the agreements, derogations from their provisions, exceptions, and adjustment assistance.

Kravis analyzes the safeguarding provisions of the General Agreement on

Tariffs and Trade, the Organization for European Economic Cooperation, the European Coal and Steel Community, and the European Economic Community and reviews their use in each organization. The main part of his book describes the background, structure, administration, safeguard experience, and operation of each organization and evaluates its general effectiveness.

Kravis is interested only in the question of cooperation among member countries and in showing how these four organizations met specific problems through the use of safeguards, not in whether the organizations themselves best met the needs of closer international economic cooperation. There is almost no discussion of the effectiveness of the organizations in expanding or rationalizing trade or of their possible negative effects in diverting it. Kravis thus seems to take the meaning of the term "international obligations" too much for granted and so fails to make clear its necessary relationships to domestic interests.

The second part of the book is a compendium of safeguard cases; one should consult it if he wishes to know how waivers were used and by whom under the GATT, how the ECSC handled the Belgian coal crisis or the marketing of steel scrap, etc. As a compendium this section is both comprehensive and authoritative; Kravis has provided a valuable service in putting this large amount of material into readily accessible form. Its organization, however, tends to obscure the role which the trade agreements played in the formulation and implementation of national commercial policies. Did the OEEC cause France to liberalize her trade faster than otherwise? Did the GATT restrain protectionist pressures in the United States and accelerate its tariff reductions? Kravis touches on these points, but the lack of extended discussion of the trade policy of any country in the context of its total international obligations leaves the reader unable to form an opinion. Such questions nonetheless bear heavily on the issue of national interests and international commitments. It is possible to argue, for instance, that France in the 1950's used the organizations to which she belonged to increase protectionism and to postpone necessary and beneficial currency and financial reforms that she might have been forced to adopt if left on her own. If the permissiveness of the trade organizations fosters such situations, it must be put on the debit side in evaluating their effectiveness.

The final section of Kravis' book seeks to draw lessons for a bolder U.S. trade policy from the successful safeguard experience of Europe, particularly that of the ECSC and the EEC. Kravis ignores, however, the most serious problems in applying European experience to the United States. The integration of the Six is basically a far easier task than that of the numerous and diverse countries forming the GATT. Kravis notes that the EEC and the ECSC were predicated on the assumption that integration would cause little industrial dislocation. The similarity of labor and capital costs and the free mobility of labor among the member countries guaranteed this. The Community has made little progress in agricultural unification, on the other hand, because of the cost differences that exist in that sector. The immense differences between the relative labor and capital costs of the United States and the poorest industrial countries mean that nondiscriminatory and substantial tariff cuts

must inevitably cause the displacement of labor-intensive U.S. industries unless derogations are invoked. This is the fundamental dilemma of U.S. trade policy and it cannot be assumed away by asking, as Kravis does, for a firm commitment to a liberal trade policy at the outset. In this respect, moreover, Europe has made as little progress as the United States and has instead sought trade arrangements having the greatest political and the least economic meaning.

In treating reconciliation policies in a systematic way, Kravis is one of the few economists to recognize what is the most important issue in trade policy. His book does not show, however, that safeguards are the best form of reconciliation or that experience is necessarily the best guide. More theoretical attention must be devoted to the problem of what is to be reconciled and how best to do it. The problem is to devise liberalization formulae and bargaining procedures that will automatically offset domestic interests against one another and that will also minimize the displacement costs necessary to achieve a given degree of effective trade liberalization. This would indeed seem to be the lesson of Kravis' book which reveals that "In each of the trading arrangements considered . . . the conflict between domestic interests and international obligations has been resolved in virtually every important case in favor of the former."

WILLIAM PENFIELD TRAVIS

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*Britain and the Postwar European Payments System.* By GRAHAM L. REES. Cardiff: University of Wales Press, 1963. Pp. 291. 42s.

This book, the author tells us, began as a study of the European Payments Union (EPU). As such, it will fill an important gap in our bookshelf on postwar financial arrangements. It provides a thorough summary of EPU operations and a judicious appraisal of the Union's contribution to the liberalization of European trade and payments. As a study of British policy, however, it falls far short of the promise conveyed by its title. There is, indeed, much less discussion of British problems and British attitudes than we require for a complete understanding of the EPU itself and of the long trek to convertibility. When Graham Rees does discuss the British position, moreover, he does so without questioning its origins, premises, or implications. His silence seems to convey a sweeping endorsement of postwar British policy, even when that policy obstructed rapid progress toward the objects that Rees has endorsed. Rees dwells at length on the proposals for an early "dash to convertibility" and on the British Government's brief flirtation with flexible exchange rates, but he gives us little insight into these episodes—into the internal debates on the proposals or the expectations of the chief protagonists, into the implications of the British position for the evolution of the EPU, or into the reasons for the ultimate retreat from those proposals. Rees has little more to say concerning the attempt to recapture sterling's ancient role in world finance and the consequence of this commitment for British policy toward Europe. He does not even mention the prolonged debates in EPU concerning the scope of ex-

change-rate guarantees—a debate that much exacerbated the negotiations and has continued relevance for sterling and the dollar.

These omissions would not be too serious if Rees had referred the reader to the several other works that study British policy from the early postwar years until convertibility. Yet his book makes bare reference to Triffin's work and none at all to Bell or Polk or to this reviewer's own account. One wonders, indeed, whether Rees made any use of these other efforts or retraced this complex history all by himself.

But Rees's work on EPU has not been done before, and specialists will have frequent occasion to consult his account—especially the middle chapters. These contain a careful history of the EPU and of the slow advance toward convertibility. They also fix the technical detail of the period into a broader analytical framework. Rees strives always to remind us that credits and discrimination may distort the allocation of real resources and that the peculiar institutions developed in the postwar years represented a response to a larger payments problem. Rees has prefaced these key chapters with 40 pages of theory and closed his account with 40 more on recent financial developments and on the spate of new proposals for international monetary reform. These first and final sections are somewhat less interesting; they offer nothing new to the specialist, yet may rush past the neophyte. But they may help to set all the history in perspective.

Rees's book is free of major error, and his conclusions stand up to the facts. At times, however, he is elliptical or elides intriguing points. One wonders, for example, about the "various influential opinions held in the United States" that barred the early establishment of an effective clearing system (p. 72). One also wonders which variety of sterling could be bought for \$2.40 in 1950 (p. 106). The author errs, moreover, when he asserts that Britain would not normally permit transferable sterling to fall below \$2.78 after 1955 (p. 182); transferable sterling was well below that lower limit during 1955 and in most of 1956-57. Finally, Rees may also err when he describes the 1955 decision to support the transferable rate as though it was a conscious step toward convertibility (p. 160); others have argued that Britain backed into this important decision while seeking to curb commodity shunting.

Appraising the contribution of EPU to the liberalization of European trade and payments, Rees concludes that the Union made "a notable contribution to European recovery, for it provided a return to global multilateralism, improving greatly on the bilateral arrangements which it superseded" (p. 255). It is hard to quarrel with this appraisal, especially in retrospect. But Rees goes on to argue that EPU should have been terminated earlier, in 1954, because, he says, access to EPU credits gave European governments a continued incentive to discriminate against dollar goods. The striking liberalization of dollar imports after 1954 challenges this view (see Rees's own Table 32). One wonders, indeed, if the prolongation of EPU did not actually serve to mitigate dollar discrimination. A premature "hardening" of European settlements might well have put a brake on liberalization. Although EPU credit could not be used in lieu of reserves to pay for dollar imports, the need to use

reserves in lieu of that credit might have weakened Europe's will to remove discrimination.

Unfortunately, Rees's book lacks the felicity of style we have come to expect from our British *confères*. His sentences are very long, and the punctuation is confusing. His detailed account of EPU operations and of the transition to convertibility becomes very tedious because he insists on translating his tables into dreary prose. Rees's book would have gained from more patient editing, especially from care with the spelling of well-known names. But the steady diet of official publications on which he must have fed for many long months is hardly nourishing to crisp and lively prose.

PETER B. KENEN

*Columbia University*

### **Business Finance; Investment and Security Markets; Insurance**

*The Future of Private Pensions.* By MERTON C. BERNSTEIN. New York: The Free Press of Glencoe, 1964. Pp. xiii, 385. \$12.50.

This is an exhaustive, and at times exhausting, treatise on the role of private pensions in providing retirement income supplementary to the benefits from the federal Old-Age, Survivors, and Disability Insurance System. Most of the author's attention is devoted to the shortcomings of present voluntary arrangements. Unless these are remedied, he warns, the pressure will mount for greater reliance on compulsory, tax-supported governmental programs. Furthermore, improvements are urgent because the arrangements in effect today are determining the adequacy of retirement incomes 20 or 30 years hence.

Despite the progress of recent decades, Bernstein reminds us that levels of retirement income for the great majority of older people represent a sharp drop from living standards enjoyed during their working years. He sees the closing of a major portion of this gap between employment income and OASDI benefits for both the worker and his widow as the long-range objective for private pension programs. For this result to be achieved on a mass basis, he argues convincingly that pension equities must be preserved from present exposures to forfeiture and loss.

Employee turnover resulting from a combination of the mobility of labor and technological change prevents the accumulation of pension credits under the delayed vesting provisions contained in most private plans. Despite the marked trend toward earlier vesting and the growth of multi-employer plans in recent years, the author argues that too many employees never receive pension credits for the early portion of their working careers during which job changes are most frequent. These pension credits cost little to fund at young ages because of the long period of interest earnings. If they could be handled economically through a pension credit clearing house, a retired worker could receive a single pension earned under three or four or even more private plans.

This concept of a central clearing house is developed at length in Chapter 10. Bernstein recognizes the difficulty of translating credits under different private plans into monetary values for purposes of transferring equities

from one plan to another. He concedes that these transfer values cannot be precisely calculated on a mass basis, but argues that minor inequities are not important in relation to the goal of at least limited transferability. This machinery to preserve pension equities is advocated as the most practical method of assuring a higher level of ultimate benefits for large numbers of the working population, especially those in the lower-income groups. The alternative of liberalizing benefit formulas without earlier vesting, in contrast, would only further concentrate the distribution of benefits to long-service employees and executives in the upper-income groups.

The organizational structure of this clearing house presents a multitude of problems, especially if it is to take over the custody of funds for individuals who leave covered employment to work either for themselves or for employers who have no pension plan. Determination of participation in and control over the operations of this institution presents difficulties which would daunt a proponent less dedicated than the author. When all of the complexities have been examined, the reader may well conclude that the vesting of pension credits in each individual plan is a simpler, more economical procedure with the aid of modern electronic data processing equipment.

Applying his legal training and experience, the author finds gaping holes in the protection afforded employees whose companies go out of business, merge, close plants, or otherwise liquidate portions of their total activities. Bernstein is at his best in arguing the case for employees who are separated in large numbers as a consequence of these kinds of corporate upheavals. He is less original and persuasive in his advocacy of flexible retirement arrangements.

The broad range of coverage does not include extensive economic analysis. For example, Bernstein treats pension costs simply as a part of wages and salaries, instead of as a portion of total outlays for placing a trained individual in a position to produce efficiently. The forfeiting of pension credits by short-term employees can be viewed as an offset to the costs of turnover and training replacements. Some observers attribute the current large volume of overtime worked in a period of uncomfortably high unemployment to the already substantial cost of fringe benefits for full-time employees.

In a broader sense, Bernstein's analysis is faulty at points because he assumes that adequate retirement income is a prime objective of most individuals. This leads him to the doubtful conclusion that a contributory feature for private plans, with limited tax deductibility for employee contributions, would greatly stimulate retirement saving among middle-income groups. The operation of state and local government retirement systems, which, incidentally, are guilty of most of the faults that Bernstein finds in private plans, suggest no such stimulus from the contributory feature. The facts are simply that raising families, education, home ownership, leisure-time activities, and the desire for a comfortable level of living in the present compete very effectively with retirement-income goals.

Bernstein concludes that "the future of private pensions will be whatever we make it" (p. 299). Perhaps he has interpreted this "we" too narrowly as being limited to lawyers, actuaries, economists, and public officials when in

reality the "we" is our vast industrial society, with white and blue collars greatly outnumbering academic attire or regalia of public office.

Taken as a whole, however, Bernstein's extensive efforts have produced a valuable critique of the emerging private pension structure. Lack of available data prevented him from providing a convincing answer to the key question: what proportion of employees covered at one or more times under private pension plans will ultimately receive pension benefits? His proposals would increase this proportion, but the extent of the improvement is not clearly established. Yet as a reference work or as a compendium of controversial proposals for improvements, this book deserves the careful study of all concerned with the role of private pensions in the American economy during coming decades.

ROGER F. MURRAY

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### **Business Organization; Managerial Economics; Marketing; Accounting**

*Unternehmensführung, Organisation und Entscheidungen.* By ERICH GUTENBERG. Wiesbaden: Betriebswirtschaftlicher Verlag Dr. Th. Gabler, 1962. Pp. 176. DM 18.

In the preface to this book the author states his disbelief in the possibility of a scientific discipline of top-managerial decision-making and his conviction that the art of formulating correct managerial policies can ultimately neither be taught nor be learned. There exist, however, a large number of managerial problems which are amenable to rigorous treatment. From among these the author has selected four major groups which he discusses in four loosely connected chapters: (1) the structure of top management (*Führungsgruppe*); (2) the nature of top-managerial decisions (*Führungsentscheidungen*); (3) the formal relationships between top management and the middle and lower managerial echelons; and (4) the nature of decisions on the middle and lower echelons. The book is neither a systematic text nor an original contribution but is rather a survey of important parts of contemporary organization and decision theories as developed chiefly in the United States. For in spite of the old *Betriebswirtschaftslehre* tradition, concern with scientific management in the U.S. sense of the term is of relatively recent origin in Germany.

To the non-German reader the most interesting chapter is the first one, in which the author gives an excellent comparative analysis of the locus and of the formal structure of top management in the German and in the Anglo-American corporate enterprise. The discussion of the difference in the roles assigned to the *Vorstand* and to the board of directors is particularly enlightening. The author also makes an interesting distinction between the German concept of *Betriebsführung*—which I have translated by *top management*—and the broad concept of *management* as used in English (p. 20). According to him there exists no German term corresponding to *management*, which

seems to indicate a difference not only in linguistic habits but in facts and attitudes.

The proper areas for top-managerial decisions are said to be: formulation of long-run policy; coordination of the major constituent parts of the enterprise; correction of maladjustments; nonroutine measures of particular importance to the whole enterprise; and appointments to top-managerial positions (*Führungsstellen*). It is typical of top-management decisions that they must be taken under conditions of uncertainty while those delegated to the middle and lower levels of management are taken under conditions of calculable risk or of certainty. In ten pages (pp. 80-90) the author gives an interesting geometrical treatment of decision-making under uncertainty. These decisions are said to be based on original probability indexes assigned to the expected results of available alternative courses of action ranging from, say, 0 to 10. Top management apparently has "certain ideas about the probability" (p. 80) of the future occurrence of any of the conceivable combinations of consumer and competitor behavior and of general trends. There is, however, no explanation of the process by which top management arrives at these ordinal probabilities. The rest of this chapter is devoted to a rapid survey of some alternative approaches to decision-making under uncertainty by W. Krelle, H. Koch, L. Hurwicz, L. J. Savage, and H. Simon and J. March.

The peculiar drawbacks of surveys become most apparent in the two last chapters, which deal with rather technical matters: internal structure, delegation of power, information and communication patterns, informal groups, decision-making by middle management under conditions of calculable risk and certainty. To the reader already familiar with procedures such as mathematical programming, game theory, etc., these sections have not much to offer. The reader who turns to them for technical information is likely to find them frustrating. For example, on pages 109-14 the author presents a simple case of assigning workers to tasks. Although mathematical expressions and tableaux are used, there is no explanation of the procedure by which the solution is obtained nor why it is superior to the one obtained by inspection. (Cf. also pp. 160-62.)

In this reviewer's opinion the weakest part of the book is a brief section on the informal groups and cliques which form spontaneously within the hierarchical and functional structure of the enterprise (pp. 147-52). These groups may have considerable influence on the morale and performance of the enterprise. The argument of this section is particularly superficial. No reference is made to the extensive empirical work being done in the areas of group dynamics and small-group research.

The faults of the book are, however, those of the class to which it belongs. On its own terms it is certainly a very good book which has a great deal to offer to the reader who requires only general information on, rather than a specialist's knowledge of, some major areas of current organization theory.

EMILE GRUNBERG

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### **Industrial Organization, Government and Business; Industry Studies**

*Theories of Business Behavior.* By JOSEPH W. MCGUIRE. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1964. Pp. xix, 268. \$7.95.

This book presents a wide variety of theories about business behavior with critical comments drawn from the literature and some of the author's own comments. After two introductory chapters, one concerned with the general methodological problems of constructing theories and the next concerned with the problem of the concept of the firm, the author starts presenting the various theories in the third chapter.

There are three chapters dealing with the "economic" approach to the firm. The first presents traditional economic theory, the second presents criticisms of that theory, and the third presents alternative theories put forward by economists. The emphasis is on the objectives which the firm seeks and on the decision-making processes.

These are followed by two chapters concerned with more mathematical approaches to decision-making: linear programming, various criteria which have been suggested for decision-making under uncertainty, and the theory of games.

The next three chapters deal with approaches developed in the social sciences but outside economics. They present material which is not usually found in economics texts, although it is becoming more familiar to students of business administration. The first of these deals with theories drawn from sociology, the second from psychology, and the third from cultural anthropology.

The final chapter discusses some of the problems and ideas common to most of the approaches previously presented, and ends on the optimistic note that we are developing "better and more complete theories of business behavior."

This is too much ground to cover in depth in one short book, and the author tries only to give a brief survey so that the student can become acquainted with the principal ideas and problems which are now being discussed in the literature. The book can therefore be appraised on the basis of the selection of topics covered, the clarity of presentation, and the quality of the analysis and appraisal of the theories presented.

The coverage of the book is very good, given the initial decision to deal briefly with a large number of theories and authors. The footnotes are extensive and would serve as an excellent bibliography for the student. This reviewer would have preferred to see less attention paid to the technical details of some of the mathematical analysis (Chapters 6 and 7) so that some of the theories could have been analyzed more fully, but complete agreement about the appropriate weight to associate with such a large number of topics can hardly be expected.

The presentation is no more pedestrian or obtuse than one expects in economics texts. There are, however, some unfortunate errors which could create serious difficulties for the student. The worst examples are on page 147 where

the author says incorrectly that the appropriate combination of  $B_1$  and  $B_2$  can be found by solving a specified equation, and footnote 39, page 70, which says that the marginal product of each productive factor must be zero if the production function requires that the factor inputs be combined efficiently to obtain the maximum output. Other examples can be found in the equation on the bottom of page 59 (and the associated footnote 42, page 71) where the subscripts are incorrect, on pages 122-23 where the previously suggested outcome of \$102 is ignored, and on page 130 where there is confusion as to whether  $Y$  is an outcome or is the surprise which would be associated with an outcome,  $X$ .

The quality of analysis and appraisal of the theories are occasionally weak. On the one hand, the author too frequently fails to give any evaluation, as on page 20 where a paragraph of criticisms of Lewin's framework includes no indication from the author of their importance or relevance, and on page 37 where the reader is told to judge the usefulness of the various concepts for himself.

On the other hand, the author is not always consistent in his appraisals. In particular, he summarizes Friedman's familiar position on methodology (in part, page 9) as follows: "the realism of the assumptions of a theory does not matter so long as the theory permits accurate predictions of behavior." He concludes his discussion (page 11) by saying "that Friedman's position is untenable . . ." and recommending ". . . that our theoretical foundations be as realistic (in the sense of truth vs. falsity), as relevant, and as informative as we can make them."

As early as page 17, in discussing the problem of the concept of the firm, the author starts moving away from his recommendation and towards Friedman's position: "Criteria for judging the merits of such concepts as the firm do not necessarily include their relation to reality." In the last chapter, he joins Friedman's camp unconditionally (page 246): "if the choice must be made [between models whose assumptions are realistic and therefore intractable and those which have few enough assumptions to be tractable but are therefore unrealistic], it had better be made on the pragmatic basis of predictability. . . ."

DIRAN BODENHORN

*Ohio State University and  
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*The Weapons Acquisition Process: Economic Incentives.* By FREDERIC M. SCHERER. Boston: Division of Research, Graduate School of Business Administration, Harvard University, 1964. Pp. xxi, 447. \$7.50.

This volume explores an important topic—the way institutional arrangements affect incentives when government arranges for the development and production of outputs. In this case the outputs are weapons, and the institutional arrangements are mainly the relationships between the Department of Defense and aerospace contractors. But indirectly much of the discussion is relevant to government purchase of other goods and services and to govern-

ment planning in general. The study brings out the difficulties, in such situations, of harnessing individual interests to promote broader interests and examines some of the alternative techniques for trying to do this.

In my view, exploration of this subject is much needed. For a long time, economists have studied the way the institutional framework affects choices and resource allocation in the private sector. They should now give more attention, using the same assumption that individuals are utility-maximizers, to the way the institutional framework shapes choices in the public sector. This book by Frederic Scherer makes some new progress along these lines. The analysis is generally sound, the observations are perceptive, the ideas provocative. For these various reasons, the volume should be of considerable interest, not only to those who are concerned about defense but also to persons who are interested in government activities, the problems of planning, and indeed political economy in general.

The book is divided into three parts. Part I deals with "competitive incentives"—that is, those generated by the competition among firms to maintain or increase future (profitable) sales to the government. Firms are sometimes rivals in developing missiles or aircraft that are close substitutes, and they are always competing for shares of the defense budget. Part II pertains to "contractual incentives," meaning those set up by the possibilities of increasing profits on particular contracts. These are the special forces, for example, that are introduced by the provisions of cost-plus-fixed-fee, fixed-price, or so-called "incentive" contracts. Part III, called "Incentives and Government Policy," examines a number of alternative policies. The work, it might be noted, is based to a considerable extent on case studies of development programs prepared in the Harvard University Weapons Acquisition Research Project and is closely related to the earlier work by Peck and Scherer.<sup>1</sup>

Among the major points stressed in the volume are the following. Contractors feel very keenly "user costs" (i.e., the possible sacrifice of future utility) and also the other side of the coin—the gains offered by actions that increase the chances of future sales, growth, and profits. In conjunction with the uncertainties and the nature of the bargaining process, this is said to produce much emphasis on improved weapon performance and early delivery and little emphasis on cost reduction. As a consequence, Scherer suggests that incentive contracts are not likely to have much impact on contractor behavior. Moreover he believes that the multidimensional incentive contracts (premiums for achievement along particular dimensions) may have harmful effects, by reducing the flexibility of contractors, making engineering changes more difficult, and obscuring the true uncertainties about Defense Department preferences.

Many other interesting points are discussed, far too many to summarize here. Along the way, the author mentions the similarity of the difficulties to those faced by the USSR in dealing with Soviet producers. Further exploration of this might have been enlightening. It is interesting to note, for exam-

<sup>1</sup> Merton J. Peck and Frederic M. Scherer, *The Weapons Acquisition Process: An Economic Analysis*, Division of Research, Harvard Business School, Boston, 1962.

ple, that, while the bargaining process here apparently leads to emphasis on engineering performance and delivery dates, the Soviet system, at least in many nondefense sectors, often leads to the sacrifice of quality and timeliness.

Although Scherer is usually careful to call something an improvement only if it brings more gain than cost, he slips, it seems to me, in talking as though less emphasis on quality and timeliness and more on cost reduction would *clearly* be an improvement. All of us would agree, of course, that costs should be cut if it can be done without impairing quality, etc., but we rarely know in advance of development what the quality-time-cost tradeoffs can or ought to be.

On a number of judgments I think Scherer is wrong, but even if he is, it does not detract significantly from the usefulness of the book. In my opinion he is a little inconsistent in stressing (Ch. 4) that contractors are seldom judged on the basis of past performance, yet (in various chapters) that they feel powerful pressures to increase weapon performance and reduce development time in order to get additional contracts. My judgment differs from his also regarding the advisability of awarding future contracts on the basis of after-the-fact evaluations of contractor performance by a single Program Evaluation Board. The dominant feature of the whole situation is uncertainty, and I believe a better hedge against uncertainties is achieved if contractors can sell in a variety of ways to a variety of clients than if great authority is given to a single evaluation board. The alternative policy changes, described in Chapter 13, appear more promising to me. But one's choices here depend on judgments about considerations that we know little about at present. Perhaps this stimulating study will lead to more tests of hypotheses and further increases in our knowledge of these matters.

ROLAND N. MCKEAN

*University of California, Los Angeles*

*The Managed Economy.* By MICHAEL D. REAGAN. New York: Oxford University Press, 1963. Pp. ix, 288. \$6.00.

Joining those commentators concerned with the growth of private, corporate power, Professor Reagan issues the call for a form of economic introspection similar to the TNEC investigation of the 'thirties. He is distressed that our economy no longer possesses the mechanism that automatically converts the gain of the firm into the welfare of the community. Indeed the economy has changed "from a system of competition to one of administered resources and private planning, and as a consequence, the market itself has lost the regulatory power it formerly had." Not only has the market mechanism died, but no equivalent control has yet been introduced in its place. The role of the stockholder, for example, has long since been reduced to passive ownership. The consumer can hardly count since he is unorganized. And antitrust action, while applicable to the single-firm monopoly, is losing its relevance under oligopoly. Though the decisions of a few corporations, be they price, wage, or investment decisions, affect substantial portions of the economy,

these decisions are neither controlled by nor accountable to the community at large. Hence, we live in a "managed economy" in which unmolested, discretionary power resides within the private sector.

The author's policy prescription for this state of affairs falls under three proposals. A first reform is to submit large corporations to greater degrees of public control. In this regard Reagan advocates the federal incorporation of the top 200 firms—a suggestion attributed to the late Senator O'Mahoney. The author also proposes that a per se rule be applied to firm size within a relevant market as well as limits placed upon conglomerate mergers. Moreover, officials representing the public interest should be appointed to the board of directors of large corporations—in the manner of the recently formed Communications Satellite Corporation.

A second reform applies to the noneconomic activities of the large firm. Specifically, the author asserts the need to "repeal the privileged tax position now enjoyed by corporations for lobbying and for cultural, educational and charitable contributions, and new legislation preventing managers from spending corporate funds, with or without tax deductions, on political activities."

Finally, Reagan argues that national planning be grafted to business planning within the economy. Price and wage decisions would be accountable to public review; government would participate in the large firms' investment decisions; and manpower and industrial location decisions would be "institutionalized." All in all, the author advocates national economic planning as a device for controlling the exercise of private economic power.

The policy recommendations of the author appear to rest upon his pessimism regarding the vitality of competition as an allocating device. But there may be life in the market mechanism yet. After all, she is not very old, and she continues to display youthful recuperative power. Moreover, many of Reagan's proposals endow some two hundred large corporations with the status of public utilities. That this route is less reassuring is evidenced by the experience in the field of the independent regulatory commission. Nevertheless, this study's recognition of corporate power and its search for resolving that power within the framework of a democratic society is both relevant and provocative.

MANLEY R. IRWIN

*University of New Hampshire*

*Marginal Cost Pricing in Practice.* Edited by J. R. NELSON. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1964. Pp. 266. \$7.50.

Suppose that somebody wants to advocate the use of economic principles and theory for the practical problems of pricing. Where do you think he would look for examples? In a competitive sector of a liberal economy or in a monopoly of a planned economy? Presumably in the first one. Well, Professor J. R. Nelson does the opposite; he deals with the problem of marginal cost pricing in practice by choosing the case of the electricity supply in France. Everybody knows that Electricité de France (EDF) is practically a monopoly, that EDF is nationalized, and that the French economy is more and

more planned (although it is a "planification indicative et non impérative"). However paradoxical this choice may appear, I think it is an excellent one for the following reasons.

EDF is well known in France for being very much concerned with the problem of "fair" and efficient pricing as a national service. Some of the very best French economists are or have been working in the economics department of EDF: Mr. P. Masse, now the head of the French Plan; Mr. M. Boiteux, well known for his works on depreciation and on marginal cost pricing; and Mr. G. Morlat, an expert in production decisions under uncertainty.

Nationalization made EDF one of the three largest French enterprises. This created a lot of problems for efficient transportation and operating policies and led to research for establishing the "right tariff" by marginal cost pricing.

EDF accounts for 1 per cent of the GNP, but for 5.5 per cent of national investment; therefore, great efforts went to defining criteria for investment decisions. Such problems are particularly difficult in this case because (a) huge amounts are involved; (b) uncertainty appears both in demand (the peak demand is hardly predictable) and in supply (half of the production comes from hydroelectrical plants and the water supply is a random variable); and (c) the rate of interest is extremely important since the life span is usually long and since the permanent inflation phenomena have to be taken into account.

This book reproduces a group of articles which summarize quite well the results which French economists found in tackling these problems.

1. *Introduction*. Chapter 1 (Boiteux) briefly describes production and tariff aspects of the economic conditions under which EDF operates.

2. *Price Policy*. Chapter 2 is very general. G. Dessus wrote this pioneering article on pricing problems in 1949 as a report for a research meeting in Brussels. Chapter 3 (Boiteux) gives in four theorems a very brief and lucid analysis of the foundations of marginal cost pricing. Chapter 4 (Boiteux) applies the previous theorems to determine short-term and long-term pricing in several important cases: plants working close to capacity, first under constant demand and then under periodic peak loads. Chapter 5 (Boiteux) deduces from the previous results the marginal cost in every point of an interconnected electricity network, then the cost of distribution at low voltages, and finally the adequate tariff structures. Chapter 6 (Boiteux) gives the final result—the French *tarif vert* (green tariff) for electricity. Chapter 7 (Masse) tries to evaluate the efficiency of the *tarif vert*.

3. *Operating Policy*. Chapter 8 (Morlat) provides a decision rule for operating reservoirs of hydroelectric plants under water-supply uncertainty.

4. *Investment Policy*. Chapter 9 (Masse) deals with the problem of minimum cost investments under equal probabilities of failure. The importance of time and risk factors is stressed. Chapter 10 (Boiteux) goes into more details and refines the concept of "hydro-thermal equivalence." Chapter 11 (Masse and Gibrat) shows how linear programming can be applied to determine the optimum investment schedule under constraints. Chapter 12 (Masse

and F. Bessière) extends the analysis to prospective views, and a sequence of investment plans is studied for long-term programming. Here again, linear programming (with its dual form) is used.

To conclude, this book will give to the reader a very lucid view of the French results on marginal cost pricing and of the present trends of this line of economic thought. One might regret that very few comments and no transition are given by Nelson. The book is therefore a simple group of articles, and some repetitions are difficult to avoid. The reader finds, for example, exactly the same words on pages 137-42 as on pages 18-23. This is, however, a very slight reproach and, at a time when U.S. economists are more and more interested in European economic issues, I believe the reader will find this book very rewarding to study.

L. STOLERU

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*Euratom—Its Background, Issues and Economic Implications.* By JAROSLAV G. POLACH. Dobbs Ferry, N.Y.: Oceana Publications, Inc., 1964. Pp. xxi, 232. \$7.50.

The European Atomic Energy Community (Euratom) has received little attention in the economic literature of this country, chiefly because atomic energy in general has not as yet performed well—to use Dr. Polach's expression—"as an instrument of economic change and growth." Yet President Johnson recently remarked, probably referring to progress in reducing capital costs and extending fuel life, that "the long promised day of economical nuclear power is close at hand." The critical moment for the new industry to become an unsubsidized competitor ought to be just as close in Europe where conventional thermal plants are usually less efficient than in the United States.

Dr. Polach describes in this monograph (prefaced by Sam Schurr, the author's senior colleague at Resources for the Future) the short history of Euratom, analyzing the events preceding its birth in 1957, its original scope, its infatuation with nuclear power, subsequent disillusionment and courage to switch objectives and to persevere despite resistance or snubs from member and nonmember nations, failure to assert supranational authority, as well as some accomplishments in building the infrastructure for a European atomic industry.

The Spaak Report of 1956 stated that the new Community was to assist "in creating conditions favoring rapid growth of nuclear industry" and to facilitate the use of nuclear techniques in other industries by means of the following methods: investing in research and in projects "as could not be undertaken alone by individual industries or countries," promoting mobility of physical and human capital, setting safety standards, establishing a clearing house for knowledge and techniques, and by assuring equal access to nuclear fuels. But the Suez crisis made Europeans nervous about their precarious fuel imports, and Euratom felt motivated to focus its efforts on the immediate development of electricity-generating capacity. The *Target for Euratom*, a docu-

ment regarded highly both in Europe and the United States, reflected this preoccupation with the mere availability of quantities in its forecasts of energy demand, to the neglect of proper consideration given to future price relations between the various sources of energy. So in fact, as Polach points out, the cost of conventional fuel was overstated and that of atomic power production judged too optimistically. That the quick production of nuclear electricity was neither technically feasible at the time nor economically justifiable became clear when the expected cost per kilo-watt-hour had to be revised upward several times and when lower crude oil prices and a slight recession led to uncomfortably large coal inventories.

Appropriately, the Euratom Commission shifted the emphasis of its activities to long-term goals, especially research and the preparation of qualified personnel. Large-scale power production was still part of its research program, but maritime propulsion, isotope utilization and the study of nuclear fusion were also included. The Commission also decided not to go it alone but to join with its funds and manpower in a number of projects undertaken privately or publicly in member countries and even outside of the Community (Britain and Norway). Since 1961 it also shared (only inside the Community) in the operating expenses of electricity-generating pilot plants, where technological changes were so rapid that without such support "no significant progress could be expected in the near future." Even though the Commission has little direct control over the national atomic industrialization schemes, it has nevertheless evolved into more than a mere coordinating agency. It is also credited by Polach with having fomented a "bandwagon psychology" in favor of continued efforts to make nuclear energy competitive. Official response was somewhat weak, though, since member nations preferred to give priority to their own plans, thus intensifying the competition for scarce technical resources and qualified manpower. Member governments also violated their treaty obligations by entering directly into agreements on atomic matters with nonmember nations, or they by-passed the Commission by seeking specific advice from other international organizations of relevant competence. France, with its well-integrated nuclear industry has tried persistently to exert its influence with regard to the control of nuclear fuel, the scope of research programs, and more importantly, the development of reactors burning available natural, instead of imported, enriched uranium.

Besides, one may, in retrospect, raise the question whether the Community, in its haste to achieve independence from advanced Anglo-Saxon technology, did not sacrifice long-run gains to satisfy a somewhat insular phobia or snobishness, as the case may be. In view of the acute initial scarcity of trained and experienced personnel, more might have been achieved by placing less reliance on home-grown developments.

Dr. Polach told the epic of Euratom very well, and only a few minor points may be criticized. Some of the numerous abbreviations in the text and in footnotes cannot be found in the glossary at the front of the book; a more consistent use of total amounts rather than differences would improve the comparability of energy requirements, ditto for consistency in the measures

of fuels (hard coal equivalents, barrels, etc.); the exuberant grand finale conclusion exceeds the otherwise well-observed limited bounds of this study.

HANS G. MUELLER

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### **Land Economics; Agricultural Economics; Economic Geography; Housing**

*Resource Demand and Structure of the Agricultural Industry.* By EARL O. HEADY AND LUTHER G. TWEETEN. Ames: Iowa State University Press, 1963. Pp. ix, 515. \$8.50.

*Resource Demand and Structure of the Agricultural Industry* is the latest of several volumes on production economics to be developed under the direction of Professor Heady. It is an attempt "to describe how 'failure' elements of low income and 'success' elements of high productivity and capital accretion have their origins in the resource structure of agriculture" (p. 11). It is the authors' major contention that "while the problems of agriculture are directly those of commodity supply and price, basically they are problems of resource demand and supply" (p. 2).

The first five chapters of the volume form a rather separate unit. Together they provide the more general descriptive and theoretical background for the extensive empirical work that follows. For example, Chapters 1 and 4 deal with the relationship between economic development and resource use in agriculture, Chapter 2 gives a summary of resources used in U.S. agriculture since 1870, and Chapter 3 lays out the calculus of optimal factor use. Throughout these chapters, emphasis is on the relationship between product supply and derived resource demand, on factor substitution, and on the forces (such as education) that have influenced the resources used in U.S. agriculture.

The heart of the book is found in Chapters 6 through 15. Different portions of this unit are devoted to a rigorous empirical examination of the factors that influence the use of major inputs in agriculture, i.e., fertilizer, labor, plant and equipment, real estate, and operating inputs. Single-equation regression analysis is the major technique employed; and emphasis is on the predictive rather than the normative aspects of factor use. A large number of models are given for different regions, time periods, and levels of factor aggregation, and an attempt is made to estimate the effect on resources of changing price ratios, of complementarities between inputs, and of technical change. The last three chapters form the final unit of the book. Contained in this section are a regional input-output model of the agribusiness sector, an estimate of the aggregate (product) supply function, and an evaluation of the resource structure likely for the United States in 1980.

There is much in *Resource Demand* that is to be recommended. Any reader of the volume cannot help but be impressed with the immense amount of work that went into it. Estimates of over 400 equations are presented, and the ingenuity displayed in these models is impressive. Some of the more provocative findings suggest that "over 400 of the actual 512 percent increase

in weighted fertilizer and lime consumption from 1926 to 1959 remains to be explained by variables other than short-run price level," (p. 178); that with zero unemployment "a 10 percent decline in farm income relative to the income of factory workers could decrease the number of family workers [only] up to 1 percent in the short run," (p. 252); and that the price elasticity of demand for annual expenditures on plant and equipment "is approximately 1.0 in the short run (1 or 2 years) and more than 2.0 in the long run (3 or 4 years)" (p. 352).

In addition to the impressive empirical work, Chapter 2 provides an excellent historical summary of the changes in agricultural resource use in the United States. The discussion of farm investment behavior draws together the distributed-lag models relevant to agriculture in a neat, systematic manner, and the theory of static fertilizer demand presented in Chapter 6 is a skillful blending of agronomic relationships and economic theory.

Yet in spite of these merits and the tremendous labor input that went into *Resource Demand*, there are several reasons why the printed output is less than 100 per cent satisfying. First of all, major portions of the book are very difficult to read. A number of chapters were taken from different theses and research papers, and there is inevitable repetition and change of style. The presentation of so many equations makes for continual problems of notation; this difficulty is compounded by the authors' use of symbols rather than variable names in the text. In addition, the lack of adequate summaries at the ends of the empirical chapters makes it very difficult to comprehend easily the general meaning of the many statistical estimates.

One can also quarrel about the scope of the book. Individuals (such as the reviewer) looking for information about the effects of different types of government policy on the demand for resources in agriculture will be disappointed. Policy effects and policy conclusions are discussed rarely. There is nothing, for example, that deals directly with the current controversy of the effect of land retirement on the demand for fertilizer and other inputs. Even the supposedly broader sections that are concerned with "economic development" are limited to an examination of the U.S. situation since 1870. An additional chapter or two on policy matters and on the relevance of the U.S. findings for other countries would have assured without question that the volume had resulted in "more than a mechanical attempt to estimate demand functions of agricultural resources" (p. 12).

Finally, there are a number of issues that stem from the chapter on the prospective resource structure in 1980. While it is somewhat depressing to struggle with the elaborate models of the early chapters, only to find the final projections made primarily on a "straight-line" basis, these estimates should serve as a good stimulus to debate. Whether, in fact, the farm labor force will decline 48 per cent between 1960 and 1980 at the same time that total inputs increase by 10 per cent and total output by 47 per cent is dependent on a great number of factors, especially public policy towards agriculture. It is on the latter point that many will find the last chapter inadequate.

While few economists will find it worth while to examine in detail all of the

models presented in *Resource Demand*, many will find the central chapters an excellent source of empirical information. In addition, instructors will find the more general chapters good supplemental reading for advanced courses on the economics of U.S. agriculture.

WALTER P. FALCON

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*Transforming Traditional Agriculture.* By THEODORE W. SCHULTZ. New Haven: Yale University Press, 1964. Pp. xiv, 212. \$6.00.

The author's name guarantees this work will receive a wide, but not necessarily careful or objective, reading. In this small book Theodore Schultz, long-time conceptual-innovator in economics, continues to arouse intellectual ferment by attacking *dear dogmas* and *ill-construed concepts*.

He precisely defines traditional agricultural economies in terms of:

. . . an equilibrium at which agriculture gradually arrives over a long period provided particular conditions prevail. . . . The critical conditions underlying this type of equilibrium . . . are . . . (1) the state of the arts remain constant, (2) the state of preference and motives for holding and acquiring sources of income remains constant, and (3) both of these states remain constant long enough for marginal preferences and motives for acquiring agricultural factors as sources of income to arrive at an equilibrium with the marginal productivity of these sources viewed as an investment in permanent income streams and with net savings approaching zero (pp. 29-30).

His initial concern is with the relative costs of obtaining a given income stream from the alternatives of investment (1) in traditional factors and (2) in modern factors and technology. He first establishes the thesis that traditional agricultures are rather well organized and operating near the equilibrium maximum within their traditional resource framework. The cost of adding to the income stream within this context is "high" and accounts for the lack of noticeable savings-investment activity. In developing the thesis he disparages the widely accepted notion that agricultural labor in many such countries has zero marginal productivity.

He then considers the cost of adding to income streams by investing in modern factors and technology. This alternative provides potentially high rewards, i.e., is a relatively "low" cost means by which traditional agricultural economies may gain additional income streams. But the alternative involves a *transformation* process which is the central question and major concern of the writer. The incentives of farm people and the form of investments are both considered critical components in the process by which a traditional agriculture is transformed to a modern one. His proposition is that "differences in land are least important, differences in the quality of material capital are of substantial importance, and differences in the capabilities of farm people are most important in explaining differences in the amount and rate of increase in agriculture productivity" that occurs during transformation.

Schultz does discuss key aspects of the demand for and supply of both

traditional and modern factors. However, he keeps the reader's attention focused on the results emerging from the interplay of these forces, i.e., the *relative rates of return* from investments. He finds the rate of return to traditional factors low, providing a weak economic incentive, and the rate to modern factors high, providing a strong economic incentive to savings and investment.

It is quite widely believed by economists that the magnitude of the return to investment that is necessary to induce savings is greater in a traditional than in a modern agriculture. A conceptual justification is that people in a traditional economy have a higher marginal utility or preference for current above future consumption as a consequence of their low incomes. Lower levels of knowledge and higher risk and uncertainty of future attainments among people in traditional economies may give added weight to the argument. The implication of this line of reasoning is that economic incentives would have to be stronger in a traditional than in a modern agriculture to induce a given saving pattern. Although Schultz touches on some of these facets of demand for factors, he may have neglected to inquire into the relative strength of economic incentives between traditional and modern agricultures to the extent some readers would like. He was most concerned with emphasizing the evidence that people in traditional agriculture *do* respond to economic incentive, they are *not* handcuffed by custom and tradition, and that *economic incentives do play a major motivating role* in transforming a traditional agriculture into a modern one.

Some parts of Schultz's thinking will not be surprising to those who have kept abreast of his recent works. He labors somewhat to emphasize the importance of his general perspective that focuses on the transformation process as the major problem in inducing growth in underdeveloped countries. This reviewer shares the belief that it needs to be emphasized. Other perspectives that concentrate attention on the large returns that can be attained from relatively small investments in modern technology such as fertilizer, better seeds, and insecticides, have policy implications that are operationally questionable, if not logically distorted. They fail to bring to the forefront the prerequisite proposition that people and their institutions require economic conditioning if new factors and technology are to be absorbed and used efficiently.

Schultz's book is a welcome addition to economic development literature. It deserves careful study. A cursory reading and immediate reaction will likely indicate less "innovation" than is actually contained in the 212-page package.

CLARENCE A. MOORE

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*American Farm Policy, A Case Study of Centralized Decision Making.* By DON PAARLBERG. New York: John Wiley and Sons, 1964. Pp. xiv, 375. \$6.95.

Professor Paarlberg sees agricultural price supports as portents for a great issue of our times: "the degree to which decision-making should be an *individual* responsibility as contrasted with an *authoritarian* task." Rightly ac-

knowledging his approach to be empirical and pragmatic, a case study from which no scientific generalizations are warranted, he establishes his authority and offers his arguments and general conclusions. The author has clearly had unusual opportunities to observe and study farm programs during the last 30 years. Starting as a Hoosier farmer, he became an agricultural economist at Purdue, a position from which he took leave to serve as an administrator of agricultural programs in Washington during Eisenhower's administration. Students of farm policy, regardless of the degree to which they agree with Paarlberg's formulation of the problems or his conclusions, will find this factual, earnest, footnoteless volume a welcome and useful addition to their libraries.

Because the facts we choose to stress about farm price policy are largely determined by prior attitudes regarding the central problem, the large number of attitudes already recorded make it increasingly difficult to add anything both novel and relevant to the discussion. He has had, nevertheless, a degree of success. The message is short, clear, and direct. Government programs during the depression were "justified by the urgency of the times, by the failure of the free system to function in an appropriate manner, and by the lack of agreement on superior alternatives. . . . Government regulation of farm prices and production during the postwar period was harmful, in that the incentive prices provided more stimulation to the agricultural plant than the weak controls could overcome. . . . For some crops like tobacco, sugar and wool, individual decision-making does not appear to yield acceptable results." For them, centralized decision-making should be continued. But corn, cotton, and rice should again be returned to individual decisions, and that preferred status should be continued for livestock, poultry, fruits, and vegetables.

In its viewpoint, the book has much in common with Benedict (*Can We Solve The Farm Problem*. Twentieth Century Fund, 1955). Its orientation is less theoretical than Schickele's (*Agricultural Policy—Farm Programs and National Welfare*, New York, 1954). In regard to the inevitability of continuing governmental farm programs, the author's view is almost the opposite of that held by Hathaway (*Government and Agriculture: Public Policy in a Democratic Society*, New York, 1963).

For those who accept the author's premise that avoiding or limiting centralized decision-making really is the central problem of our time, the facts and arguments are likely to elicit ready acceptance and high approval.

Should one think that the level of employment, the distribution of income, or the rate of economic growth are the most important problems, he could use Paarlberg's facts to arrive at some very different conclusions.

In Chapter 22, for example, he outlines the dramatic adjustments and accomplishments realized in the farm sector during the past quarter century. They include wiser resource use which improved both the nutritive quality and the palatability of the American diet. Farms, enlarged to make use of the new technology, released to industry a stream of manpower such that farm population was reduced by 50 per cent. Geographic shifts in the location of crop production contributed to greater efficiency. Moreover, after these changes, nearly three-fifths of all farms were wholly owned by their opera-

tors; 62 per cent were free of real estate debts; and farm foreclosures in 1962 totaled only 1.6 per thousand in contrast to 39 per thousand in 1932. Could this mean that the \$17.7 billion dollars of realized cost of farm programs intended to stabilize farm prices and income from 1930 to 1959 (a sum equal to about one-third of one year's current military budget) may have aided adjustment? The author thinks not. "Most of these adjustments," he asserts, "were accomplished by the farm people themselves, without government programs. Some were achieved with the help of government-supplied research, education and credit. A number were achieved *despite* government programs."

Fortunately, we can learn a great deal about the politics of the commodity programs for "Corn, Which We Cannot Control" (Ch. 23), "Wheat, Which We Will Not Control" (Ch. 24), "Tobacco, Which We Have Controlled" (Ch. 25) and have the benefit of his good advice on "A Long Range Program For Agriculture" (Ch. 34), whether or not we find his economic metaphysics compelling.

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### Labor Economics

*Unionism and Relative Wages in the United States: An Empirical Enquiry.*

By H. G. LEWIS. Chicago: The University of Chicago Press, 1963. Pp. xvii, 308. \$7.50.

It has now become almost commonplace to deplore the unformed state of wage theory in the body of economic doctrine. Although some of the imprecision of labor market analysis is attributed to the economic "irrationality" of the individual market components, the principal *bête noire* is generally found to be unionism—a powerful and often dominating intermediary in the interaction of supply and demand.

As Marshall's lengthy discussions of the subject indicate, the controversy over the union's impact on wages is an old one, coincident with the growing strength of organized labor. In the United States, the controversy was aroused anew in World War II and the postwar years as a result of the major collective-bargaining agreements—with and without strikes—in such industries as coal, steel, and autos. Congressmen and practical men of affairs were convinced that these wage settlements contributed significantly to the inflationary pressures of the period. Economists—as is their wont—stepped in to show that, even in such industries as coal and steel, unionism had little power to raise wages beyond nonunion levels. Others sought to prove that the non-economic facets of trade union policy resulted in rounds of collective-bargaining wage settlements which were substantially above the competitive norm. There followed an outpouring of studies, utilizing a variety of techniques, time periods, and levels of analysis, with conclusions as varied as the methodology adopted. To the casual observer, the postwar studies on the impact of unionism are frequently conflicting, and the composite findings are incon-

clusive. To paraphrase Kenneth Boulding's rhythmic conclusion, "When workers unite/To take dough from the rich/Wages go up or they don't/I do not know which."

Fortunately, Professor Lewis is no casual observer in this field, and he discerns order in the chaos. He makes a notable contribution in his careful review of empirical studies of unionism and relative wages, and he finds that much of the seeming disagreement results from conceptual differences in the particular wage effects examined, differences in time periods studied, and differences in the authors' choice of language for stating their conclusions. Utilizing a synthesis of these studies and his own investigations, Lewis' main purpose is to estimate the magnitude of the impact of unionism on relative (percentage) wage differentials among groups of labor. He attempts to give quantitative answers to such questions as: (1) How much has unionism increased the average wage of union labor relative to the average wage of all labor, both union and nonunion? (2) How have the average wages of different industries been affected by unionism? (3) How variable were the effects of unionism on relative wages from one date to another during the last 40 years? (4) How much of the relative inequality in average wages among industries and in the distribution of wages among all workers can be attributed to the wage effects of unionism?

Lewis' most significant findings concern the differential impact of unionism on wages in different time periods. Significance is gained here because there is considerable uniformity on this point in the studies under review. Students of the labor movement have long had an intuitive feeling that unions are more effective in maintaining wages relative to the nonunion sector in depression than they are in surpassing nonunion wages in a period of full employment and inflation. Individual authors have provided quantitative evidence of this differential time-period impact in particular studies. Lewis' contribution lies in his generalization of these estimates over a 40-year period. At the depth of the Great Depression (1932-33), the effect of unionism on the average wage of union workers relative to the average wage of nonunion workers may have been above 25 per cent. By the end of the 1930's, the relative wage effect had declined to a level between 10 per cent and 20 per cent; and the decline continued until the average union/nonunion relative wage effect of unionism fell close to zero—under 5 per cent—at the peak of the postwar inflation in 1947-48. However, the declining effect was reversed in the 1950's, and it is estimated that in recent years the average union/nonunion wage was approximately 10 per cent to 15 per cent higher than it would have been in the absence of unionism.

These estimates, based on other empirical studies, are buttressed by Lewis' own quantitative study of the effects of changes in the general price level on the average union/nonunion relative wage. High rates of inflation during the period, 1920-58, strongly tended to reduce—and abnormally low rates of inflation to increase—the effect of unionism on the average union/nonunion relative wage.

The author's efforts to reach estimates of the wage impact of unionism among industries and among individual workers are less successful because

of inadequacies in the required empirical data. He estimates that the majority of workers are employed in industries whose average relative wages have been raised or lowered by unionism by no more than 4 per cent. In industries employing a small fraction of the labor force (less than 6 per cent), the relative wage effect is 20 per cent or more. It is estimated that in 1958 unionism increased the relative inequality of average wage rates among industries by about 6 to 10 per cent. At the same time unions *reduce* the average wage inequality *within* industries (but by no more than 5 per cent in the economy as a whole). Thus the effects of unions in changing relative wage inequality among all individual workers in the labor force are small, probably less than 6 per cent.

In arriving at his estimates of the wage impact of unionism, Lewis has been faced with serious methodological and data problems—problems which beset the studies he reviews, which inevitably weaken his own conclusions, and which will continue to plague research investigators in this field for some time. Only two of these issues can be mentioned here. First, is the “threat effect” of unionism on nonunion wages. In order to forestall a union organizing drive, nonunion employers may raise wages to parallel union-negotiated wages in the same industry or community. Such a result might constitute a considerable impact of unionism on wages, and yet this effect would be obscured in statistical comparisons between average wages in union and nonunion sectors, plants, and industries. Although Lewis is not unmindful of this problem, he tends to minimize its significance. And yet the indirect “threat effect” may have been of greatest importance in the late 1930’s and the 1940’s when the union organizing drive was strongest and the political climate was most conducive to unionism. This is the very period in which the measurable direct effects of unions on relative wages were found to be smallest.

A second difficulty stems from the unreliability of data on the extent and strength of union organization, especially in particular industries. Although Lewis makes adjustments in “official” data on collective-bargaining coverage, it must be recognized that alternative reasonable assumptions might be made, and these might result in substantially different estimates, thereby affecting crucially the correlations between the extent of unionism and average wages.

Even though some might argue with Lewis’ assumptions and adjustments in meeting particular methodological and data problems, it should be noted that he is explicit in the choice of the alternatives before him and careful in expounding the reasons for his selection. One can ask for little more in this difficult and controversial area of research. It is safe to predict that all future studies of the wage effects of unions will be indebted to Lewis for his work in this useful and important volume.

GERALD G. SOMERS

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*Workers Councils—A Study of Workplace Organizations of Both Sides of the Iron Curtain.* By A. STURMTHAL. Cambridge: Harvard University Press, 1964. Pp. x, 217. \$5.00.

The study examines the evolution of the works council, its operation in

two democratic and two totalitarian countries, and the relation of this institution to the labor movement, collective bargaining, and management. Several chapters evaluating these institutions and forecasting their future close this short and excellent study.

The works council is a special kind of worker organization, and it appears, at least to me, to differ from the shop groups established by an outside trade union to administer its contract and to aid its members. The trade union in the shop exists to protect the position of the worker it represents, and to promote his rights under the contract. It is true that, as Professor Sturmthal has noted, unions encroach upon managerial authority. They always have, and employers in the distant past were as concerned with the intrusion of the union in the domain of management as they are today. Yet there appears to be a difference between encroachment which is incidental to achieving certain protective purposes and the overt aim of intruding upon the collective powers of management.

The works council in France is traced to the pre-World War I syndicalists, from where it was carried and modified by the Italian fascists who adopted it as the corporate state. While the French syndicalists were, for their day, extreme revolutionists and antimilitarists, a number, including one of its leading editors, ended as supporters of Petain during the occupation. The idea had sufficient vitality to survive the liberation as the "plant committee." Its revolutionary origin does not mean that the works council is a vehicle for militant action. Sturmthal notes that "the main field of action of the plant committees . . . is the administration of the welfare activities of the enterprise."

Works councils in Germany have been, according to the author, more or less active since 1848. They had their day after the ousting of the imperial regime and were for a time centers for the more radical kind of activity. The detailed chapter reveals the reasons why councils are no substitute for a plant union. Aside from welfare activities, these councils do not appear to do very much. Moreover, their method of election makes them dependent upon the firm and frees them from the restraints which an outside union would impose upon activities. If an organization is to serve the interests of its members and not be a sort of dispenser of largesse, it has to speak with one voice.

The chapters devoted to Yugoslavia and Poland are interesting and informative. The works councils in those countries are not independent decision-making institutions. Governments recognize that force and pressure do not always yield the best results, especially over the long run. Like the legally inhibited American company unions, many of which were works councils, those east of the iron curtain can ameliorate minor inequities and mitigate the harshness and arbitrariness of subordinates, but they cannot oppose centrally determined policies. One need not deny that the councils may at times be a useful and even significant instrument for promoting labor welfare. But as Sturmthal notes, the decline in the position of the Polish councils began when they "were rapidly developing into a political threat to the party and the regime."

The four concluding chapters contain a number of cogent observations sup-

ported by reference to the discussions in the preceding chapters. One cannot avoid the conclusion that these councils are essentially agents of political groups or instruments of the employer. In Germany, as the author shows, the political parties seek to influence the councils because the postwar trade union movement is not closely linked to a particular party. All and all, the excellent study reveals that the councils have weakened the trade unions, that they perform minor functions, and why they are welcomed by the politicians of the left and those employers who would weaken organized labor.

PHILIP TAFT

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*Unemployment and the American Economy.* Edited by ARTHUR M. ROSS. New York: John Wiley & Sons, Inc., 1964. Pp. xiv, 21. \$4.95.

*Unemployment and the American Economy* is the result of the first annual conference of a four-year research program on unemployment at the Institute of Industrial Relations, University of California (Berkeley), sponsored by the Ford Foundation. The conference was held during April, 1963, and it "included about seventy-five scholars, government officials, and representatives of management, labor, and community organizations from various parts of the country." As might be expected in a volume representing the views of such a diverse group of people, the quality and interest of the papers and comments to professional economists vary greatly. Also predictably, the papers are of generally higher quality than the comments, with two notable exceptions.

The papers and comments are divided into three major sections. The first deals with retraining and labor market policies; the papers are by William Haber and Seymour L. Wolfbein, with comments by E. Wight Bakke, Nathaniel Goldfinger, and Gerald G. Somers. The second is concerned with the problem of expanding economic activity; the papers are by Walter W. Heller and Otto Eckstein, with comments by Albert Rees, Paul W. McCracken, and Don Vial. The final section deals with the question of what the United States can learn from European experience. The papers are by Jack Downie (deceased) and Robert J. Myers; the discussants are Robben W. Fleming, William H. Miernyk, and Richard A. Lester. Arthur M. Ross has written both an introduction and a conclusion to the volume.

The first section is moderately interesting and presents a useful summary and critique of U.S. labor market policy regarding retraining, unemployment compensation, information services, and the like. Gerald Somers' comments in this section are cogently put forward and are especially worth-while reading.

In the second section, the interesting papers by Heller and Eckstein are probably the best of the volume. They argue forcefully that the United States does indeed have an unemployment problem which is not merely the result of improper statistical procedures in measuring the unemployment rate, that the unemployment problem is most likely due to an inadequate rate of growth of demand for output, and that the late President Kennedy's tax reduction scheme is the most satisfactory method of combatting this situation.

The first two sections leave the reader with the impression that the present unemployment problem is more the product of aggregate than of structural phenomena, and the evidence presented in the Heller and Eckstein papers is persuasive, if not complete. For instance, Heller points out that:

The main conclusion to be drawn from the analysis is that changes in the overall unemployment rate explain most of the fluctuations in unemployment in the specific occupations and industries [most commonly thought to be the source of structural unemployment]. Moreover, in general, the relationship between unemployment in these occupations and industries and the overall rate has not changed. If unemployment in blue-collar occupations and goods-producing industries is correlated with the overall unemployment rate and time for the postwar period, the partial correlation coefficient for the time trend is not significant, except for construction. In some instances—nondurable goods manufacturing and skilled workers—the trend coefficient is negative (p. 100).

The feeling of the reviewer after reading Sections I and II is that policies designed to combat structural unemployment are best thought of as long-run policies which are desirable regardless of the stage of the business cycle. Desirable policies are those which assist individuals in overcoming imperfections in the spread of labor market information and in access to resources for the support of education and training. Such policies may be thought of formally as implementing wage subsidies to certain groups of individuals and occupations. During conditions of tight labor market conditions with rising prices that usually accompany them, relative wages are rather free to adjust to the conditions of demand and supply in such a way as to alleviate extreme unemployment differentials by age, occupation, industry, and race. Under conditions of slack demand, however, as long as wages tend to be sticky downwards, these adjustments are hindered. Therefore, in periods of relative prosperity, training and retraining programs and the like may help reduce what appears to be an almost inevitable concomitant upward trend in prices, and during periods of relative depression, such programs should help mitigate the rise in unemployment rates.

In his comments on Section II, Albert Rees accepts Heller and Eckstein's analyses of the nature of the unemployment problem in the United States, but he is critical of their assessments of the danger of inflation and the proper "tradeoff" between inflation and high employment. There is a thoughtful outline of the effects of inflation and the only explicit suggestion in the volume that an alteration in our international monetary arrangements might be a desirable and even a necessary component of an effective anti-unemployment program. He points out (pp. 138-39) that:

Professional economists owe it to the public, and in particular to the unemployed and their families, to bring the issue of gold policy into the arena of open discussion. . . . It is fortunate for American policy makers that our unemployed have been so patient, but can we count on this indefinitely?

The third section, on unemployment in Western Europe and the United

States, is perhaps the least interesting of the volume. This is perhaps because it appears that there is nothing spectacularly different in European experience which sheds new light on the U.S. unemployment problem. European unemployment does appear to be lower than that in North America, even after differences in definition are considered. This seems to be because output and the demand for labor in Europe have been growing more rapidly than in the United States and possibly also because the European nations have been willing to adopt somewhat more paternalistic labor market policies than has the United States.

In general, the papers and comments in this volume are of sufficient interest and quality to make them recommended reading for intermediate and advanced labor economics classes. There are some glaring examples of undefended venting of old prejudices and positions which are perhaps inevitable in the proceedings of a conference of this type. Some examples are:

... the kaleidoscopic changes in technology, occupational structure, industrial location [and] the more exacting standards of performance to which the economy is subject [indicate] that there will have to be more extensive economic planning in the future than we are accustomed to now (p. 209).

... we need a lesser redistribution of income via the taxing mechanism (p. 142).

... it is a [well?] known fact that the rich have been getting richer and the poor have been getting poorer (p. 148).

Nevertheless, there are sufficient sound analyses and recommendations in the volume to make it of interest to both students of economics and to those concerned with the formulation of public policy.

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### **Population; Welfare Programs; Consumer Economics**

*The Economics of Human Resources.* By H. CORREA. Amsterdam: North Holland Publishing Co., 1963. Pp. 262. \$7.00.

The economics of human resources, with its emphasis on the study of qualitative differences in the labor force, has become a subject of great interest to economists only in the past few years. This reasonably short book by H. Correa covers a wide range of topics in a burgeoning field, summarizing the existing state of knowledge in some places and extending it in others. The unifying thread is the elemental economic concept which permits the book to be divided into one half dealing with supply and the other with demand. Nevertheless the field has not advanced sufficiently to permit simultaneous consideration of all facets within an economic framework, and the various parts of the book therefore tend to live independent existences.

The first half is a review of the factors which work through the supply curve to determine the size and skill composition of the labor force. Most of the discussion deals with education, but the author also examines a wide

range of other determinants, including health, nutrition, and mental aptitudes. He has read widely in other disciplines, especially in education and educational psychology, and it is from a remarkable variety of sources that he brings references to bear on the issue at hand. Nevertheless this part of the book is not wholly satisfactory. Several times the author's line of reasoning comes to an abrupt halt because these other disciplines have not dealt with the problem he poses, so he is left with nothing to build on and must state problems without answering them. In a few other instances conclusions are reached only with the help of dubious assumptions or a questionable interpretation of the results of educational or psychological research. However, many of these inadequacies may be attributed to the author's having plunged into territory largely uncharted by the economist, and such a bold approach yields returns as well. The specialist in this field will profit from this survey of the research done by other disciplines and from the specification of problems yet to be treated in the depth they deserve. These problems amount to a catalog of needed interdisciplinary research.

In the book's second half, the author returns to more conventional economics and considers the relation between education and aggregate output. He does this first by assessing the growth of educational capital as a partial explanation of the large share of growth in output per worker which cannot be explained by increases in the stock of physical capital. This is a problem originally tackled by Schultz, and Correa follows Schultz's approach, as he acknowledges, but with some variation in both data and method.

Probably the most interesting chapters of the book are those concerned with mathematical models, presented in illustrative form and applicable to problems of educational planning, i.e., Chapters 8, 14, and 15. The last two contain an extensive exposition on educational programming in which an educational system has been superimposed upon a Harrod-Domar growth model. Output bears a fixed ratio not only to physical capital but also to each particular skill level in the labor force, and the function of the educational system is not only to supply the various skill levels in the desired quantities, but also to maintain itself through producing teachers. The model developed is a system of difference equations, and when Correa examines its stability conditions, he finds it to be unstable and explosive. Nevertheless a study of its equilibrium growth path is extremely valuable in showing the trend in the size of educational effort demanded by a growing economy. The model gains further interest as the author considers the educational effort required when the economy is raised to a higher equilibrium growth rate through an increase in the savings rate. By the workings of the model, a transition period of some length, during which the educational system builds up its stock of teachers, must precede the increase in savings and in the growth rate. If the growth rate is pushed up to some extent during the transition period, then more skilled people are drawn into production, and the stock of teachers is built up less rapidly.

A model of this sort should be of particular interest to planners in developing countries, where in many cases massive changes in the size of a nation's

educational system are occurring. It brings out very well the almost insuperable difficulties in realizing quickly the desire for universal primary education. However, while the model can thus provide useful guidance to the planner, it by no means solves all his technical problems. The statistical problem of determining appropriate education-output ratios is a difficult one in most countries, but an essential problem of educational planning is to see how flexible these coefficients can be made, not of measuring them once and assuming them fixed. This is particularly so when education is measured by years of school completed; this is a questionable technique in the United States, but is even more dubious in a less developed country, where quality is often a serious problem, and where it could be said that the educational problem is as much one of changing attitudes as of supplying knowledge.

There are two further elaborations of the model which deserve mention. In the first, separate consideration is given to each economic sector, so that the model becomes a dynamic input-output system, in which the educational sector is added to those conventionally included. In the second, income distribution is introduced into a simpler version of the model, and the conclusion is reached that "the concentration of income is detrimental to development, an idea contrary to that usually accepted" (p. 245). This reviewer must part company with the author on this particular conclusion, however, at least as a statement about the real world. It arises out of a series of unreal assumptions in the model: that capitalists save to invest only in physical capital, while workers save to invest only in education; that there are fixed factor proportions; and that investment in physical capital is realized very quickly, while investment in education is invariably realized only after a long gestation period.

Correa has written a book in a language other than his native tongue, an accomplishment to which the great majority of American economists, including this reviewer, must doff their hats. Nevertheless I must say with honesty that the book's literary style is not smooth, and the exposition is often more difficult to follow than it need have been. Therefore this may not be the book for the general economist who would like to learn a little about recent developments in the economics of education and human resources. For the specialist, however, and particularly for the educational planner, this is an important book which I would like to recommend.

SHANE HUNT

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*The Economics of American Medicine.* By SEYMOUR HARRIS. New York: The Macmillan Co., 1964. Pp. xvi, 508. \$8.50.

Probably no other field of enterprise has been so studiously avoided by U.S. economists as the distribution of medical benefit. Only a few experienced medical economists exist; they quietly enrich the curricula of three universities or busily collate statistics at The Brookings Institution or The Department of Health, Education, and Welfare. Nowhere in the United States does a competent and authoritative body speak for the outraged patient, as

it does in advanced societies elsewhere. When an occasional protest is heard against the absurd irony of U.S. medical practice—that amidst abundant facilities and affluent physicians there continue to be needless death and disability and bankruptcy generated through morbidity—it is uttered in most delicate phrases.

Such an occasional commentary is Seymour Harris' latest adventure in broad-spectrum economic analysis, *The Economics of American Medicine*. Here is set out, in just under five hundred pages, a rather summary discourse on factors influencing purveyors of medical and pharmaceutical goods and services and health insurance carriers in the pricing and allocation of their specialties. Those readers who are up to plowing through interminable qualifications (Harris calls them "offsets") and irritating classroom jargon will find solid proof that ratios of performance to cost in this sector are at a level which would be considered unacceptable in other sectors of the economy. Whether or not they are properly so seems to be Harris' main concern.

We are told that the over-all shortage of doctors, already severe, is steadily worsening. On the basis of a 45-hour week, there are not enough general practitioners in the country to allow each person an annual two-hour examination; and not enough doctors of any kind to permit everyone longer visits. Before the end of this decade the number of physicians must increase by 50 per cent, and 20 new medical schools must be opened, merely to maintain the present doctor-patient ratio. In an era of treatment whose greatest challenges are preventative medicine and degenerative disease, the trend of physician training offers bleak promise. One might add that the acute shortage of trained medical, psychiatric, and paramedical workers in public institutions is a national disgrace, as is the lack of reasonable accommodation and proper care for the chronically infirm. In the face of such knowledge it may seem odd that professional medical associations continue to agitate against expansion and to restrict opportunities for medical training and panel practice. However, it seems odd only until we begin to appreciate the significance of very high medical incomes (in relation to those of other professions requiring comparable training), the rapid advancement of fee schedules, fee-splitting arrangements between practitioners and specialists, and the custom among hospital specialists of holding out for a good percentage of the hospital's take. It is not really odd so much as it is lamentable.

What really is odd is Harris' repeated assertion that the physician "is often embarrassed by his high incomes." Should this be true, it is an embarrassment easily avoided. On the contrary, other studies have indicated that high income is a prime attraction for a majority of medical school candidates; and arrangements for salaried practice have been consistently (and often maliciously) opposed by medical associations on the ground that salary impairs "incentive"—presumably, the incentive to make a fast buck.

Harris' analysis shows that hospital expenditures have risen since World War II by something like  $2\frac{1}{2}$  times the rise in personal disposable income, and that hospital prices have risen by about three times the rate of all consumer prices. These are attributed largely to wage increases, costly innova-

tions for advanced therapy, increasingly expensive education and research, regional maldistribution of hospitals (paralleling maldistribution of income), and the relative increase in small and more costly hospitals. Divided responsibility in hospital administration is also regarded as contributing to "low productivity." With the drying up of philanthropy (about nine-tenths of total hospital revenue now comes from patients), U.S. hospitals face a dilemma similar to, albeit of lesser degree than, that facing Britain's hospitals before the War. British hospitals were saved by the National Health Service, while ours are being "bailed out" with insurance; although, on balance, the English solution would appear more humane. U.S. hospitals are still confronted by the perennial decision of how many indigents to turn away, when Victorian rules of eligibility enable local welfare authorities to avoid paying for their care.

The chapter on the British National Health Service reveals, for those not already aware of it, that NHS has successfully met Britain's medical needs to a degree unknown in the United States, and that it enjoys the full endorsement of all major professional associations (as do the compulsory programs in Scandinavia and elsewhere). Harris might also have noted that such compulsory government-financed programs operate at much lower cost than the U.S. system, both in terms of relative administrative expense and of the proportion of GNP required for adequate coverage.

There is an interesting chapter on the pharmaceutical industry (drawn largely from the 1961 Kefauver Subcommittee hearings), except for Harris' contention that "To encourage research, patent rights are necessary." Surely it is general knowledge that drug patent rights are commonly obtained by means unassociated with discovery, and that the discoverer is seldom the chief beneficiary of the patent on his drug. There is nothing to indicate that research has been less significant in countries with little or no patent protection; while, on the other hand, the history of U.S. patents in some drugs is fraught with restriction to competitive enterprise, interference suits leading to market-sharing among the litigants, and intense political pressures or cartel arrangements tending to internationalize the existing pattern of very high domestic concentration in the drug industry.

The serious student of medical economics will find this book disappointing. All of the statistical data antedate those in the more trenchant and comprehensive works already on the market; there is, for example, no significant addition to the excellent study by Somers for Brookings four years ago, despite the availability of more recent data. The newcomer to the subject will find the book provoking for its display of limited imagination (such as the old saw that health insurance induces claims, without mentioning the necessary precondition that coverage be less than comprehensive) as well as for its unmoving conclusions (that some solution must be found for the difficult problems that exist, and that government should subsidize free-enterprise medicine more heavily). It is a source of wonder that Harris never faces up to any practical solution for ending our exploitation of the sick.

He refers to Chapter 2 of his book as "largely statistical and hence dull."

Actually, Chapter 2 is no worse than the rest; they are mostly dull and show little evidence of revision since the day they went through the dictating machine. They are further labored by awkward metaphors (such as "the skyrocketing of incomes of physicians") and by habitual abbreviation of common phrases, such as "institutions of higher learning (IHL)" even on those occasions when the reference is not repeated.

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### Related Disciplines

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# NOTES

## *Deaths*

- Jules I. Bogen, New York.  
Bruce Futhey, professor of accounting and vice-chairman, School of Commerce, New York University, July 17, 1964.  
Herman D. Graham, professor of economics, Fresno State College, August 2, 1964.  
Thomas R. Hamilton, College Station, Texas.  
James Holladay, School of Commerce, University of Alabama.  
Macy M. Skinner, Baltimore, February 9, 1964.  
George A. Smalley, Chicago, August 9, 1964.  
J. C. Somers, Jackson Heights, N.Y.  
George G. Sommaripa, Tuxedo Park, N.Y.  
James A. Stranahan, Jr., Pittsburgh, May 10, 1964.  
Anthony Trimikas, Brooklyn, February 27, 1964.  
John Henry Urban, Lehigh University, June 30, 1964.

## *Retirements*

- John W. Carncross, professor of agricultural economics and marketing, Rutgers--The State University, July 1964.  
John C. Fetzter, professor of economics, School of Business Administration, University of Miami, May 1964.  
O. Rogers Flynn, professor of finance, Graduate School of Business, Columbia University.  
Howell A. Inghram, professor of accounting, Columbia University.  
George McCabe, instructor in economics, School of Business Administration, University of Miami, May 1964.  
James W. Martin, director, Bureau of Business Research, College of Commerce, University of Kentucky, December 1963.  
Mark C. Mills, Indiana University, June 1964.  
Robert D. Patton, professor emeritus, Ohio State University.  
John H. Shields, professor, department of economics, Duke University.  
Robert B. Stewart, dean, Fletcher School, Tufts University.  
Elmer Wood, professor emeritus of economics, University of Missouri, June 1964.

## *Visiting Foreign Scholars*

- Karl H. Borch, Norway: visiting professor, department of economics and School of Business Administration, University of California, Los Angeles, 1964-65.  
Rikard Lang, Yugoslavia: visiting professor of economics, University of Minnesota, fall 1964.  
M. M. Postan, Cambridge University, England: visiting professor of economics and history, and research economist, Institute of Industrial Relations, University of California, Berkeley, spring semester, 1964.  
Clifton Wharton, University of Malaya: visiting professor of economics, Stanford University, winter 1964.

*Promotions*

Khamis Abdul-Magid: associate professor of economics, College of William and Mary.

Frank J. Angell: professor of insurance, School of Commerce, New York University.

John W. Ashley: associate professor, University of Missouri.

Clinton A. Baker: associate professor of marketing, University of South Carolina.

Wallace Barr: associate professor, Ohio State University, July 1964.

Thomas R. Beard: associate professor, economics department, Louisiana State University.

Eugene A. Brady: associate professor, department of economics and sociology, Iowa State University.

William L. Breit: associate professor, economics department, Louisiana State University.

Troy J. Cauley: professor, economics department, Indiana University.

Frank J. Charvat: professor, School of Business Administration, Emory University.

Edward L. Claiborn: associate professor of economics, U.S. Air Force Academy.

Edwin William Crooks, Jr.: professor of marketing, College of Commerce, West Virginia University.

Martin David: associate professor of economics, University of Wisconsin.

Bruce Davie: associate professor, economics department, Georgetown University.

Frederick T. Downs: associate professor, New Mexico State University.

Bruce Duncombe: associate professor, economics department, Georgetown University.

Robert W. Gillespie: associate professor of economics, University of Illinois.

I. Bernard Goodman: professor of economics, Wayne State University.

H. Peter Gray: associate professor of economics, Wayne State University.

Ralph Hofmeister: assistant professor of economics, University of Minnesota.

John A. Hogan: Carter professor of economics, University of New Hampshire.

Charles H. Ingraham: assistant professor, Ohio State University, July 1964.

Vilho O. Jarvinen: associate professor of marketing, School of Commerce, New York University.

Harold L. Johnson: professor, School of Business Administration, Emory University.

Myron Joseph: professor of economics and industrial administration, Carnegie Institute of Technology.

John F. Kain: associate professor of economics, U.S. Air Force Academy.

Hiromitsu Kaneda: assistant professor, University of California, Davis.

Arno F. Knapper: associate professor of business administration, School of Business, University of Kansas.

John M. Kuhlman: professor of economics, University of Missouri.

Roy L. Lassiter, Jr.: professor, department of economics, University of Florida.

D. G. Lockett: professor, department of economics and sociology, Iowa State University.

Francis B. McCormick: professor, Ohio State University.

James L. McKenney: associate professor of business administration, Harvard University, July 1964.

S. Sterling McMillan: professor of economics, School of Business, Western Reserve University.

Allan H. Meltzer: professor of economics and industrial administration, Carnegie Institute of Technology.

Charles W. Meyer: associate professor, department of economics and sociology, Iowa State University.

Garland C. Owens: professor of accounting, Graduate School of Business, Columbia University.

Frank Palinay: professor of economics, Wisconsin State University, Whitewater.

Kenneth J. Rea: associate professor of economics, department of economics and political science, University of Saskatchewan.

Marvin E. Rozen: professor of economics, Pennsylvania State University.

Ansel M. Sharp: professor of economics, Oklahoma State University.

Charles P. Skaggs: assistant professor of accounting, College of Commerce, West Virginia University.

Dufferin S. Spafford: assistant professor of economics, department of economics and political science, University of Saskatchewan.

Case M. Sprengle: associate professor, department of economics, University of Illinois.

Dennis R. Starleaf: assistant professor, department of economics and sociology, Iowa State University.

Ada B. Thomas: assistant professor of business administration, University of South Carolina.

Robert W. Thomas: associate professor, department of economics and sociology, Iowa State University.

George W. Tomlin: professor of economics, University of South Carolina.

J. Robert Tompkin: professor, Ohio State University, July 1964.

Hylke Van de Wetering: assistant professor, department of economics and sociology, Iowa State University.

Paul L. Van Moescke: associate professor, department of economics and sociology, Iowa State University.

Vern Vandemark: assistant professor, Ohio State University, July 1964.

George Viksnins: associate professor, economics department, Georgetown University.

Francis E. Walkner: associate professor, Ohio State University, July 1964.

Neil Wallace: assistant professor of economics, University of Minnesota.

Leonard W. Weiss: professor of economics, University of Wisconsin.

Edward L. Whalen: assistant professor, economics department, Indiana University.

Marina Whitman: assistant professor of economics, University of Pittsburgh.

Jeffrey Williamson: associate professor of economics, University of Wisconsin.

Dallas M. Young: professor, department of economics, Western Reserve University.

Richard Zock: assistant professor of economics, U.S. Air Force Academy.

### *Administrative Appointments*

Ray A. Bailey: associate director, AID International Agricultural Credit Project, Ohio State University.

Morton S. Baratz: chairman, department of economics, Bryn Mawr College.

Edwin F. Beal: head, department of finance and business environment, University of Oregon.

George H. Borts: chairman, department of economics, Brown University, July 1964.

Thomas C. Campbell, Jr.: dean, College of Commerce, West Virginia University, April 1964.

Robert W. Cherry: chief, Cooperative Qualification Branch, Milk Marketing Orders Division, Agricultural Marketing Service, U.S. Dept. of Agriculture, August 1964.

Sherrill Cleland: dean of academic affairs, Kalamazoo College.

David C. Cole, AID, Washington: economic adviser and chief, economics department branch, U.S. Operations Mission to Korea.

William C. Freund, New York University: chief economist and director of investment research, Prudential Insurance Co.

John L. Fulmer, Georgia Institute of Technology: director, Bureau of Business Research, University of Kentucky.

Joseph Gartner: research director, Center for Consumer Affairs, University of Wisconsin-Milwaukee.

Mark R. Greene: acting dean, School of Business Administration, University of Oregon, academic year 1964-65.

Edmund Gullion, ambassador to the Republic of the Congo: dean, Fletcher School, Tufts University.

Arnold C. Harberger: chairman, department of economics, University of Chicago.

Dale S. Harwood, Jr.: head, department of accounting and business statistics, University of Oregon.

Harold J. Heck, U.S. Dept. of Commerce: professor and chairman, department of international trade and transportation, School of Foreign Service, Georgetown University.

Paul E. Junk: chairman, department of economics, University of Missouri, September 1964.

George E. Lent, U.S. Treasury Dept.: chief, Tax Policy Division, International Monetary Fund, Washington, D.C.

William J. McKeon: professor and acting chairman, department of marketing, School of Commerce, New York University.

James G. March, Carnegie Institute of Technology: dean of social sciences, University of California, Irvine.

John T. Masten: acting chairman of economics, College of Commerce, University of Kentucky.

John M. Mattila: acting chairman, department of economics, Wayne State University, 1964-65.

William H. Meckling, Franklin Institute: dean, College of Business Administration, University of Rochester, September 1964.

James A. Morris, University of South Carolina: board of directors, Charlotte Branch, Richmond Federal Reserve Bank.

Richard E. Neel: chairman, economics and finance programs, University of South Florida.

Murray E. Polakoff: chairman, department of economics, New York University.

Richard C. Porter, Yale University: associate professor of economics and associate director, Center for Research on Economic Development, University of Michigan.

W. F. Putman: assistant dean, School of Business Administration, University of South Carolina.

W. Dwayne Richins: acting head, department of finance and business environment, School of Business Administration, University of Oregon, academic year 1964-65.

Leonard G. Rosenberg, Institute for Defense Analysis: assistant vice-president, Surveys and Research Corporation, Washington D.C.

Ryuzo Sato: associate professor and chairman, department of economics, University of Hawaii.

James S. Schindler: dean, School of Business Administration, State University of New York, Buffalo.

Mervin G. Smith: director, AID International Agricultural Credit Program, Ohio State University.

Merton P. Stoltz: dean, Brown University, July 1964.

Theo V. Surányi-Unger: dean, Faculty of Economics and Social Sciences, Goettingen University, academic year 1964-65.

Jack T. Turner, Indiana University: professor and associate dean, College of Business Administration, University of Illinois.

Thomas E. Van Dahm, Southern Illinois University: professor of economics and chairman, business and economics division, Carthage College.

Pinkney C. Walker: dean, School of Business and Public Administration, University of Missouri, August 1964.

Paul W. Zickefoose: head, department of economics, New Mexico State University.

### *Appointments*

Russell L. Ackoff: professor, statistics and operations research department, Wharton School, University of Pennsylvania.

Gerald Albaum: assistant professor, department of marketing, University of Arizona.

Leslie P. Anderson: associate professor of finance, School of Business Administration, University of Oregon.

Lee D. Badgett: instructor in economics, U.S. Air Force Academy.

Haim Barkai: visiting lecturer, economics department, Wharton School, University of Pennsylvania, academic year 1964-65.

Robert A. Barry: instructor in economics, College of William and Mary.

Richard F. Barton: associate professor of business administration, School of Business, University of Kansas.

Amrit Baruah: assistant professor, Department of public administration, University of Arizona.

Merrill J. Bateman: instructor in economics, U.S. Air Force Academy.

Robert W. Beckstead: assistant professor, New Mexico State University.

Gordon C. Bjork: assistant professor of business, Graduate School of Business, Columbia University.

Victor Z. Brink: professor of business, Graduate School of Business, Columbia University.

Glenn E. Burruss, University of Kentucky: senior staff economist with *Business Week*.

Benjamin C. Butcher: assistant professor, department of marketing, University of Arizona.

Adhemar A. Byl: instructor, economics department, Georgetown University.

David J. Cantor: assistant professor, economics department, Boston University.

Edwin H. Caplan: associate professor of accounting, School of Business Administration, University of Oregon.

Robert S. Carlson: assistant professor of business, Graduate School of Business, Columbia University.

Janet Chapman: visiting associate professor of economics, University of Pittsburgh, 1964-65.

Frank C. Child, University of California, Davis: professor of economics, University of California, Davis and Berkeley, spring semester, 1964.

John C. Clendenin: visiting professor of finance, School of Business Administration, University of Oregon.

Harold A. Cohen, Cornell University: assistant professor of economics, University of Georgia.

Robert E. Coleberd, Jr.: associate professor, department of economics, Western Maryland College, September 1964.

Robert W. Crowe: associate professor of insurance and W. Frank Hipp chair of insurance, University of South Carolina.

J. D. DeForest, Agency for International Development: Michigan Fellow in College Administration, University of Michigan, academic year 1964-65.

Lynn E. Dellenbarger: associate professor of finance, West Virginia University.

Eberhard W. Dinkelacker: instructor, economics department, Georgetown University.

- Peter C. Dooley: assistant professor of economics, Pennsylvania State University.
- Bert E. Elwert, Indiana University: assistant professor of business administration, University of Illinois.
- Hugh N. Emerson: professor of economics, Texas College of Arts and Industries.
- Donald R. Escarraz: assistant professor of economics, University of Florida.
- Delbert A. Fitchett: logistics department, RAND Corporation.
- John R. Foster: assistant professor of marketing, University of South Carolina.
- Glenn C. Gambles: instructor in economics, University of Maryland.
- Ved P. Gandhi: assistant professor of economics, University of Massachusetts.
- David T. Geithman: interim assistant professor of economics, University of Florida.
- George Gelson, Georgetown University: College of Notre Dame.
- Harry I. Greenfield: assistant professor of economics, Queens College, City University of New York, and research economist, Conservation of Human Resources Project, Columbia University.
- Karl D. Gregory: assistant professor of economics, Wayne State University.
- David M. Grether: acting instructor, department of economics, Stanford University.
- Jack M. Guttentag, University of Pennsylvania: research staff, National Bureau of Economic Research, June 1964.
- George R. Hall: logistics department, RAND Corporation.
- James F. Halstead: acting instructor, department of economics, Stanford University.
- Sidney S. Handel: logistics department, RAND Corporation.
- Hugh G. Hansen: associate professor of economics, University of Wyoming.
- W. Lee Hansen: associate professor of economics, University of Wisconsin.
- Robert Harbison: instructor in accounting, School of Business, University of Kansas.
- M. F. Hassan: assistant professor of economics, School of Business Administration, University of Miami.
- Robert Hawkins: instructor, department of economics, New York University.
- Sandra Henderson: instructor in economics, West Virginia University.
- Roy J. Hensley: professor of economics, Lehigh University.
- Dale L. Hiestand: assistant professor of business economics, Graduate School of Business, Columbia University.
- Glenn C. Himes: assistant professor, department of agricultural economics and rural sociology, Ohio State University.
- David B. Houston: associate professor of economics, Pennsylvania State University.
- Charles W. Hultman: visiting associate professor, University of California, Riverside, academic year 1964-65.
- Robert Jacobson: associate professor, department of agricultural economics and rural sociology, Ohio State University, November 1964.
- Donald B. Jeasing: assistant professor of economics, Columbia University.
- Glenn L. Johnson: assistant professor of business administration, School of Business, University of Kansas.
- Bob M. Keeny, University of Kansas: School of Business, University of Missouri, Kansas City.
- Herbert J. Kieseling: lecturer, department of economics, Indiana University.
- Marshall Kolin, Harvard University: assistant professor, department of economics, New York University.
- Richard F. Kosobud: visiting associate professor, department of economics, Wayne State University, 1964-65.
- Melvyn B. Krauss, Brooklyn College: instructor, economics department, New York University.

- Joseph Krislov: professor of economics, University of Kentucky.
- W. E. Kuhn, Federal Reserve Bank of Chicago: Fulbright lecturer, University of Dacca, academic year 1964-65.
- Paul W. Kuznets: assistant professor, department of economics, Indiana University.
- Richard D. Lentz: instructor in economics, University of Maryland, September 1964.
- Youngil Lim: assistant professor of economics, University of Hawaii.
- John V. Lintner, Jr.: George Gund professor of economics and business administration, Harvard University, July 1964.
- John D. Logsdon: assistant professor of business administration, School of Business, University of Kansas.
- John Y. Lu, IBM: economist, logistics department RAND Corporation.
- Gary A. Luoma: assistant professor, School of Business Administration, Emory University, September 1964.
- William F. Lyle: instructor in marketing, West Virginia University.
- P. J. Lymberopoulos, University of Texas: assistant professor of business statistics, University of Colorado, September 1964.
- Arthur E. Mace: professor of business statistics, School of Business Administration, University of Oregon.
- Anthony Mach: instructor, department of economics, Western Maryland College.
- Donald C. Marschner: associate professor of business administration, University of New Hampshire.
- Wayne S. Marshall: assistant professor of business, Columbia University.
- Otto B. Martinson: instructor in economics, U.S. Air Force Academy.
- Mya Maung: assistant professor of economics, South Dakota State University.
- Harendra Mazumdar: visiting professor of economics, University of Hawaii.
- John F. Mead: instructor in economics, University of Kentucky.
- Kenneth W. Meinken, Rutgers—The State University: econometrician, Connell and Co.
- William C. Merrill: assistant professor, department of economics and sociology, Iowa State University.
- Thomas E. Miller, University of Kansas: School of Business, University of Missouri, Kansas City.
- Victor U. Mok: assistant professor of economics, Saint Mary's College.
- Donald R. Monath: assistant professor of finance, University of South Florida.
- Charles C. Moore: assistant member, Industrial Workers Division, International Labour Office, Geneva.
- Walter A. Morton: visiting professor of economics, University of Missouri.
- Carolyn C. Mueller: assistant professor of accounting, University of South Carolina.
- Robert A. Mundell, McGill University: visiting research professor of international economics, Brookings Institution, academic year 1964-65.
- John C. Narver: assistant professor of marketing, University of British Columbia, July 1964.
- Myron B. Neace: assistant professor, School of Business Administration, Emory University, September 1964.
- Michael Nicholson, Carnegie Institute of Technology: University of Stockholm, Sweden.
- James W. Nordyke: associate professor, New Mexico State University.
- Jeffrey Nugent: assistant professor, economics department, University of Southern California.
- James R. O'Connor: visiting assistant professor, department of economics, Washington University, academic year 1964-65.

- Charles F. Peake: instructor in economics, University of Maryland, September 1964.
- Joseph M. Perry: interim assistant professor of economics, University of Florida.
- William H. Peterson, New York University: economist, U.S. Steel Corporation.
- Richard C. Pickering: Office of Research and Home Finance, Federal Home Loan Bank Board.
- Fred B. Power: instructor in finance, University of South Florida.
- George F. Rohrlich, International Labour Office, Geneva: visiting professor, School of Social Service Administration, University of Chicago, October 1964.
- Richard Rosenberg: instructor in economics, Pennsylvania State University.
- Robert L. Sarna: instructor in accounting, School of Business, University of Kansas.
- Frank H. Sargent: assistant professor, department of economics and business administration, Norwich University.
- Norman Schneider: assistant professor, University of California, Davis.
- Nancy Schwartz: assistant professor of economics, Graduate School of Industrial Administration, Carnegie Institute of Technology.
- Robert Shapiro: assistant professor, department of economics, New York University.
- Robert P. Shay: professor of banking and finance, Graduate School of Business, Columbia University.
- Ching Sheng Shen: assistant professor of economics, Lehigh University.
- Hale T. Shenefield: visiting professor, department of economics, University of Southern California.
- Edward W. Shows: instructor in economics, University of South Florida.
- Larry D. Singell: instructor, department of economics, Wayne State University.
- Roger L. Singleton, University of Kansas: Standard Oil Co. of New Jersey.
- Edward K. Smith, Area Redevelopment Administration: department of economics, Boston College.
- Merlin Spencer: assistant professor, School of Business, University of Kansas.
- Howard Steele: department of agricultural economics and rural sociology, Ohio State University.
- Donald F. Swanson: visiting associate professor of economics, University of South Carolina.
- Atsushi Suzuki: lecturer, department of economics, Indiana University.
- F. Corine Thompson: interim assistant professor of statistics, University of Florida.
- Richard Thorn, City University of New York: associate professor of economics, University of Pittsburgh.
- Yien-L Tu: assistant professor of economics, University of Kentucky.
- Evangelos Voloudakis: economic research department, Bank of Greece, Athens.
- Franklin V. Walker: economist, Board of Governors. Federal Reserve System, June 1964.
- Ross G. Walker: visiting professor, School of Business Administration, Emory University, September 1964.
- Cornelius F. Walsh: instructor in economics, Trinity College.
- Howard E. Warren: instructor, department of economics, Western Maryland College.
- Frederick E. Webster, Jr.: assistant professor of marketing, Graduate School of Business, Columbia University.
- Burton A. Weisbrod: associate professor of economics, University of Wisconsin.
- Howard C. Williams, Ohio State University: National Agricultural Advisory Commission for three years.

Jean Wilbrun: visiting assistant professor of economics, Barnard College, academic year 1964-65.

Marilynn G. Winborne: assistant professor, department of accounting, University of Arizona, September 1964.

John Wise: visiting professor of economics, University of Hawaii.

J. N. Wolfe, University of California, Santa Barbara: professor of economics, University of Edinburgh.

Yi-Chang Yin: visiting assistant professor, economics department and Institute on Communist Strategy and Propaganda, University of Southern California.

### *Leaves for Special Appointments*

Clarence L. Barber, University of Manitoba: visiting professor of economics, department of economics and political science, McGill University, September 1964.

Thomas R. Beard, Louisiana State University: visiting professor, Board of Governors, Washington, D.C., 1964-65.

Harold K. Charlesworth, U.S. Department of Commerce: visiting associate professor, University of Texas, 1964-65.

C. H. Donovan, University of Florida: visiting professor, University of Khartoum, Sudan, September 1964 to April 1965.

R. B. Eutsler, University of Florida: professor of business administration, University of Lagos, Nigeria.

Walter Galenson, University of California, Berkeley: visiting professor, economics department, Stanford University, fall 1964.

Marion H. Gillim, Barnard College: Public Finance Adviser, U.N. Economic Commission for Latin America, Mexico City, academic year 1964-65.

Bruce B. Glassburner, University of California, Davis: chief adviser, Institute of Development Economics, Karachi, Pakistan, 1964-65.

Charles W. Howe, Purdue University: University College, Nairobi, for one year.

Clifton H. Kreps, Jr., University of North Carolina: professor, department of economics, Duke University, fall semester 1964-65.

Henry D. Lytton, Veterans Administration: Council for International Progress in Management, Peruvian National Productivity Center, Lima, Peru, August-October 1964.

Allan H. Meltzer, Carnegie Institute of Technology: visiting professor, department of economics, University of Chicago, academic year 1964-65.

Melvin W. Reder, Stanford University: visiting professor of economics, University of California, Berkeley, spring semester 1964.

Richard T. Selden, Cornell University: Fulbright research grant and Guggenheim Fellowship, European Economic Community Headquarters, Brussels, Belgium, January to August, 1964.

Jati K. Sengupta, Iowa State University: visiting associate professor of economics, Institute of Management, Calcutta, India, academic year 1964-65.

John H. Sitterley, Ohio State University: head, Ohio State Contract Team, Sao Paulo, Brazil.

Howard Steele, Ohio State University: Ohio State Contract team, Sao Paulo, Brazil, for two years.

Jack D. Steele, University of Kansas: Stanford University, academic year 1964-65.

R. Dean Tousley, Washington State University: Fulbright lecturer, Netherlands School of Economics, Rotterdam, 1964-65.

Harris L. Wofford, Columbia University: resident coordinator, Columbia University-Robert College Exchange Program, Istanbul, Turkey, 1964-66.

Dallas M. Young, Western Reserve University: Institute of Labor and Industrial Relations, University of Illinois, first semester 1964-65.

*Resignations*

- John H. Bassett: University of New Hampshire, July 1964.  
Alan B. Batchelder: Ohio State University, September 1964.  
Lloyd J. Elliott: College of William and Mary.  
William L. Furlong: Emory University, August 1964.  
James D. Hammond: Ohio State University, September 1964.  
Fred Hanga: Lehigh University.  
Harold R. Hartzler: West Virginia University.  
Russell W. Johnson: University of New Hampshire, July 1964.  
James R. Leonard: Ohio State University, September 1964.  
Eugene A. Mattis: West Virginia University.  
Ronald D. Michman: University of New Hampshire, July 1964.  
Michael K. Mišchaikow: West Virginia University.  
John R. Moroney, Jr.: Lehigh University.  
Ramon Myers: University of Hawaii.  
Stanley A. Nicholson: College of William and Mary.  
Eugene R. Schlesinger: New York University.  
Joginder Uppal: University of Hawaii.  
John H. Wicks: Ohio State University, September 1964.

## VACANCIES AND APPLICATIONS

The Association is glad to render service to applicants who wish to make known their availability for positions in the field of economics and to administrative officers of colleges and universities and to others who are seeking to fill vacancies.

The officers of the Association take no responsibility for making a selection among the applicants or following up the results. The Secretary's office will merely afford a central point for clearing inquiries; and the *Review* will publish in this section a brief description of vacancies announced and of applications submitted (with necessary editorial changes). Since the Association has no other way of knowing whether or not this section is performing a real service, the Secretary would appreciate receiving notification of appointments made as a result of these announcements. It is optional with those submitting such announcements to publish name and address or to use a key number. Deadlines for the four issues of the *Review* are February 1, May 1, August 1, and November 1.

Communications should be addressed to: The Secretary, American Economic Association, 629 Noyes Street, Evanston, Illinois. 60201.

### *Vacancies*

*Labor economists and/or statisticians:* The measurement of the economic damages suffered by those who are wrongfully injured or killed is a new area for the application of economic and statistical knowledge and research techniques. This field has been pioneered successfully and Associated Appraisers of Impaired Earning Capacity is now providing this service to attorneys on a nationwide basis. Those in it appraise the losses and are required to testify as an expert in court trials and must withstand hostile cross-examination. It is one of the highest-paid forms of work in the profession and it is possible to engage in it as a part-time activity. Requirements: (1) minimum status of associate professor; (2) experience in research in collective bargaining agreements and fringe benefit programs; (3) location in or near major metropolitan area. Send résumé and list of publications to: Philip Eden, President, Associated Appraisers of Impaired Earning Capacity, 1303 Walnut St., Berkeley, Calif. 94709.

*Economists and Econometricians:* We seek several outstanding men or women for the professional staff of a small management consulting firm whose principal clients are blue-chip industrial companies. Postgraduate training in economics at a leading university and several years' experience in business, consulting, or teaching required. New York City location. Ten per cent travel. Salary open and depends on qualifications and previous achievements.

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*Micro- and macrotheory; urban and regional economic development:* Need a person in each of these areas. Candidate with Ph.D., preferably with experience. Salary with minimum experience about \$8,500 for nine months. Employment during one of two five-week summer sessions usually available. Municipal university located in urban midwest. Opportunity for research. Liberal fringe benefits. Opportunity to teach a graduate course (M.A.) level. Twelve-hour teaching load, of which one-fourth must be taught after 4:00 P.M. Applications and credentials may be sent to: Dr. E. J. Steele, Economics Department, Omaha University, Omaha, Nebraska.

*Economist:* Two positions open. \$9,096 to start; 5 per cent annual increments to \$11,616. Master's degree in economics or regional science, plus 4 years of responsible research experience or an equivalent combination of experience and training. Ph.D. in economics or regional science with emphasis on econometrics may be substituted for two years of experience. One year of research utilizing econometric techniques in development and use of regional input-output tables and economic models may be substituted for two years of general research experience. Position 1: With Research and Analysis Division of Department of Planning and Economic Development, State of Hawaii, to conduct research, prepare detailed reports, make recommendations relating to revision of State General Plan and achievement of State's economic goals.

# AMERICAN ECONOMIC REVIEW

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PART 2

MARCH, 1964

SUPPLEMENT



## SURVEYS OF FOREIGN POSTWAR DEVELOPMENTS IN ECONOMIC THOUGHT

1. The Old and the New—Some Recent Trends in the Literature of  
German Economics K. W. Rothschild
2. Some Developments of Economic Thought in the Netherlands H. Theil

Price \$1.00

## FOREWORD

The two essays appearing in this supplement of the *American Economic Review* are the first of a series of papers recounting and assessing post-war developments in economic thought in leading professional centers where English is not the conventional language in use. This venture was made possible by a grant by the Ford Foundation to the American Economic Association. The Committee is pleased to make these essays available to the membership.

In extending our invitation, the Committee asked each author to consider whether the impact of postwar policy problems had been significant to the development of thinking and research, or whether the evolution of professional work had taken a more detached course. We also urged him to concentrate upon a few major themes. Notwithstanding these suggestions, each author was given full freedom to develop his topic as he envisaged it. In consequence, each essay represents his personal judgment of what he considers to be the important developments in thought in the country or region with which he is concerned.

Committee on Surveys of Foreign Economic Research

ABRAM BERGSON

MARTIN BRONFENBRENNER

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# THE OLD AND THE NEW— SOME RECENT TRENDS IN THE LITERATURE OF GERMAN ECONOMICS\*

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Unfortunately there is no word corresponding to "Anglo-Saxon" which covers the German-speaking countries and regions. Even the Northern European countries with their different languages can be conveniently grouped under the title "Scandinavian." No such shortcut is open to the reviewer of literature written in German. Though most of the writing naturally comes from Germany, accuracy would require the constant use of such clumsy phrases as "economic theory in Germany, Austria, and Switzerland" or "economic theory in the German-speaking countries." To simplify matters I shall be less exact and let the word *German* frequently stand for developments in all three countries.

A second complication arises from the division of Germany into two parts. In this article, only the literature of West Germany is taken into

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account. East Germany has also produced a considerable amount of economic literature since World War II, but since research there is predominantly on Marxist lines and is closely linked to the discussions going on in the Soviet Union and Eastern Europe, its presentation is best suited to a study of theoretical developments in the socialist countries.

Needless to say, nothing like a complete review of German economics will be aimed at. Limitations of space and of the author's knowledge alike rule out any such attempt. All that can be hoped for is a sketch of some of the major trends—old and new—which influence current economic thought in the German-speaking countries. The choice of authors and books to illustrate my points is necessarily somewhat haphazard, but it is hoped that in the end the reader will have gained some feel of what is going on in this part of the world.

### I. *The Advance of Macroeconomics*

It would be much easier for the reviewer of current economic thinking in Germany, Austria, and Switzerland to arrange his material if there were any clear-cut "schools" that originated in these countries in the postwar period. At the turn of the century the "Austrian school," the German "historical school," the "Kathedersozialisten," the intense Marxist controversy (outside the universities) would have provided obvious starting points for a panorama of German economic theory. Today there are no such definite German "schools," neoliberalism perhaps being the only exception. But neoliberalism has—as we shall see later—little to offer in the field of economic theory that is new. The main stress of the neoliberal writings lies on programmatic aspects.

The difficulty of detecting any special German schools may be partly a matter of time. I do not know how long the Austrian school or the Swedish school actually existed before they were clearly perceived as separate groups. It is certainly easier to find the common ground of different and differing writers in retrospect than during the process of "school" formation. Furthermore, present day circumstances are not conducive to the existence of a multitude of different schools. First, economic debate is far more world-wide today than it used to be (with Anglo-Saxon publications playing a leading and unifying role), leaving less room for national and regional enclaves. Second, economic theory is getting more and more specialized and departmentalized, and this reduces the scope for grand methodological line-ups.

Even so the dim outlines of some special theoretical groups can be discerned in the postwar world. One could think of the sociological approach of some French authors, of the "Chicago school," of the Tinbergen group in the Netherlands, the Ragnar Frisch group in Norway, and others. I doubt whether it would be fruitful to try to discover the

nucleus of some such special schools in postwar Germany. Typical of postwar German economics has been a broad stream of economic writings covering a very wide field of interests and methods. This stream combines contributions from various sources and differing traditions. It is the special *mixture* of past and present strands of thought going into current research that gives German economic literature its own distinct character. By tracing some of the major influences on present-day German economics, I hope to provide a preliminary glimpse of a vast, and not easily digestible, field.

The years of fascism and still more those of war—twelve rather decisive years in the development of Anglo-Saxon economic theory—left German economic theory isolated from the big discussions that spread so quickly from Keynes' Cambridge to the English-speaking and Scandinavian countries. This was necessarily so in Germany and Austria, which for political reasons were more or less cut off from the ideological currents in the democratic countries. But to some extent it also applied to Switzerland, where no such *political* impediments existed. The influence of German traditions was strong enough to let the new economics filter through only slowly.

A good example is the first volume of Walter Adolf Jöhr's extensive work on the theoretical foundations of economic policy which deals with the problem of perfect competition and which appeared in St. Gallen in 1943 [28]. Jöhr, who had spent some months in Harvard and Chicago, includes in his work all the important results of Joan Robinson, E. H. Chamberlin, and R. Triffin on competition and monopoly; but they are so imbedded in basic remarks about the economic order, in numerous metaphysical and psychological asides, and in all sorts of additional economic considerations that the difference between this and a contemporary English book on the same subject is at once obvious.<sup>1</sup> The difference becomes the more noticeable when we look at the second volume of Jöhr's large work which appeared nine years later [29]. In this comprehensive (700-page) study of the trade cycle, which can stand comparison with G. Haberler's celebrated work, we still find a comparatively large part devoted to older and now mostly neglected German sources; but the breakthrough of the new theories that had taken place in the meantime is evident. In addition to J. A. Schumpeter, we find Keynes, Hansen, Harrod, Hicks, and Samuelson among the most frequently quoted authors.<sup>2</sup>

<sup>1</sup> I do not mean to pass judgment on the relative merits of the two kinds of approach. All I want to stress is that in those days the "new economics," though not inaccessible to the Swiss, had not made much imprint on their leading works.

<sup>2</sup> While Jöhr gives full account of the various "mechanistic" trade cycle theories in vogue, he himself inclines toward a "non-deterministic" psychological theory. He draws particular attention to certain aspects of mass psychology which he regards as a decisive force in the cumulative processes of the cycle.

In some cases Swiss writers *did* join the bandwagon of "modern" macroeconomics and econometrics at a rather early period with interesting and critical contributions.<sup>3</sup> But, on the whole, these were exceptions and it remains true that Switzerland, not much less than Germany and Austria, remained comparatively unaffected by the winds that were blowing so strongly beyond the Channel and the Atlantic. In a review of Alec Cairncross's *Introduction to Economics* [11], written after the war, Emil Küng could still say that this book and Benham's *Economics* are "an excellent introduction into the economic way of thinking at present practiced in the Anglo-Saxon countries which is bound to have a growing influence on Switzerland—and not to the country's disadvantage" [64, p. 666].

When the war was over, the door was opened in Germany and Austria to an influx of the new ideas. What had been developed rather organically in long drawn-out discussions in Britain and America could now pour in rather suddenly in its semi-finished or finished forms. There was, to begin with, little time for a process of sifting and mixing. Different streams of thought rolled on side by side, connected by few and unsatisfactory communication systems.

The entry of the new economics demanded first of all rather wide surveys in order to enable the students and the younger economists to get a general view of the existing state of modern economic theory as it had developed outside Germany and, in particular, in the Anglo-Saxon and Scandinavian countries. At this stage, textbooks had a more than ordinary function to fulfill.

Two texts which came out rather soon after the war did play a decisive part in spreading the new economics. On a more elementary level, Andreas Paulsen's introduction to Keynes [43] gave numerous students and interested laymen the first extensive and reliable (though not quite systematic) survey of Keynesian and post-Keynesian economics. On a quite different level, we find the three-volume *Introduction to Economic Theory* by Erich Schneider [52]. Now in its eighth edition with improvements and revisions constantly being made, this work represents one of the best introductions to modern economic analysis available, not alone in the German language but quite generally. Schneider, who has taught in Denmark as well as in Germany, is able to blend Scandinavian and German contributions with the substance of contemporary Anglo-Saxon theory and to present it in a lucid style, supported but not displaced by mathematics and geometry. His text has helped the students of the postwar years to enter "into the

<sup>3</sup> See, for instance, H. G. Bieri, *Die neuen Konjunkturtheorien von Keynes, Hawtrey and Ohlin in kritisch vergleichender Betrachtung*. Wollishofen 1944, or the contributions by H. Bohl and H. Dutschler in *Konkurrenz und Planwirtschaft*. Bern 1946.

secrets of macroeconomic dynamics,<sup>4</sup> a generation of students which in the meantime has moved into leading positions in national and international statistical offices, in the civil service, and in research institutes" [8, pp. 218-19].

A short sketch of the structure of this work (as it presents itself in its later editions) may not be out of place. Like J. R. Hicks in his *Social Framework*, Schneider introduces his reader to economics by giving him a view of the interdependence of the whole system. This is the subject of the first volume. The process of national income formation is thoroughly described with the aid of "social accounting." Current methods of representation are generously used: national accounting, equations, flow diagrams, and input-output tables. Earlier macroeconomic approaches—Quesnay, Marx—are brought in for comparison. In the end the reader is familiar with the important formal relationships among the basic macroeconomic aggregates. The second volume deals with the traditional subject of microeconomic statics. The theory of demand is derived with the aid of indifference curves, and a linkage between the micro- and macroeconomic consumption function is established. The theory of production, often neglected in textbooks, is well developed, not least because this is a field in which Schneider has been actively engaged for many years.<sup>5</sup> Next the different varieties of competition and monopoly and the revenue plans of the firms are discussed. This leads to a treatment of the demand for factors of production. Price theory (including interest theory where the liquidity preference and loanable funds theories are combined) is at first developed from selected partial equilibrium cases, while the final chapter of this volume closes with an extended version of (Walrasian) general equilibrium. (The extension does away with Walras' full employment assumptions.) The third volume brings together the threads of the first two volumes. After an exposition of money and its functions, a discussion of the dynamics of the whole system is presented, first in a purely private economy, later with special attention to government activities. The instruments of modern employment, trade cycle, and growth theory—multiplier, accelerator, consumption and investment functions, and the like—can here be sampled and critically examined.

While modern economics thus entered postwar Germany early, it certainly did not dominate the field. More traditional lines of thought did

<sup>4</sup> I prefer to translate the German word "Kreislauftheorie" somewhat freely in this way rather than with the more verbal but clumsy "circular flow theory."

<sup>5</sup> Apart from several articles, see his *Wirtschaftlichkeitsrechnung*, Tübingen 1952, and *Industrielles Rechnungswesen*, 2d ed. Tübingen 1954. The latter book had already appeared before the war but was greatly extended in the postwar edition.

not just fade away but continued to exist. In the textbook field one should name: Alexander Mahr's *Introduction* [37] which is based on the Austrian marginal utility school (particularly in the refined shape it obtained after Menger and Böhm-Bawerk in the hands of Friedrich von Wieser and Hans Mayer), with value, prices of final goods, and derived demand ("imputation") for factors of production providing the backbone of the investigation;<sup>6</sup> Hans Ritschl's two-volume work on economic theory [46] with its roots firmly based in the historical school and in metaphysical traditions ("... the individual is ethically autonomous, he is as a body-soul-being an entity, directly related to God,") or Erich Carell's much used textbook on traditional lines [12].

This rather sudden confrontation of different fully established theoretical approaches naturally created some heat, and some clashes of opinion were more passionate and more of the "either-or" type than they could have been in the Anglo-Saxon world in this period. A good example—though in the sharpness of its tone not quite typical—is the essay on Keynes' theory by Hans Mayer (d. 1955) [39]. Mayer attacks all along the whole line. He mixes well-deserved criticism (not all of it new) of some of Keynes' ambiguities and conceptual weaknesses with rather irrelevant quarrels about words and definitions. In some cases he fights more against Keynes' empirical assumptions than against the underlying theory (e.g., do workers aim predominantly at a money or a real wage?); in other cases he dismisses Keynes' contributions (multiplier, etc.) as "old tools." The system as a whole is, therefore, somewhat lost from view. Mayer's main charge, however, is directed against the big aggregates. In the change-over from microeconomics to macroeconomics he sees a return to mercantilism. Thus he regards Keynesian theory—its method, structure, and results—as a retrogressive step.<sup>7</sup>

Mayer's attack and similar views expressed by some other writers (Röpke, Hahn, Adolf Weber) drew a spirited reply from Erich Schneider [54, 89 ff.]. In just over thirty pages he presented a con-

<sup>6</sup> It is, however, significant for the "spirit" of the intervening years that the second, greatly extended edition of Mahr's textbook, *Volkswirtschaftslehre*, Wien 1959, has partly lost its predominantly Austrian character and includes macroeconomic and growth problems.

In current research there have been few attempts to revive the earlier utility and value discussions of the Austrian school. A notable exception is the book by L. Illy-Schönfeld (d. 1952), *Das Gesetz des Grenznutzens*, Wien 1948. See also the two articles by Mahr, "Das Gesetz vom Grenznutzenniveau im Lichte der Kritik" in *Neue Beiträge zur Wirtschaftstheorie, Festschrift anlässlich des 70. Geburtstages von Hans Mayer*, edited by A. Mahr, Wien 1949, and "Indifferenzkurven und Grenznutzenniveau," *Zeitschrift für Nationalökonomie*, 1954, 14, 325 ff., in which the indifference curve approach is sharply attacked. For a critical reply see W. Krelle, "Die 'Neue Österreichische Schule' in der Nationalökonomie," *Jahrbücher für Nationalökonomie und Statistik*, 1953, 165, 454 ff.

<sup>7</sup> *Op. cit.*, p. 50.

densed defense of all the major aspects of Keynes' methods (not necessarily of his conclusions) culminating in a plea to abolish the artificial line between "old" and "new" economics.

One other case of such a full-scale controversy concerning an entire branch of modern economics may be cited. In 1954 Werner Hofmann published his book on national income accounting [27]. The postwar practices of national income calculations and the economic interpretations surrounding them are rejected lock, stock, and barrel. Here again a well-placed scepticism and a critical insight into some of the superficialities of the national income conventions are curiously mixed with inflated trivialities and downright misunderstandings. The rationale of the total procedure never becomes clear and the possibility for a balanced judgment is correspondingly reduced. Here again it did not take long for an extensive and fundamental reply to appear.<sup>8</sup>

While in the heat of these earlier critical attacks some arguments went badly astray, they, nevertheless, fulfilled a certain purpose. They served as an antidote against too uncritical an acceptance of some of the imported ideas—the opposite danger from that of complete negation. Gradually the critical attitude toward the "new" became both informed and less agitated. No longer did one think that one could completely destroy this or that theoretical system by a single pungent article. The treatment became longer; more space was given to exposition and judgment. In this field we may mention the books by Wilhelm Weber and Albrecht Forstmann [61] [18]. The latter book in particular, though no doubt rather hostile to Keynesianism in its basic attitude and not free of serious misinterpretations, presents an impressive array of the international macroeconomic literature up to the date of its publication.

Today, though differences in emphasis still exist, the acute controversies have largely died down. The new economics has been absorbed—at least to some extent—by a growing number of economists. Indeed one could almost say of 1960-German economics what Samuelson had said ten years earlier of American economics: "In recent years 90 per cent of American economists have stopped being Keynesian economists or anti-Keynesian economists" [48]. This is best seen by glancing through the leading theoretical journals of Germany, Austria, and Switzerland. Types of subject and methods of approach differ little from what one is used to finding in Anglo-Saxon journals.

If we take growth theory as an example of one of the most fashionable recent developments in the Keynesian (or macroeconomic) tradi-

<sup>8</sup> G. Bombach, "Volkswirtschaftliche Gesamtrechnungen—eine Weltanschauung?" *Weltwirtschaftliches Archiv*, 1955 II, 75, Schrifttum, 1\*ff. A further exchange between Hofmann and Bombach took place in *Weltwirtschaftliches Archiv*, 1956 II, 77.

tion, Germany certainly stands the test of the time. There is a constant flow of contributions to this field. Erich Preiser tells us that two out of three manuscripts which he receives as coeditor of the oldest German economic journal deal with problems which belong in the sphere of growth theory [45, p. 119]. So far, German theory has not been able to add any fundamental or highly original works to this field, although there have been several interesting improvements in one detail or another.<sup>9</sup> But perhaps the most succinct and the most extensive exposition of growth economics can be found in German literature. In 1953 Gottfried Bombach wrote an informative article on growth theory [9, 110 ff.], which beautifully presents the origin, the essence, the structure, and the limitations of Harrod-Domar-type growth models.<sup>10</sup> At the other end of the scale is Sigurd Klatt's volume on the theory of industrialization [30] where we find—gathered around the theoretical structure provided by Harrod, Domar, Hicks, Colin Clark, and others—an extensive treatment of the incentives to economic growth, of the growth process itself, and of the elements making for stagnation. Here practically no niche of growth theory remains unlighted, no point untouched. The text runs over 430 pages, followed by 90 pages of systematic bibliography and an authors' index of 17 pages containing some 1400 names (my rough estimate). If ever Germany's reputation for thoroughness should be in danger, here—at least for the field of growth economics—is ample proof of its continued existence.

## II. *German Pioneers of Modern Macroeconomics and National Income Analysis*

One consequence of the force with which the voluminous Keynesian literature poured into postwar Germany, Austria, and Switzerland was

<sup>9</sup> As an early example one could name W. G. Hoffmann (Walther Hoffmann, not to be confused with the earlier mentioned Werner Hofmann), "Wachstumsprobleme offener Volkswirtschaften," *Schweizerische Zeitschrift für Volkswirtschaft und Statistik*, 1950, 86, 498 ff., where the special problems of an open economy under growth conditions are stressed. These had been rather neglected in the earlier discussions.

<sup>10</sup> At this point we may mention that longish articles like this (the Bombach article runs over 56 pages), covering the results of recent research for a whole subject, have appeared from time to time in some of the German journals and have to some extent filled the need which in the Anglo-Saxon world has been met after the war by the two *Surveys of Contemporary Economics* and is currently being satisfied by a series of survey articles appearing in the *Economic Journal* and in the *American Economic Review*. In addition to the above mentioned article by Bombach, we have among these comprehensive surveys another contribution from the same author, "Kreislauftheorie und volkswirtschaftliche Gesamtrechnung" (Circular flow analysis and national income accounting), *Jahrbuch für Sozialwissenschaft*, 1960, 11, 217 ff.; further, A. Predöhl, "Neuere Literatur zur Aussenwirtschaft" (Recent literature on international trade), same journal, 1959, 4/10, 80 ff.; K. D. Kuhlo, "Eine Analyse des Vollkostenprinzips" (An analysis of the full cost principle), *Weltwirtschaftliches Archiv*, 1955, II, 75, 137 ff.; and several others.

that important and original German contributions to macroeconomics were somewhat neglected. In fact, the search for a new macroeconomic approach originated in Germany almost thirty years ago. Two pioneers set out to develop macroeconomic *Kreislaufmodelle* (circular flow models): Ferdinand Grünig (d. 1960) and Carl Föhl. It is perhaps no accident that both started their careers as engineers, changing over to economics later in life. Their macroeconomic dynamics are obviously influenced by analogies from technical flow processes. As with Keynes, their interest in the mechanism of the macroeconomic process was roused by the Great Depression of the thirties.

Grünig started to write his book on economic flows—the flow of goods and the flow of money—in 1931; it was published in 1933 [20]. In it he developed a model analysis (by no means usual at that time) which contained all the terms of modern employment theory, though not in the brilliant form it received by Keynes. Here we find already the equation showing the equality between (ex-post) saving and investment, the recognition that purchasing power is a function of the level of production, the idea of the multiplier, and the possibility of underemployment equilibrium. Grünig's book did not only deal with theory; from the few statistical sources available at the time he also constructed national accounts for the German economy for the years 1929 and 1932. His policy conclusions were that in a noninvesting economy—Germany in 1932—the growth of savings is harmful. Public investment should be increased and the building of private homes and agricultural improvements should be fostered. He also tried to work out, with the aid of his model, the full quantitative primary and secondary effects of such actions.

On a far more sophisticated level, in its theoretical standing quite on a par with Keynes' *General Theory* we have Föhl's book on money and the macroeconomic process.<sup>11</sup> From its structure, most Keynesian and several post-Keynesian developments can easily be deduced. Like Keynes, Föhl laid bare the necessary relationships between important macroeconomic aggregates—saving, consumption, investment, production, incomes; like Keynes, he introduced a consumption function and an investment function; like Keynes, he saw the demand for cash (and not the demand for savings) as the regulator of interest rates.

But there are, of course, also significant differences. It is a curious thing that Föhl keeps in his analysis a greater formal continuity with Keynes' *Treatise on Money* than did Keynes in his *General Theory*. Throughout his book, Föhl distinguishes between firms and their fac-

<sup>11</sup> C. Föhl, *Geldschöpfung und Wirtschaftskreislauf*, 2d ed., Berlin, München, and Leipzig 1937. The manuscript had been finished in 1935 and was thus written quite independently of the *General Theory*.

tors of production. Income flow is divided into factor incomes and residual (windfall) profits (depending on the difference between investment and factor savings), and total income becomes equal to consumption expenditure of firms plus consumption expenditure of factors plus savings of factors. This higher degree of disaggregation leads to a more useful investment function which in Föhl's case is not only dependent on the rate of interest but also on profits. Föhl's theory of the rate of interest is less dependent on underemployment conditions than is Keynes': he stresses that under full employment it may have to be brought into line with the state of the labor market. On the other hand, Föhl did not fully develop the multiplier process, and he is on the whole less sure of his subject than Keynes when it comes to questions of the monetary and banking mechanism.

Föhl's exposition is very different from that of Keynes. He is more difficult to read; he does not have Keynes' elegance of style. On the other hand, Föhl made ample use of ingenious circular flow diagrams which forced on him (and the reader) a degree of rigor which was sometimes lacking in Keynes' argumentation.

As we said before, the impact of Grünig's and Föhl's early attempts on German economics was limited and has been partly swamped by the reception of the richer Keynesian and post-Keynesian literature. Originally their influence was hampered because being outsiders and using untraditional forms of analysis they remained for a long time unnoticed by a large part of the profession. But the interest in macroeconomic flow models which they had kindled did not die down, and there has been, in the past twenty-five years, a constant trickle of important contributions. In this context one should mention Hans Peter (d. 1959) who developed interesting static and dynamic mathematical models of the circular flow process [44]. The engineering origins of German circular flow analysis enabled German economists to meet with mathematicians and physicists to discuss the analogies between economic and technical control processes.<sup>12</sup> While there is a danger that analogies of this sort are driven too far and lead to a disregard of the sociological aspects of economic life—already far too much neglected in non-Marxian economics—there is no doubt but that they contain considerable heuristic possibilities.

The full logic of the circular flow thinking has recently been directed to a greater extent to monetary flows. While statisticians in the United States and elsewhere are struggling more or less successfully to provide

<sup>12</sup> *Volkswirtschaftliche Regelungsvorgänge im Vergleich zu Regelungsvorgängen der Technik*, zusammengestellt von H. Geyer und W. Oppelt, München 1957. In the same line lies also the book by W. G. Waffenschmidt, *Wirtschaftsmechanik*, Stuttgart 1956.

the data for a watertight flow-of-funds analysis, there are still many problems of definition, consistency, and operational relevance to be solved. German theorists, it would seem, will take an active part in these discussions.<sup>13</sup>

### III. *Monopoly and Competition*

So far we have spoken of the new economics mainly in terms of Keynesian and post-Keynesian developments. If we stretch the term to include the "monopolistic competition revolution," the situation in German economics is quite different from the one pictured earlier with regard to the Keynesian debate. In this field, no large-scale import of foreign ready-made results was required. From the very start of the new value theory, German writers have produced numerous and original contributions to the discussion. Even before Chamberlin and Joan Robinson had published their treatise did an attempt at a more sophisticated classification of market behavior appear in a German journal [57, 761 ff.]. The attempt had several shortcomings and did not come off. In subsequent years, however, some of the leading German economists entered the market-form debate—Heinrich Stackelberg, Erich Schneider, Walter Eucken.<sup>14</sup> Since then the chain of writings on monopoly and competition has never been broken.<sup>15</sup> While this voluminous literature has touched almost every aspect of monopolistic competition and oligopoly theory, too much weight has probably been given to problems of classification. Like the connoisseur of a large butterfly collection, economists have tried to put their different (market) types into some perfect systematic order. What baroque forms twenty-five years of filigree work have produced can best be judged from the following table which is taken from the book by Ott cited in the previous footnote.<sup>16</sup>

<sup>13</sup> An important contribution is W. Stützel, *Volkswirtschaftliche Saldenmechanik. Ein Beitrag zur Geldtheorie*, Tübingen 1958.

<sup>14</sup> H. Stackelberg, *Marktform und Gleichgewicht*, Berlin 1934; E. Schneider, "Zur Konkurrenz und Preisbildung auf vollkommenen und unvollkommenen und unvollkommenen Märkten," *Weltwirtschaftliches Archiv*, 1938 II, 48, 399 ff.; W. Eucken *Die Grundlagen der Nationalökonomie*, Jena 1940. All these authors have also later contributions to their names.

<sup>15</sup> Here is a random selection of some recent contributions: A. Ott, *Marktformen und Verhaltensweise*, Stuttgart 1959; H. Lampert, "Die Preisfuhrerschaft. Versuch einer zusammenfassenden Darstellung," *Jahrbuch für Nationalökonomie und Statistik*, 1960, 172, 203 ff.; E. Heuss, "Das Oligopol, ein determinierter Markt," *Weltwirtschaftliches Archiv*, 1960, 84, 165 ff. This may be the appropriate place to mention Krelle's very comprehensive book on all conceivable aspects of price theory (W. Krelle, *Preistheorie*, Zürich und Tübingen 1961).

<sup>16</sup> A. Ott, *op. cit.*, p. 16. Ott himself is not just a "classifier." In his book he tried to achieve a synthesis between what he calls the "morphological" (number of sellers, etc.) and the "behavioristic" (elasticities, cross-elasticities, game theory) theories of market forms.

Sellers Buyers	many small ones	many small ones, a few middle ones	a few middle ones	many small ones, one big one	one big one
many small ones	Bilateral polypoly	Partial oligopoly	Oligopoly	Partial monopoly	Monopoly
many small ones, a few middle ones	Partial oligopsony	Bilateral partial oligopoly	Oligopoly limited by partial oligopsony	Partial monopoly limited by partial oligopsony	Monopoly limited by partial oligopsony
a few middle ones	Oligopsony	Oligopsony limited by partial oligopoly	Bilateral oligopoly	Partial monopoly limited by oligopsony	Monopoly limited by oligopsony
many small ones, one big one	Partial monopsony	Partial monopsony limited by partial oligopoly	Partial monopsony limited by oligopoly	Bilateral partial monopoly	Monopoly limited by partial monopsony
one big one	Monopsony	Monopsony limited by partial oligopoly	Monopsony limited by oligopoly	Monopsony limited by partial monopoly	Bilateral monopoly

#### IV. *Traditional Influences and Influential Traditions*

As mentioned earlier, present-day economic research in Germany, Austria, and Switzerland no longer differs greatly from research activities in the Anglo-Saxon world. But while the type of questions asked and the methods used are similar, it would be strange indeed if past traditions did not color to some extent the total *oeuvre* of German economists. What, then, are some of these distinguishing features?

First, we have the influence of the historical school and of a whole list of prominent writers of the past (Marx, Max Weber, Sombart, Oppenheimer, Weiser, Schumpeter) who never rested content with pure economics, but always aimed at a blend of economic and sociological theorizing and—if necessary—ventured pretty far into the neglected fields of sociology and social psychology. Though some of them were regarded as outsiders by the main stream of academic economists they were not without influence. In this respect, German economics resembles somewhat the American situation, where Veblen and institutionalism played a similar (though less important) role; there are fewer points of contact with England where developments of this sort were the exception.

A definite historical school, which tried to draw on all aspects of history and civilization, no longer exists, but the spirit of this school is clearly visible in some writings. Here we could name Edgar Salin<sup>17</sup> who, significantly, has written not only on economics but also on politics, philosophy, and poetry. Also in the historical vein are some of the economic publications of the List-Gesellschaft, which are edited by Salin and Erwin von Beckerath.

More widespread than the survival of a predominantly historical approach is an alertness to sociological factors, a realization that the neglect of *exogenous* circumstances may falsify the *pure* economic picture. Good examples in this direction are Erich Preiser's excellent contributions in the field of distribution theory<sup>18</sup> where he shows the influence of property on the formation of incomes. He stresses that the institution of private property has been neglected vis-à-vis the problem of market forms (monopoly, monopsony), although it has equal or even stronger effects on the demand and supply situation of the factors of production. With this point in view, the whole marginal productivity theory acquires a different and more limited meaning. In this direction, too, are some of Josef Dobretsberger's studies on monetary problems which are written with a strong sociological and institutional slant.<sup>19</sup> In the Schumpeterian tradition there is also no inhibition to cross the border and write a critical analysis on predominantly sociological themes.<sup>20</sup> Again, a comparison of the previously mentioned work by Jöhr on the trade cycle<sup>21</sup> (which draws on German as well as on Anglo-Saxon sources) with an English or American text on the same subject reveals greater stress in Jöhr's volume on extraeconomic factors.

<sup>17</sup> See, for instance, his booklet (based on an article in *Kyklos*, 1955, 8, *Ökonomik der Atomkraft. Vor einer neuen Etappe der industriellen Revolution*, Köln-Marienburg 1955) where he roams over past and present tendencies in the energy sector in order to answer the question (affirmatively) whether the industrial revolution and mankind have entered into a new era.

<sup>18</sup> E. Preiser, "Besitz und Macht in der Distributionstheorie" in *Synopsis. Festgabe für Alfred Weber*, Heidelberg 1948, 333 ff.; *ibid.*, "Erkenntniswert und Grenzen der Grenzproduktivitätstheorie," *Schweizerische Zeitschrift für Volkswirtschaft und Statistik*, 1953, 89, 25 ff. Both essays have been republished in E. Preiser, *Bildung und Verteilung des Volkseinkommens. Gesammelte Aufsätze zur Wirtschaftstheorie und Wirtschaftspolitik*, Göttingen 1957). The first of these two contributions has been translated into English: "Poverty and Power in the Theory in Distribution," *International Economic Papers*, No. 2 (1952).

<sup>19</sup> J. Dobretsberger, *Das Geld im Wandel der Wirtschaft, 14 Studien zu den Währungsfragen der Gegenwart*, Bern 1946. In addition to essays on the origin of money, on money and prices, monetary trade cycle theories, etc., there is also one—again significantly—on the theory of economic blocs and one on imperialism in currency politics.

<sup>20</sup> See the interesting article by Dobretsberger, "Krise der Intelligenz," *Zeitschrift für die gesamte Staatswissenschaft*, 1951, 107, 1 ff.

<sup>21</sup> See above, p. 3.

Another German speciality of bygone days was an excessive interest in methodological questions. While for a long time problems of method probably received too little attention in the pragmatic Anglo-Saxon literature, some German books were in danger of burying their theoretical analysis under a mountain of methodological considerations. The heat which methodological controversies could create was best illustrated by the bitter dispute between the historical and the Austrian school (particularly between Schmoller and Menger) in the 1880's, which belongs to the classics of German economic polemics.

In recent decades, the extreme preoccupation with methodological questions has subsided. Now books on economics can be and are written without lengthy reflections on the appropriateness of the methods chosen. Yet a healthy interest in methodological problems has survived. Occasionally there are still reflections of the not very fruitful *either-or* type (inherited from Sombart's days): whether economics is an *understanding* (verstehende Wissenschaft) or a quantitative science. But on the whole the interest of the methodologists has moved to newer problems, posed by recent developments in the fields of philosophy and economic research. In these writings—which have their counterparts in Anglo-Saxon specialist literature and which owe a good deal to Myrdal's pioneer work—we find a greater awareness of the ideological trappings in much of the received doctrine and an endeavor to lay the methodological basis for a more empirical economic theory. This occasionally links up quite naturally with a demand for an increased consideration of sociological factors in economic theory.<sup>22</sup> With the new forms of economic models gaining in importance, increasing attention has been given to the question of how far the use of mathematics is justified.<sup>23</sup> Full agreement in these discussions is, of course, not to be expected, because of differences in training, tradition, field of study, and the like.

Closely related to the problem of method is the question of (ethical) value judgments in economics which has been haunting German economists ever since Max Weber demanded that economics should be *wertfrei* (at least when economists speak *qua* economists). Though much has been written on this subject in the past, the discussion is still continuing. While most writers probably accept Weber's position that economic theory should be restricted to a detached investigation of

<sup>22</sup> In this direction see the interesting contributions by Hans Albert, in particular his *Ökonomische Ideologie und politische Theorie*, Göttingen 1954, and "Nationalökonomie als Soziologie," *Zur sozialwissenschaftlichen Integrationsproblematik*, *Kyklos*, 1960, *XIII*, 1 ff.

<sup>23</sup> Two recent attempts at an evaluation of the scope and usefulness of mathematical methods are G. Kade, *Die logischen Grundlagen der mathematischen Wirtschaftstheorie als Metho-enproblem der theoretischen Ökonomik*, Berlin 1958, and H. W. Brand, *Über die Fruchtbarkeit mathematischer Verfahren in der Wirtschaftstheorie*, Frankfurt a.M. 1959.

what *is* and not of what *ought to be*,<sup>24</sup> there are others who believe that there are immanent values in the economic process which can be detected by the economist, enabling him to use the adjectives *good* and *bad* in an objective sense.<sup>25</sup> Still others stress that present-day economics is so loaded with hidden valuation problems that scientific objectivity will be best served, not by a complete prohibition of value judgments, but rather by a conscious and critical exposition of the chosen value basis by the economist [6, 26 ff.].

Whatever the economists' attitudes (if any!) to this problem may be in principle, in actual practice they tend (almost necessarily, I believe) to pack value judgments into and between their lines—today as much, if not more, than ever. This is most prominent in the way problems are chosen or left out of consideration, but direct value pronouncements are also not infrequent, and this tendency has expanded along with the general cold war atmosphere. Free markets, economic integration, and other targets of official economic policy are often uncritically accepted (explicitly or implicitly) as higher aims, desirable in themselves and beneficial to *all* classes of society. It may be fairly easy to find angry young men in German belles-lettres; among economists they are very rare indeed. But this is probably not a specific note of German economics.

Since the turn of the century German economics has known a rather sharp division between *Volkswirtschaftslehre* and *Betriebswirtschaftslehre* (political economy and business economics, respectively). The great expansion of theoretical economics (teaching and research) after World War II has led to a similar growth in the sector of *Betriebswirtschaft*. But the difference between the two disciplines, always rather thin at the edges, is becoming more and more blurred. On the one hand linear programming, input-output analysis, and similar developments in economic theory have pushed *Volkswirtschaftslehre* nearer to production problems of individual industries and plants; on the other hand, some *Betriebswirtschaftler* (business economists) are expanding their theoretical apparatus, assimilating it with the models used in *Volkswirtschaftslehre*. This leads to books which cannot be easily placed in one category or the other, as for instance the excellent book on production and selling by the *Betriebswirtschaftler* Gutenberg [21], which could

<sup>24</sup> For a full discussion of the evolution of this view see Wilhelm Weber and E. Topitsch, "Das Wertfreiheitsproblem seit Max Weber," *Zeitschrift für Nationalökonomie*, 1952, 13, 158 ff. It is realized that a valuation process creeps in: (a) in setting the aim of a "value-free" economics, and (b) in the choice of subjects. The introduction into the analysis of valuations by other persons *as data* is, of course, not objected to.

<sup>25</sup> This view, which in the interwar years had been most consistently pressed by the German economist Gottl-Ottlienfeld, is for instance taken by T. Pütz, *Theorie der allgemeinen Wirtschaftspolitik und Wirtschaftslenkung*, Wien 1948, Chs. I, II, and IV.

equally well serve as a first-class text on the theory of the firm in a course on economic theory.

I want to conclude this section with the one specific German aspect in economics which is not so much a remnant of long-standing traditions and influences, but rather the outcome of recent growth. I refer to the school of neoliberalism (ORDO-liberalism, *Theorie der sozialen Marktwirtschaft*—theory of the social market economy). Some readers may be surprised that this branch, which is perhaps the best-known speciality in German postwar economics, has not been mentioned earlier and in a more prominent place. I shall account for this presently.

German neoliberalism has its roots in the *Freiburger Schule*, a group of liberal economists in the University of Freiburg who started to discuss the problem of *modernizing* liberalism a quarter of a century ago. The nucleus of this group soon became Walter Eucken (d. 1950), who developed his concept of a new liberal order in various writings in the last ten years of his life.<sup>26</sup> Typical of his thought was the belief in a fairly rigid relationship between the different market forms and a corresponding social and political setting. The choice of a *proper* market order becomes, therefore, the crucial problem for mankind. Eucken, who believed that this choice can be made comparatively freely (irrespective of the historical, technical, and economic stage of society and the relative strength of different classes), developed—in the words of one of his followers—"a simple, complete, understandable and crystal-clear pattern of possibilities for the shaping and regulation (*Gestaltungs- und Ordnungsmöglichkeiten*) of the economic system."<sup>27</sup> The pattern which the Eucken circle (the neoliberals or ORDO-liberals) has chosen—for economic, political, ideological, and philosophical reasons—is a revised liberalism. Gone is the belief in the automatic working of *laissez-faire*, but a hostility against monopolies and state intervention has remained. The economic concept of the neoliberals envisages a minimum of state action necessary to preserve free markets, competition, and small business, and to prevent the social excesses of pure *laissez-faire*.

Whether the reader is enchanted by this program or whether he rejects it, he will understand why so little room has been spared for neoliberalism in this essay on the main trends in current German economic

<sup>26</sup> See W. Eucken, *Die Grundlagen der Nationalökonomie*, Jena 1940; idem, "Das ordnungspolitische Problem" in *ORDO, Jahrbuch für die Ordnung von Wirtschaft und Gesellschaft*, 1948, I. Also, F. Böhm, "Die Idee des ORDO im Denken Walter Euckens," *ORDO-Jahrbuch*, 1950, III; E. W. Dürr, *Wesen und Ziele des Ordoliberalismus*, Winterthur, 1954; H. M. Oliver, Jr., "German Neoliberalism," *Quart. Jour. Econ.*, 1960, 74, 117 ff.

<sup>27</sup> F. Böhm, *op. cit.*, p. XLI.

*theory*. A detailed study of neoliberalism belongs in the field of German ideologies. It would then be seen that before 1945 some of the neoliberal writings represented a courageous independence vis-à-vis the basic fascist economic tenets, but that more recently they have turned mainly into a kind of apologetics for the present-day variety of Western German capitalism combined with a *petit-bourgeois* aversion against the advance of the big monopolies.

By saying that neoliberalism as a "school" offers more of interest to the chronicler of economic ideologies than to the student of economic theories, I do not want to suggest that members of this group have contributed nothing to the theoretical discussion. This is by no means true. If one goes through the pages of the ORDO yearbooks (an annual publication founded by the neoliberal group in 1948 as their main platform) one finds many interesting articles dealing with various aspects of economic theory. But in so far as they are of *general* theoretical interest they could have been written by other economists as well. It would be difficult to detect in them a special neoliberal stamp.<sup>28</sup>

The ORDO circle stands out because it is the only group of economists which continuously and massively presses for a certain (though vaguely defined) economic policy. But its presence has given a particular fillip to the discussion of problems of the "proper" economic order (*Wirtschaftsordnung*). The number of publications touching issues of this type is very large; they range from short essays to voluminous treatises. All sorts of views are represented, like the catholic standpoint of Oswald von Nell-Breuning<sup>29</sup> who takes his inspiration from the medieval ideas of an ordered hierarchy of the body politic and from the papal encyclicals or full employment ideologies of the current Anglo-Saxon type,<sup>30</sup> or again social-democratic and similar reformist proposals of a mildly interventionist character.<sup>31</sup>

<sup>28</sup> It may be significant that the specifically neoliberal themes in the ORDO yearbooks have declined in number as the years passed by. In the first volume (1948) eight out of the nine main contributions dealt quite clearly with neoliberal themes (economic order, monopoly, individualism, etc.). Of the twelve contributions in Vol. XI (1959) practically none could be termed as typically or exclusively neoliberal (the one exception perhaps being some critical notes on economic integration by Wilhelm Röpke).

<sup>29</sup> See, for instance, his *Wirtschaft und Gesellschaft, Band I: Grundfragen*, Freiburg 1956.

<sup>30</sup> For instance, E. Preiser, *Die Zukunft unserer Wirtschaftsordnung*, 2d ed. Göttingen 1955.

<sup>31</sup> Here we may mention K. Schiller, *Sozialismus und Wettbewerb*, Hamburg 1955, and the voluminous treatise by H. Bayer, *Wirtschaftsgestaltung*, Berlin 1958, which views the economic process as a multitude of tensions and looks for an economic order that would regulate and ease the tensions. The views of various writers on problems of economic order can be found in E. Lagler and J. Messner, eds., *Wirtschaftliche Entwicklung und soziale Ordnung*, Wien 1952.

### V. *Public Finance*

The theory of public finance and taxation has always occupied a rather special place in Central European economic science, far more so than in Anglo-Saxon literature. There it has usually been part of the general economic reasoning. Even Keynesianism and macroeconomic employment theory, which so enormously stimulated interest in fiscal action, did not lead to any clear separation of public finance theory from the main body of economic science. On the contrary, the new macroeconomic thinking and the interest in policy questions promoted a fuller integration of public finance problems into the basic theoretical framework.

On the continent, *Finanzwissenschaft* has a special standing of its own, almost on a par with the general subject of economics (*Volkswirtschaftslehre*). This tradition has its roots in the 17th and 18th centuries in the Cameralist writings which were greatly concerned with problems of public finance and taxation and had their main home in Italy, Germany, and Austria.<sup>82</sup> In the interwar years the neoclassical theory of public finance had a particularly strong basis in Italy. In fact, one of the best available English textbooks on the subject in those days was a translation from the Italian: Antonio de Viti de Marco's *First Principles of Public Finance*. But in German literature, too, public finance kept an important place. Since 1945, the interest in fiscal problems has been greatly stimulated by the Keynesian debates, and some of the most important contributions and liveliest debates have occurred in this field.

The main directions in which the diversified postwar literature on public finance has developed may be characterized by indicating the basic approach adopted in the works of three outstanding representatives of this branch of economics. Shortly after the war there appeared the fundamental treatise on income taxes by Fritz Neumark (then still an *émigré* at the University of Istanbul) [41]. He set a very high standard for the thorough investigation of all aspects of the subject concerned. There is no playing around with simplified models of the economy as a whole. The study has its roots firmly fixed in the essentials and the structure of the income tax itself. All possible characteristics and forms of an income tax are duly catalogued and discussed, and the door to reality is opened widely by giving extensive accounts of income-tax practice in seven countries. Its thorough and concrete approach and the wide sweep of its verbal analysis mark this work (and later contributions in a similar vein) as a continuation of the best classical tra-

<sup>82</sup> See A. W. Small, *The Cameralists*, Chicago 1909; J. A. Schumpeter, *History of Economic Analysis*, New York 1954, pp. 159-206.

ditions carried on at a higher level of specialization. Yet it would be a mistake to see only the traditional side of this work. In the chapters on shifting and on cyclical fluctuations, F. Neumark delves deeply into the problems introduced by the new economics. In this respect, his book carried very distinctly the stamp of the modern age and is clearly separated from some contemporary works which consist mainly of a high refinement of the prewar approach.<sup>33</sup> Yet it remains characteristic that these modern themes are modestly embedded in the descriptive and classificatory structure of the work.

At the opposite end of the spectrum stands Heinz Haller's *Finanzpolitik* [22]. Here the Anglo-Saxon reader well versed in Keynesian and post-Keynesian literature would feel at home right from the start. The basis of the book lies in its extensively developed macroeconomic models (illustrated by numerous diagrams) of income and employment with attention concentrated on the government sector. The models serve to tackle the main problems of the book: budgetary policies toward a stable income, and the shifting and incidence of taxation. In the course of these discussions, many of the newer theoretical problems connected with employment policy come up for close inspection. Thus Haller's treatment of the balanced-budget multiplier<sup>34</sup> represents an interesting investigation of the various secondary effects of a (balanced) budget increase showing that an expansionary effect will only occur under certain circumstances.

Macroeconomic relationships under the influence of a tax system are Haller's main interest. The various taxes with their special aspects and peculiarities are of lesser importance to him. They receive some attention but mainly in combination with the question of the overall efficiency of the tax system. Similarly, the institutional framework is only roughly incorporated in the premises of the models; only in the final chapter does it receive some detailed treatment. In general, Haller's approach is that of the "economic engineer" (familiar from many works in Anglo-Saxon literature) whose theoretical models serve to show the complex consequences of fiscal action, and thus, it is hoped, will aid and improve fiscal policy. *What* this fiscal policy is likely to be in actual practice is largely determined by factors outside the chosen boundaries.

It is here that the third approach appears, which finds its fullest expression in *Finanzpolitik* by Günter Schmolders [50]. Tax policy and fiscal action are traced back to their psychological, sociological, and

<sup>33</sup> See, for instance, W. Gerloff, *Öffentliche Finanzwirtschaft*, 2d ed. Frankfurt a.M. 1948, I, 1950, II; A. Amonn, *Grundsätze der Finanzwissenschaft*, Bern 1947.

<sup>34</sup> *Op. cit.*, pp. 72 ff.

institutional origins. It is not so much the question of what economic consequences will follow from a certain fiscal measure that occupies Schmölders' mind. This question is treated, but often in a rather vague manner.<sup>85</sup> The main trend of the analysis is directed toward a discovery of the pulls and stresses from which the aims for fiscal policy emanate and which delimit the space for fiscal action. While Haller wants to show how a desired aim can be achieved or what consequences will flow from a certain action, Schmölders wants to find out what will be desired and what is practically possible in a given institutional setup. This is an important and legitimate theoretical question. In the hands of Schmölders, it suffers from insufficient insight into the driving forces of capitalist society. Thus, in spite of many interesting details and important additions to our knowledge, Schmölders' work does not reach that depth of understanding to which his kind of approach should ultimately lead.

Neumark, Haller, and Schmölders were chosen as significant representatives of the different types of economic thinking in the field of public finance. With regard to both content and persons, the discussion naturally ranges over a far wider field. A good idea of the variety of research can be gained by looking at the new (second) edition of the three-volume encyclopedia of public finance [63] or at the issues of the *Finanzarchiv*, an economic journal especially dedicated to problems of public finance.

It was in the pages of this journal that one of the liveliest theoretical debates of the postwar period took place. Prominent German-speaking economists on both sides of the Atlantic engaged in the discussion which extended over two years: Carl Föhl, Hans Peter, Hans-Joachim Rüstow, Rudolf Stucken, Werner Ehrlicher, Gerhard Colm, Hans Neisser, Wilhelm Krelle, Heinz Haller, Karl Häuser, Rudolf Richter, and Horst C. Recktenwald. It was opened by a pointed and provocatively formulated article by Föhl [16, 88 ff.], in which he argued that any hope of improving the income distribution with the aid of a progressive profit tax must be disappointing. A profit tax would be completely shifted to nonentrepreneurs; gross profits would rise to such an extent as to leave net profits unimpaired. This conclusion was derived on the basis of an instantaneous (i.e., lagless) circular flow model of the sort which Föhl had previously introduced in his major work on macroeconomics.<sup>86</sup>

Reduced to its barest essentials his argument runs as follows. Starting with a fully employed economy and assuming that production remains at that level, we can divide the income flow into (contractual)

<sup>85</sup> A relevant section is significantly subtitled: "In the maze of tax consequences."

<sup>86</sup> See footnote 11, p. 9.

factor incomes and entrepreneurial incomes. Neglecting for the moment factor savings, the factor incomes in an uninterrupted flow model will equal factor consumption, and the entrepreneurial incomes (profits) will equal investment and entrepreneurial consumption. Next we assume that a tax on profits is introduced and that the proceeds of these taxes are immediately spent on goods and services or passed on to government employees, old-age pensioners, and others, who in turn buy consumption goods. The money taxed away from the entrepreneurs would, therefore, return to them in the form of an additional demand of nonentrepreneurs for consumption goods. The entrepreneurs would, under these circumstances, maintain their consumption and investment expenditure. Total demand and gross profits would rise by exactly the amount of the profit tax. Since production is unchanged, prices would go up and this would curtail the consumption of the factors to an extent that is needed to free the resources required by the state. The tax has been completely shifted. The argument is not altered when we allow for factor savings, as long as the savings remain unchanged after the imposition of the tax.

Brought down to a still more simplified level we can visualize the Föhl model as an economy of consuming factors and investing entrepreneurs. The state wants to raise funds for consumption purposes. With total product and investment given, the burden of the state expenditure must ultimately fall on the consuming factors, no matter where the taxes are initially placed.

It is perhaps not surprising that the article with its sharp formulations drew forth a whole chorus of critical replies. Many of the critics shared Föhl's skepticism about the effectiveness of progressive taxation for redistributive policies, but all of them attacked his complete negation of any effect whatever.

The first replies turned against isolated shortcomings in Föhl's method or premises. Rüstow [47, 360 ff.] replaced Föhl's instantaneous flow model by a lagged approach which—if undisturbed—would in the end yield the same result as Föhl's analysis. (The parallel to the instantaneous and lagged multiplier is evident.) But *during* the period of expansion various adjustments may retard the process. The profit tax will influence the investment and consumption decisions of employers, and the rising price level will lead to counteractions by the central bank and the government. The final result will, therefore, differ somewhat from Föhl's solution. In particular, a redistribution of incomes among *entrepreneurs* is to be expected. Stucken and Ehrlicher [58, 367 ff.] stress the fact that the taxed profits will not immediately enter the market. Even if the state spends the proceeds without delay, the beneficiaries (pensioners, government employees, etc.) will spread

their consumption over a period. With a given quantity of money (and unchanged velocity of circulation in the rest of the economy), the increase in prices and turnover which would fully restore the pretax profits will not materialize. Investment of capitalist consumption will be reduced unless an adequate credit expansion—tacitly included in Föhl's model—opens the door for the required price increases. Further factors mentioned by the two authors are: changes in stocks leading to (involuntary) changes in investment, complications stemming from the uneven distribution of the changed expenditure structure, and the attempt of wage earners to maintain their real wages and the consequent necessity of the monetary authorities to intervene in the resulting price-wage spiral. Several modifications to the Föhl argument are proposed by Colm [13, 525 ff.] who regards the analysis as too general and built too much on premises derived from the situation in postwar Germany. Among his more important points are the assertion that in an underdeveloped or growing economy a profit tax may help to bring the (individual and corporate) saving intentions into line with changing investment decisions; a criticism of the division of income earners into an investing upper class and a consuming lower class as being too simplified; a reminder that progressive taxation introduced in war and emergency periods was designed to eat into investment as well as into consumption expenditures. Finally he points out that accepted standards of fairness and equity will limit the administrative feasibility of tax reforms.

The next group of participants in the debate went right down to a dissection of Föhl's model and analytical method, in order to find out where the "trouble" originates. Neisser [40, 687 ff.] lays particular stress on structural shifts in production from investment to consumption. These follow after the imposition of the profit tax which reduces the immediate demand for investment goods while the profitability of the consumption goods industries grows with the consumption expenditure of the additionally employed government staff. Föhl's model is charged with being too rigidly divided into two productive compartments (consumption and investment) and for not paying sufficient attention to secondary behavioral reactions. A thorough dissection of the Föhl model is contributed by Krelle [33, 22 ff.] who shows that Föhl's results—the full and unavoidable shifting of a profit tax—follow quite necessarily from his *assumption* (hidden away in his flow diagrams and matrices) of unchanged investment and capitalist consumption. But this is only one of several possible cases. The practical outcome will depend on price and investment reactions after the tax change. The reaction functions must be obtained from factual observation and this is lacking in Föhl's study. Krelle contributes a mathematical model

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lished in advance of the 1960 Congress which dealt with concentration tendencies in the modern economy [3].

Among the theoretical fields which obtained a fruitful stimulus from the activities of the *Verein für Socialpolitik* the problem of income distribution stands out with particular force. This was the subject of the 1956 Congress [26] and of special working parties [51]. In this series some interesting and important contributions have appeared, like Hans Peter's thoughtful essay on the limitations of purely economic distribution theories; Wilhelm Krelle's attempt at a macroeconomic distribution theory;<sup>39</sup> Jürg Niehans' extensive formal analysis of the influence of wage changes, technical progress, taxation, and saving on prices, production, and income distribution; and Gottfried Bombach's comparison of the basic determinants in different distribution theories.<sup>40</sup>

There are other professional societies in Germany, Austria, and Switzerland but none of them plays a role comparable to that of the *Verein für Socialpolitik*. Reference should be made, however, to the *List-Gesellschaft* (like the *Verein für Socialpolitik* recreated after the war) because it has acted and still acts as a stimulus to the investigation of problems connected with economic integration. Under its auspices appeared the only thorough *theoretically* oriented study on economic integration [49] (there are any number of publications dealing with *applied* aspects of integration). The study builds an impressive structure on the theoretical elements provided by J. Meade, J. Viner, and J. Tinbergen. It is indeed surprising that the *theory* of customs unions has been mainly developed in the United States and Britain, while the practical steps were taken on the continent.<sup>41</sup>

<sup>39</sup> Starting from the identity  $B/Y = B/K \cdot K/Y$ , where  $B$  stands for property income,  $Y$  for national income and  $K$  for the private capital stock, the share of property income in total income is seen as the product of the "real" rate of interest ( $B/K$ ) and the capital-output ratio. Krelle then proceeds to discover some of the cyclical and long-run tendencies of these two components.

A macroeconomic distribution theory on lines similar to Kaldor's "Keynesian" theory ("Alternative Theories of Distribution," *Rev. Econ. Stud.*, 1955-56, 23, has been published by E. Schneider in *L'Industria* (No. 2/1957); "Einkommen und Einkommensverteilung in der makroökonomischen Theorie" (English translation "Income and Income Distribution in Macro-economic Theory" in *International Economic Papers*, No. 8, 1958). Preiser's contributions to distribution theory have been mentioned in a different context (see above, p. 13).

<sup>40</sup> The article by Peter and Krelle are contained (besides others) in W. G. Hoffmann, ed., *op. cit.*, 42 ff. and 55 ff.; the articles by Niehans and Bombach in E. Schneider, ed., *Einkommensverteilung und technischer Fortschritt*, 9 ff. and 95 ff.

<sup>41</sup> Not that international trade problems have been altogether neglected in German economics, but there is no special theoretical treatment of the problems connected with integration. In the general field of international economics we find useful textbooks like A. Predöhl's theoretical and historical *Aussenwirtschaft. Weltwirtschaft, Handelspolitik und Währungspolitik*, Göttingen 1949, or E. Küng's monumental work on the balance of payments (*Zahlungsbilanspolitik*, Zürich-Tübingen 1959)—with its 900 pages of truly Ameri-

Institutes of economic research have particularly flowered in Germany. Their origin goes back to the twenties when the first were founded in Berlin and Vienna as institutes for cyclical research to study the *Konjunkturlage* (cyclical situation). Since then they have greatly expanded both in personnel and in the fields covered by their research. They differ from the National Bureau of Economic Research in New York and the National Institute of Economic and Social Research in London in their greater concentration on comparatively short-term and topical research projects.<sup>43</sup> They must also be distinguished from university institutes (which exist in large numbers), since they are independent of the universities and work with a staff of their own. There are four big institutes of this sort in Germany (Berlin, München, Essen, Kiel) and one in Austria (Vienna). In Switzerland, they are attached to universities, one to the Technical University (*Eidgenössische Technische Hochschule*) in Zürich, and one—specializing on foreign trade problems—to the Commercial University (*Handels-Hochschule*) in St. Gallen.

The existence of these special institutes has meant that much of the applied research which in Anglo-Saxon countries comes out of the universities and appears in the ordinary journals is, in the German-speaking countries, to be found in the publications of the research institutes. A considerable amount of valuable material has appeared in this way. The IFO-Institute in Munich has developed the *Konjunkturtest* [38], a method of obtaining quickly (with the aid of questionnaires sent out to firms) nonquantitative short-term indicators of the direction of economic changes, and has experimented in various ways to squeeze quantitative estimates and other information out of the rough-and-ready answers supplied by business [2, 159 ff.]. It was in the publications of the Berlin Institute where Albert Wissler (d. 1957) ran his cute commentary on the economic situation and where Ferdinand Grunig<sup>43</sup> developed *quarterly* national income accounts (the first in Europe). Here we find also the important pioneer work by Rolf Kren-

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can textbook dimensions. Among original contributions to detailed aspects of international trade theory the articles by Herbert Giersch deserve particular notice. See his "Akzelerationsprinzip und Importneigung," *Weltwirtschaftliches Archiv*, 1953 I, 70, 241 ff., and "Das Handelsoptimum," *Weltwirtschaftliches Archiv*, 1956 I, 76, 1 ff. The articles have been translated into English: "The Acceleration Principle and the Propensity of Import" appeared in *International Economic Papers* No. 4 (1954), "The Trade Optimum" in *International Economic Papers* No. 7 (1957).

<sup>43</sup> With the publication of its *National Institute Economic Review* since January 1959, the London Institute has entered the sphere of activities typical for the German and Austrian institutes.

<sup>44</sup> See above, p. 9.

gel on capital growth, capital-output ratios, etc.<sup>44</sup> The work carried on in the Institute in Vienna has done a great deal to lay bare the mechanism of the economic process and to explain the macroeconomic quantitative interrelationships in the Austrian economy.<sup>45</sup> The various new tendencies in European and world trade are regularly analyzed by the Institutes in Kiel and St. Gallen.<sup>46</sup>

These few indications of the work done by the institutes were only given by way of example. They should suffice to give an idea of the part the institutes play in the research activity of the German-speaking countries. More recently the amount of quantitative research carried out within the universities has grown considerably.<sup>47</sup> This is a healthy sign that an unwelcome tradition of the past—a certain disdain to empirical research in academic circles—is rapidly breaking down. Of course, with the turn toward more realistic research a host of new problems arises: availability and reliability of data, relevance of the models chosen, questions of testing, and the like. But it would be unwarranted to go into these sorrows in this article. They represent by no means a special German headache. It is shared by economists all over the world.

### VII. *Concluding Remarks*

The preceding pages, in spite of their incompleteness, have shown—so it is hoped—that over the past fifteen years German economics has moved to a considerable extent toward current Anglo-Saxon trends without losing some distinctive marks of its own. Similar tendencies will probably be recognizable in other “national” theories. How could it be otherwise in a world where language barriers are rapidly breaking down with the increasing knowledge of English and with the growing number of translations; and where personal contacts are facilitated (and sometimes over-facilitated) by the rising number of congresses visited and miles traveled?

<sup>44</sup> See R. Kregel, “Die langfristige Entwicklung der Brutto-Anlage-Investitionen der westdeutschen Industrie von 1924 bis 1955-56,” *Vierteljahrshefte zur Wirtschaftsforschung*, Nr. 2, 1957, 168 ff. and articles in later issues of the same publication.

<sup>45</sup> See, for instance, Josef Steindl’s *Wie wirkt die Ausgabe einer zusätzlichen Milliarden Schilling?* (Sonderheft Nr. 9 des Österreichischen Instituts für Wirtschaftsforschung, Wien 1956) where an attempt is made to follow through the full quantitative effects on the economy of a given additional expenditure.

<sup>46</sup> See the regular commentaries by Hans Bachmann and Alfred Bosshardt in *Aussenwirtschaft* and the special issues of this journal dealing with the problems of European economic integration.

<sup>47</sup> Here are some recent titles: W. G. Waffenschmidt, *Einkommenserhöhung in der deutschen Volkswirtschaft*, Stuttgart 1959; W. G. Waffenschmidt and others, *Deutsche Volkswirtschaftliche Gesamtrechnung und ihre Lenkungsmodelle 1949-1955*, Stuttgart 1959; W. Krelle, *Volkswirtschaftliche Gesamtrechnung einschliesslich input-output Analyse mit Zahlen für die Bundesrepublik Deutschland*, Berlin 1959.

How will it go on? The supreme test for an economic theorist is to try his hand at forecasting future trends. Is the expositor of current economic doctrines obliged to subject himself to a similar test? I doubt it. However, the query about further developments remains a legitimate one and demands at least a tentative reply. The question is whether or not German economics is likely to move still further toward the trends which are visible in American and British economics and lose any special traits it possesses. We may question further whether or not such a development would be desirable.

Let us take the second question first. One could very well ask what point there can be in maintaining national or traditional differences in the approach to a common subject. After all, there can be only one "truth" and the quicker all theoretical approaches converge toward the "best" method, the better. This view could be accepted easily if the subject matter of economics were comparatively straightforward and uncomplicated. However, the very opposite is the case. Like most social sciences, economics throws up a multitude of questions, each of which has a large number of different and changing aspects. The choice of relevant questions and aspects is by no means clearly dictated by the subject itself. The choice will be influenced by the economic and political situation, by the predilections and ideology of the theorist, and by the analytical methods and traditions with which he has been nurtured. Different choices will lead to different approaches, each of which may spotlight a different aspect of a complex problem. Therefore, a certain degree of variation in methods, traditions, and starting points may be an asset rather than a disadvantage.

Between these different approaches, competition and cooperation can and does exist *within* each country and does not necessarily require national or language barriers to survive. The fact remains, however, that within a given country, with common language, traditions, educational institutions, and so on, one or a few "schools" will usually sweep the scene at the cost of others. If we remember that economics is not free from swings of fashion (in the choice of subjects and methods) the danger of too much uniformity is obvious. Seen from this angle, the preservation of some differences in the approach to economics in the international sphere is perhaps not quite undesirable. To be really fruitful, such differentiation must be supplemented by constant mutual contact and a lively exchange of notes. In this respect, much remains to be done. Until now the stream of information has been moving rather unevenly: fairly quickly from Anglo-Saxon sources to the rest of the world, but only hesitatingly and slowly in the other direction.<sup>48</sup>

<sup>48</sup> A valuable contribution in this field is made by *International Economic Papers*, the journal of translations published by the International Economic Association.

It would be a good thing if this stream of information could be widened and the "balance of information" improved.

Having decided that some national or traditional differentiation in approach may be an advantage, let us return to the first question. Is it likely that German, Swiss, and Austrian economics will continue to follow a slightly different path from that of Anglo-Saxon economics? There can be little doubt that a number of trends will become very similar. This tendency will be the natural consequence of growing specialization in economics. It will necessitate closer contacts and a common language with colleagues across the frontier—with whom, however, communication will be much easier than with fellow-economists from a different branch in one's own country. Apart from this internationalization of highly specialized branches of economic theory, we can expect some special characteristics to continue, at least for some time to come.

The traditional nexus between economics, law, and politics in a single faculty still exists in many universities in Western Germany, Austria, and Switzerland, or has only recently been loosened. This means that "literary" economics will continue to play a more important role than in the United States; the "mathematization" of the theoretical language will be slower and less universal. Not only will the method of analysis be influenced but also the aspects which are analyzed. The contact with law subjects promotes a consideration of institutional and historical influences.<sup>49</sup> Econometrics, with its stress on quantitative and purely economic factors—though also advancing, is less likely to conquer the scene to the extent it has in the Anglo-Saxon countries.

A similar effect emanates from the secondary school system which is still based on the ideal of general education and does not permit much specialization at the pre-university level. Therefore, the student, even if inclined toward mathematics and the exact sciences, has at least a nodding acquaintance with the humanities. This knowledge will leave its imprint on the work of the economics student—at least sometimes. Psychology, philosophy, "cultural background," and their influence on *homo oeconomicus* will come more quickly to his mind than to the mind of the more specialized college boy. All this will be strengthened by the perseverance of past traditions. In addition, in Austria and in Switzerland (as in other small nations) the smallness of the country acts as a brake on too extensive specialization and may promote a greater awareness of the interdependencies of the specialized branches of economic theory—provided their output can still be comprehended by the non-specialist.

<sup>49</sup> This provides affinities to the American institutionalists. Their survival is, however, probably less secure than that of their German counterparts.

At its best, therefore, German economics may be able to contribute a comparatively large proportion of economic literature that avoids the dangers of over-specialization and extreme mathematical sophistication. At its worst, this line of approach may mean that what is gained in breadth and generality is being lost in accuracy and reliability.

These tentative prognostications must be understood as broad generalizations pointing toward *relative weights* in the large body of research taking place in the Anglo-Saxon and German-speaking countries. For each type of theoretical study in one region it should not be difficult to find a spiritually and methodologically related work in the other area. However, when we compare the theoretical outputs as a whole, we should expect that their structure will not completely match for a considerable time to come.

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# SOME DEVELOPMENTS OF ECONOMIC THOUGHT IN THE NETHERLANDS

By H. THEIL\*

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Contrary to several other small European countries, such as Sweden and Austria, the Netherlands cannot claim to have a great tradition in economic theory which goes back during many decades. Originally, economics was a minor field that could be studied in the departments of law of the universities; the first change in this respect took place in 1913 when the Netherlands School of Economics was established at Rotterdam. In the next decade this move was followed in two other places, leading to the Department of Economics of the (Municipal) University of Amsterdam and the Catholic School of Economics at Tilburg. After the second World War two other universities established

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economics departments, the State University of Groningen and the Free (Calvinist) University of Amsterdam.

The structure of these institutions differs from that of their American counterparts in several respects. For one thing, as in many other continental countries, economic theory and business administration have a nearly symmetric position in the teaching system, and it is only in the last ten years or so that graduate students have been permitted to specialize in either of these fields. For another, only a small minority of the graduate students have a university career in mind, so that the number of Ph.D. theses is also small compared with the total number of graduates. The situation is changing now due to the increasing size of university staffs and hence the better possibilities for a university career, but as a whole this development is still in the infant stage.

The situation just described is, of course, no stimulus for large-scale research activities. We must add to this the language barrier. Until the mid-thirties the *Germania docet* still did its work, a considerable part of the reading lists consisting of German textbooks. This situation changed gradually under Keynesian and political influences, then there was the interruption of the war, but at the moment the Anglo-Saxon influence is clearly dominating. This is well illustrated by the fact that J. G. Koopmans' 1933 essay [20] on neutral money was published in German, whereas later monetary discussions by Holtrop, Tinbergen, and Witteveen were summarized by H. C. Bos [11] in English. I should also mention J. Pen's Ph.D. thesis (in Dutch) on collective bargaining [31], followed by his article in English [30] on the same subject.

There is an important exception to the over-all picture, which is largely due to the work of one man—J. Tinbergen. In the 1930's he became head of a division of the Central Bureau of Statistics, where he and his collaborators carried out a large number of studies in econometrics, including the first econometric model that was ever constructed for the Netherlands as a whole. This work was continued after the war, partly in the Central Bureau of Statistics but mainly in the Central Planning Bureau of which Tinbergen was the director from 1945 until 1955; after 1956 a new stream of econometric investigations came from the Econometric Institute of the Netherlands School of Economics. A rather large proportion of the research in economics in the Netherlands as a whole has, therefore, a mathematical character. Another interesting feature is that a relatively sizable proportion of the research is carried out by workers who cooperate in an institute or a bureau and who continue to be staff members of such institutions during a relatively long period of time. The mobility of labor is much less than in the United States, partly no doubt because the country is so small.

In this article, an attempt is made to survey four more or less related research activities. They are all mathematical to some degree, which means that the picture is somewhat one-sided; but the choice has the advantage of coherence. Section I. deals with the empirical side of model building; Section II. with some methodological aspects; Section III. with some studies in demand analysis; Section IV. with the use of models for decision purposes.

### I. *Empirical Results in Model Building: The Example of Import Demand*

The Central Planning Bureau's econometric model serves the dual purpose of providing forecasts for next year's values of endogenous variables, given certain assumptions on the exogenous variables, and of estimating the effects of alternative measures of government economic policy. Actually, it is not entirely correct to speak about "the" model, because the procedure followed amounted to changing its structure in the light of evidence and changing theoretical convictions. The first of this series of models was constructed in the early fifties, its main ingredients being intuition, courage, and least squares. Sophistication came later. At the moment a research project is under way to make a better model. Some of the earlier results have been reported by P. J. Verdoorn and C. J. Van Eijk at the Bilbao Meeting of the Econometric Society [49].

Although the general flavor of these models is not different from that of similar models constructed by L. R. Klein for the United States, there are special features which are due to the fact that the Netherlands is a small country and, consequently, foreign trade is of great relative importance. Hence imports and exports deserve careful attention; the same is true for wages and prices, for the relative price level compared with that of competitors abroad is one of the determinants of the components of foreign trade. It may, therefore, be of interest to pay some attention to the import demand equation as it was estimated recently by Verdoorn.<sup>1</sup>

Let us write  $m$  for the annual percentage change in the volume of imports of goods (services being excluded). Then the equation runs as follows:

$$\begin{aligned}
 (1) \quad m = & 1.24v_m + 0.29\Delta v_m + 2.50J - 0.38p_1 + 0.32\Delta p_2 \\
 & (0.25) \quad (0.15) \qquad (0.14) \quad (0.09) \\
 & - 0.30k + 0.11k'. \\
 & (0.09) \quad (0.07)
 \end{aligned}$$

<sup>1</sup> The account which follows is largely derived from a syllabus by T. Klock [18].

Its structure can be explained conveniently under the following four headings:

1. The first two terms deal with total output (excluding invisibles and inventory changes),  $v_m$  being the percentage change in this variable. It is obtained by weighting the four components of total output by their import contents:

$$(2) \quad v_m = 0.45c + 0.15x + 0.11i + 0.29b,$$

where  $c$  stands for the percentage change in total private consumption,  $x$  for that in autonomous expenditure (government expenditure on commodities, investment by government enterprises, and residential construction),  $i$  for that in gross private investment (excluding residential construction and inventory changes), and  $b$  for the percentage change in the volume of exports of commodities. It is interesting to compare the weights of (2) with those of the corresponding definitional equation. To see how this works out we consider the Keynesian identity  $Y = C + I$ , where the three variables stand successively for income, consumption, and investment. Now the variables in (2) are all annual percentage changes, so we should write the Keynesian identity in the form

$$(3) \quad 100 \frac{\Delta Y}{Y_{-1}} = \frac{C_{-1}}{Y_{-1}} \left( 100 \frac{\Delta C}{C_{-1}} \right) + \frac{I_{-1}}{Y_{-1}} \left( 100 \frac{\Delta I}{I_{-1}} \right),$$

whence it follows that the coefficients of the identity in this form are the lagged values of the ratios  $C/Y$  and  $I/Y$ . Obviously, these ratios are no constants; but as a first approximation they can be replaced by corresponding averages in the sample period. In our case this leads to the aggregate

$$(4) \quad v = 0.59c + 0.11x + 0.08i + 0.22b.$$

On comparing (2) with (4) we can conclude that the imports in the consumption sector are relatively less important than those in the other three sectors.

If we apply a linear approximation over time, we can write the first two terms on the right of (1) as

$$(5) \quad 1.24v_m + 0.29\{v_m - (v_m)_{-1}\} \sim 1.82(v_m)_{1/4},$$

which shows that these terms can be regarded as measuring the influences of output changes on imports with a lead of three months. This reflects the fact that imports are necessary before output is there. Given that the relation is linear in relative changes, we should interpret the coefficients as elasticities. In the present case the value is 1.82, i.e., well above 1, which implies that imports are rather sharply sensitive to relative changes in total output.

2. In the expenditure categories considered above, we excluded inven-

tory changes. The third term,  $2.50J$ , serves to correct for this omission. Since equation (1) is in relative changes,  $J$  itself should be defined in terms of relative changes in inventory changes. The precise definition is as follows:  $J$  is the first difference of the value of inventory changes expressed as a percentage of the value of last year's total output. (In accordance with  $v_m$  of (2), total output is defined as excluding invisibles and inventory changes.) It appears to be difficult to estimate the influence of  $J$ ; the elasticity value  $2\frac{1}{2}$  has the character of an a priori estimate.

3. The fourth and fifth terms of (1) deal with price effects. The definition of  $p_1$  is

$$(6) \quad p_1 = p_m - (p_2)_{-1} + 0.06\tau_{-1/2},$$

where  $p_m$  stands for the percentage change in the price level of imported commodities,  $p_2$  for that in the price index of total output (invisibles and inventory changes being again excluded), and  $\tau$  for the percentage change in the rate of indirect taxes less subsidies. Thus, the fourth term in (1),  $-0.38p_1$ , essentially says that if the import price level increases by 1 per cent relative to the home price level, the percentage change in the volume of imported commodities is about 0.4 below the value that would have been realized if import and home prices had moved proportionally. The half-year lag of  $p_2$  in (6) reflects the fact that, on average, import contracts are made about half a year before the goods are actually imported. The variable  $\tau$ , dealing with indirect taxes, serves as an approximation to import duties, the coefficient 0.06 being the weight used for this approximation; the four-month lag has the same meaning as the half-year lag of  $p_2$ . The fifth term ( $0.32\Delta p_2$ ), finally, serves to stress that sharpened inflationary strains at home encourage buying abroad.

4. The sixth and seventh terms of (1) have to do with quantitative import restrictions. These were introduced before the war and  $-0.30k$  represents their effect,  $k$  being the percentage of commodities (weighted according to their import shares in 1929) which were subject to such restrictions. After the war imports were liberalized and  $0.11k'$  stands for this effect,  $k'$  being the percentage of commodities (weighted according to their import shares in 1948) which were subject to liberalization.

The model as a whole contains 11 behavioral and technical equations as well as a large number of definitional equations. It has been estimated by the two-stage least-squares method (see Section on *Two-Stage Least-Squares*, p. 40 below) using data of the period 1923-38, 1949-57. Other behavioral and technical equations include a consumption function, an investment function, an equation explaining inventory changes, a demand-for-labor equation, an unemployment equation,

an export equation, and four equations describing different price indices.<sup>2</sup>

## II. *Econometric Methodology: Equation System Estimation*

The practical use of such models led naturally to an increased interest in the statistical methodology which underlies the construction of these models. Much of the basic work in this field had been done at the Cowles Commission for Research in Economics at the University of Chicago [14, 21], but the methods proposed were relatively complicated. Several successful efforts have been made to find simpler methods, at first in the Central Planning Bureau, later in the Econometric Institute. They will be illustrated here by means of a simple supply and demand system:

$$(7) \quad \begin{aligned} q(t) &= \alpha_1 p(t) + \alpha_2 k(t) + u_1(t) && \text{(supply)} \\ q(t) &= \alpha'_1 p(t) + \alpha'_2 \mu_1(t) + \alpha'_3 \mu_2(t) + u_2(t) && \text{(demand)} \end{aligned}$$

where  $q$  stands for the quantity bought and sold,  $p$  for the price,  $k$  for the price of a raw material,  $\mu_1$  and  $\mu_2$  for the incomes of two groups of buyers, and  $u_1$  and  $u_2$  for disturbance terms. The  $t$  in brackets stands for time (in years, say); it is supposed to take the values  $1, 2, \dots, T$ , where  $T$  is the number of observations (the sample size). The problem is to estimate the unknown parameters (the  $\alpha$ 's) on the basis of this sample of observations on the variables  $p, q, k, \mu_1$  and  $\mu_2$ . Constant terms have been deleted from (7); they are handled easily by measuring all variables as deviates from their means.

The usual approach is to take  $k, \mu_1$  and  $\mu_2$  as exogenous variables (whose values are nonstochastic) and  $p$  and  $q$  as endogenous variables (whose values are stochastic and correlated with the disturbances). The system (7) is then completely specified as soon as we specify the probabilistic properties of the disturbances. We shall specify that they have zero mean:

$$(8) \quad Eu_1(t) = Eu_2(t) = 0 \text{ for all } t,$$

that they are uncorrelated over time, and that they have constant variances and "contemporaneous" covariances:

$$(9) \quad E\{u_i(t)u_i(t')\} = \begin{cases} \sigma_i^2 & \text{if } t = t' \\ 0 & \text{if } t \neq t' \end{cases} \quad i = 1, 2;$$

$$(10) \quad E\{u_1(t)u_2(t')\} = \begin{cases} \rho\sigma_1\sigma_2 & \text{if } t = t' \\ 0 & \text{if } t \neq t', \end{cases}$$

<sup>2</sup> For more details, see the syllabus by Klok [18]. A very concise treatment is also given in the Appendix of [12].

where  $\sigma_i$  is the standard deviation of  $u_i(t)$  and  $\rho$  is the coefficient of correlation of  $u_1(t)$  and  $u_2(t)$ .

### *Two-Stage Least-Squares*

Suppose now that we decide to estimate the supply equation by ordinary least-squares. These estimates are obtained by solving the following normal equations:

$$(11) \quad \begin{bmatrix} \Sigma pq \\ \Sigma kq \end{bmatrix} = \begin{bmatrix} \Sigma p^2 & \Sigma pk \\ \Sigma pk & \Sigma k^2 \end{bmatrix} \begin{bmatrix} (\hat{\alpha}_1)_{LS} \\ (\hat{\alpha}_2)_{LS} \end{bmatrix},$$

where the summations are all over  $t$  (from 1 till  $T$ ). The difficulty with respect to least-squares is that the estimator is biased and inconsistent, which is due to the fact that one of the explanatory variables ( $p$ ) is endogenous and hence correlated with the disturbance term. We can attack this problem in the following manner. The system (7) can be solved with respect to the endogenous values  $p(t)$  and  $q(t)$ , which leads to a new system of two equations (the reduced form of the original system), one for  $p(t)$  and one for  $q(t)$ . The former equation is

$$(12) \quad p(t) = \bar{p}(t) + \frac{u_2(t) - u_1(t)}{\alpha_1 - \alpha_1'},$$

where

$$(13) \quad \bar{p}(t) = \frac{-\alpha_2 k(t) + \alpha_2' \mu_1(t) + \alpha_1' \mu_2(t)}{\alpha_1 - \alpha_1'}.$$

Next, we write the supply equation in the following form:

$$(14) \quad q(t) = \alpha_1 \bar{p}(t) + \alpha_2 k(t) + \left\{ u_1(t) + \alpha_1 \frac{u_2(t) - u_1(t)}{\alpha_1 - \alpha_1'} \right\}$$

i.e., we replace  $p(t)$  by  $\bar{p}(t)$  in the right-hand side but add the error made by doing so to the disturbance term. Suppose finally that we decide to estimate  $\alpha_1$  by computing an ordinary least-squares regression of  $q$  on  $\bar{p}$  and  $k$ . In fact, it is not difficult to see that this would solve the problem of correlation between explanatory variables and disturbances. For  $\bar{p}(t)$  is a linear combination of the exogenous values  $k(t)$ ,  $\mu_1(t)$ ,  $\mu_2(t)$ , see (13), so that it is nonstochastic; hence, by replacing  $p$  by  $\bar{p}$  we avoid the problem of a regression on variables which are correlated with the disturbances.

However, there is now the new difficulty that we do not know  $\bar{p}(t)$ , because it depends on unknown parameters, see (13). As a first approximation we can estimate  $\bar{p}(t)$  by computing the reduced-form regression, i.e., by applying least-squares with  $p$  as dependent and  $k$ ,  $\mu_1$  and  $\mu_2$  as

independent variables. Let us write  $v(t)$  for the residuals of this regression, then  $p(t) - v(t)$  estimates  $\bar{p}(t)$ . Hence (14) is now approximated by

$$(15) \quad q(t) = \alpha_1\{p(t) - v(t)\} + \alpha_2 k(t) + \{u_1(t) + \alpha_1 v(t)\},$$

and we run a second regression (hence the name two-stage least-squares, 2SLS), where  $q$  is the dependent variable and  $p-v$  and  $k$  the independent variables. This leads to the following normal equations:

$$(16) \quad \begin{bmatrix} \Sigma(p-v)q \\ \Sigma kq \end{bmatrix} = \begin{bmatrix} \Sigma(p-v)^2 & \Sigma p k \\ \Sigma p k & \Sigma k^2 \end{bmatrix} \begin{bmatrix} a_1 \\ a_2 \end{bmatrix},$$

where  $a_1$  and  $a_2$  are the 2SLS-estimates of  $\alpha_1$  and  $\alpha_2$ , respectively. The matrix of sampling variances and covariances is  $\sigma_1^2$  (the common variance of the disturbances of the supply equation) times the inverse of the  $2 \times 2$  matrix on the right of (16):

$$(17) \quad V \begin{bmatrix} a_1 \\ a_2 \end{bmatrix} = \frac{\sigma_1^2}{\Sigma(p-v)^2 \Sigma k^2 - (\Sigma p k)^2} \begin{bmatrix} \Sigma k^2 & -\Sigma p k \\ -\Sigma p k & \Sigma(p-v)^2 \end{bmatrix},$$

apart from terms which can be neglected when the sample size is sufficiently large. The 2SLS-method was found by H. Theil [38, 41] and later and independently by R. Basman [3].

### Three-Stage Least-Squares

We shall now attack the same problem along slightly different lines. We start by observing that the ordinary least-squares approach (11) amounts to multiplying the supply equation by the explanatory values  $p(t)$  and  $k(t)$  and summing over  $t$ , so that two linear equations result from which the estimates can be solved; furthermore, that the difficulty with respect to least-squares was that in one case these multipliers [viz.,  $p(t)$ ] are endogenous rather than exogenous. But if this is the case, an obvious approach is to multiply the supply equation by the exogenous values  $k(t)$ ,  $\mu_1(t)$  and  $\mu_2(t)$ . Doing so, and summing over  $t$ , we obtain the following three equations:

$$(18) \quad \begin{aligned} \Sigma k q &= \alpha_1 \Sigma p k + \alpha_2 \Sigma k^2 + \Sigma k u_1 \\ \Sigma \mu_1 q &= \alpha_1 \Sigma p \mu_1 + \alpha_2 \Sigma k \mu_1 + \Sigma \mu_1 u_1 \\ \Sigma \mu_2 q &= \alpha_1 \Sigma p \mu_2 + \alpha_2 \Sigma k \mu_2 + \Sigma \mu_2 u_1. \end{aligned}$$

Let us do the same with the demand equation:

$$(19) \quad \begin{aligned} \Sigma k q &= \alpha'_1 \Sigma p k + \alpha'_2 \Sigma k \mu_1 + \alpha'_3 \Sigma k \mu_2 + \Sigma k u_2 \\ \Sigma \mu_1 q &= \alpha'_1 \Sigma p \mu_1 + \alpha'_2 \Sigma \mu_1^2 + \alpha'_3 \Sigma \mu_1 \mu_2 + \Sigma \mu_1 u_2 \\ \Sigma \mu_2 q &= \alpha'_1 \Sigma p \mu_2 + \alpha'_2 \Sigma \mu_1 \mu_2 + \alpha'_3 \Sigma \mu_2^2 + \Sigma \mu_2 u_2. \end{aligned}$$

It is not difficult to see that (18) and (19) can be combined as follows:

$$(20) \quad \begin{bmatrix} \Sigma kq \\ \Sigma \mu_1 q \\ \Sigma \mu_2 q \\ \Sigma kq \\ \Sigma \mu_1 q \\ \Sigma \mu_2 q \end{bmatrix} = \begin{bmatrix} \Sigma pk & \Sigma k^2 & 0 & 0 & 0 \\ \Sigma p\mu_1 & \Sigma k\mu_1 & 0 & 0 & 0 \\ \Sigma p\mu_2 & \Sigma k\mu_2 & 0 & 0 & 0 \\ 0 & 0 & \Sigma pk & \Sigma k\mu_1 & \Sigma k\mu_2 \\ 0 & 0 & \Sigma p\mu_1 & \Sigma \mu_1^2 & \Sigma \mu_1\mu_2 \\ 0 & 0 & \Sigma p\mu_2 & \Sigma \mu_1\mu_2 & \Sigma \mu_2^2 \end{bmatrix} \begin{bmatrix} \alpha_1 \\ \alpha_2 \\ \alpha_1' \\ \alpha_2' \\ \alpha_3' \end{bmatrix} + \begin{bmatrix} \Sigma ku_1 \\ \Sigma \mu_1 u_1 \\ \Sigma \mu_2 u_1 \\ \Sigma ku_2 \\ \Sigma \mu_1 u_2 \\ \Sigma \mu_2 u_2 \end{bmatrix},$$

which is a system of six equations in the five unknown coefficients (the  $\alpha$ 's) of the supply-and-demand system. This suggests that we are approaching a method by which all coefficients of the system are estimated simultaneously, rather than equation by equation as in 2SLS. Indeed, the three-stage least-squares method (3SLS), developed by A. Zellner and Theil [52], does precisely this. The main ideas can be described conveniently in the following three steps:

1. We regard the last vector on the right of (20) as a disturbance vector. One might think that there is a complication due to the fact that its elements (such as  $\Sigma ku_1$ ,  $\Sigma \mu_1 u_1$ ,  $\dots$ ) are correlated with  $\Sigma pk$ ,  $\Sigma p\mu_1$ ,  $\dots$  (the elements of the  $6 \times 5$  matrix in (20)); but it can be proved that these correlations are asymptotically zero and hence do not matter for large samples.

2. The most straightforward approach would seem to run an ordinary least-squares regression on (20), i.e., to regard the left-hand column as six values taken by the dependent variable and the right-hand  $6 \times 5$  matrix as the array of values taken by five independent variables. This would lead directly to estimates of the five  $\alpha$ 's. However, ordinary least-squares is efficient only if the disturbances have all the same variance and are all uncorrelated, which is not the case here. For example, the variance of the first disturbance is

$$(21) \quad \text{var} (\Sigma ku_1) = \sum_i \sum_{i'} k(i)k(i')E\{u_1(i)u_1(i')\} = \sigma_1^2 \Sigma k^2$$

and the variance of the fourth disturbance,  $\Sigma ku_2$ , is  $\sigma_2^2 \Sigma k^2$ , while their covariance is  $\rho\sigma_1\sigma_2 \Sigma k^2$ . In this way one can derive the complete  $6 \times 6$

covariance matrix of the disturbance vector. If this covariance matrix is known, one can apply A. C. Aitken's method of generalized least-squares [1] to estimate the  $\alpha$ 's from (20). This method essentially amounts to a linear transformation of the system (20) such that the six transformed disturbances have the same variance and are uncorrelated; thereafter one applies least-squares in the way described in the beginning of this paragraph.

3. A difficulty is that the covariance matrix of the disturbance vector is in general not known. We just found  $\sigma_1^2 \Sigma k^2$ ,  $\sigma_2^2 \Sigma k^2$ , and  $\rho \sigma_1 \sigma_2 \Sigma k^2$  for three elements of this matrix; and when continuing in this way we find that all these elements depend on  $\sigma_1$ ,  $\sigma_2$ , and/or  $\rho$ , which are in general as much unknown as the  $\alpha$ 's. However, it can be shown that if we replace  $\sigma_1$ ,  $\sigma_2$ , and  $\rho$  by their 2SLS-estimates, the asymptotic properties of the resulting 3SLS  $\alpha$ -estimates are not affected. Hence the procedure is as follows: first, determine the 2SLS-estimates of the  $\alpha$ 's, both of the supply and of the demand equation, and compute the implied estimated variances and covariance of the disturbances of these equations; second, estimate the  $6 \times 6$  covariance matrix of the disturbance vector in (20) on the basis of the latter estimates and apply generalized least-squares to find new estimates of all  $\alpha$ 's. These estimates are called those of three-stage least-squares, the generalized least-squares procedure being the origin of "three." It can be shown that 3SLS is in general asymptotically more efficient than 2SLS.

### *Linearized Maximum Likelihood*

An interesting question is obviously whether the estimates considered so far are as efficient as or less efficient than the maximum-likelihood methods which were introduced by the Cowles Commission. Answers are possible when the sample size is sufficiently large. Regarding 2SLS, this method is asymptotically as efficient as limited-information maximum likelihood. As to 3SLS, it was recently shown by T. Rothenberg and C. T. Leenders [33] that its estimator is asymptotically as efficient as full-information maximum likelihood, provided that there is no prior information on the variances and covariances of the disturbances of the equations.

Since 2SLS and 3SLS are computationally far simpler than their maximum-likelihood counterparts, the interesting problem which remains is how to estimate efficiently in case there is prior information on the covariance matrix of the disturbances. Of course, full-information maximum likelihood does estimate efficiently in that case, but the difficulty is that its estimates have to be solved from nonlinear estimation equations. This problem, too, was attacked successfully by Rothenberg and Leenders, who derived the method of linearized maximum likeli-

hood (LML) which is asymptotically as efficient as the full-information maximum likelihood method. The main idea can be described easily as follows. Let  $\phi$  be the logarithmic likelihood function and write  $\theta$  for the vector of all parameters of the system. Let  $f$  be the vector of first-order derivatives of  $\phi$ ; then  $f(t)=0$  is the set of equations from which the maximum-likelihood estimates (arranged in a vector  $t$ ) can be solved. The problem is that these equations are nonlinear. The LML method linearizes them, starting at the 2SLS-estimates (for which we write  $t_0$ ). This means that  $f(t)=0$  is replaced by

$$(22) \quad f(t_0) + F(t - t_0) = 0,$$

where  $F$  is the matrix of second-order derivatives of  $\phi$  evaluated at  $t_0$ . Of course, (22) is only an approximation to  $f(t)=0$ , but it can be shown that the approximation error does not affect the large-sample properties of the estimator. Since  $f(t_0)$  and  $F$  are known, the estimator  $t$  can be derived easily by premultiplying (22) by  $F^{-1}$ ; it can also be shown that minus this inverse matrix is the covariance matrix of the LML estimator, apart from terms which tend to vanish when the sample size is sufficiently large.

### *On Underidentified Equations and Undersized Samples*

There are situations in which the estimation methods discussed here break down. To see this we go back to the supply and demand system and suppose now that  $\alpha_2=0$ , i.e., the variable  $k$  is no longer one of the exogenous variables of the system. In that case the addition of the supply equation (possibly multiplied by any nonzero coefficient) to the demand equation yields another linear relation between  $q$ ,  $p$ ,  $\mu_1$  and  $\mu_2$  which cannot be distinguished from the demand equation on the basis of observations. This is the case of underidentification, which occurs as soon as the number of coefficients of an equation exceeds the number of exogenous variables in the system. When the two numbers are equal [as happens to be the case for the demand equation of (7)], we have the situation of just-identification for which the estimation methods can be simplified in certain respects.

But most econometric models are much larger than (7) in the sense that they consist of more equations. The number of exogenous variables is then usually considerably in excess of the number of coefficients to be estimated in the separate equations,<sup>8</sup> so that the identification problem ceases to be important. Another restriction then gains in importance: the number of exogenous variables should not exceed the number of observations. This must be so because we need the inverse of the moment matrix of these variables, which would be singular if there were

<sup>8</sup> When there are lagged variables (either endogenous or exogenous) these should be added to the set of (current) exogenous variables to form the set of all "predetermined" variables.

fewer observations than variables. But there are nevertheless situations, particularly for large models, where this restriction is violated. As T. Kloek and L. B. M. Mennes [19] argued, the best procedure is then to replace exogenous variables by a certain number of their principal components.

This problem of "undersized samples" stands in sharp contrast to the large-sample character of the theory which underlies the estimation methods. A. L. Nagar [25, 26, 27] made several efforts to improve on this aspect by evaluating the bias of certain simultaneous-equation estimators to the order  $1/T$  and their moment matrix to the order  $1/T^2$ ,  $T$  being the number of observations.

### III. *Studies in Demand Theory*

#### *The Leser-Somermeijer-Houthakker Model*

In the last few years Mr. W. H. Somermeijer of the Central Bureau of Statistics and his assistants carried out several analyses in demand theory. The model which they use is the same as that of C. E. V. Leser [23]; it was used independently by H. S. Houthakker in two recent publications [16, 17]. It can be considered conveniently in relation to statistical estimation theory as will become clear immediately.

Let  $w_i$  for  $i=1, \dots, n$  be the budget share of the  $i$ th commodity; then the model implies that

$$(23) \quad w_i = \frac{A_i(\mu, p_i)}{\sum A_j(\mu, p_j)}$$

with

$$(24) \quad A_i(\mu, p_i) = B_i(\mu/p_i)^{\alpha_i} u_i,$$

where  $\mu$  is income or total expenditure,  $p_i$  the price of the  $i$ th commodity,  $B_i$  and  $\alpha_i$  parameters to be estimated and  $u_i$  a disturbance. It is easily seen that the budget shares satisfy the condition that they be all nonnegative and add up to 1. It can also be shown that the form of the demand functions implied by (23)–(24) is such that the Slutsky equations and inequalities are satisfied provided that  $\alpha_i > -1$  for all  $i$ . The income elasticity of  $w_i$  is  $\alpha_i - \bar{\alpha}$  where  $\bar{\alpha} = \sum w_i \alpha_i$  is the weighted mean of all  $\alpha$ 's with the budget shares as weights. (In fact, it is quite instructive to formulate the demand theory (23)–(24) in terms of a discrete frequency distribution of which the  $\alpha_i$  are the  $n$  values and the  $w_i$  the corresponding relative frequencies.)

To estimate the parameters  $B_i$  and  $\alpha_i$  we have to get rid of the awkward denominator in (23). This can be done conveniently by taking the ratio  $w_i/w_j$ , the natural logarithm of which is

$$(25) \quad \log(w_i/w_j) = \log(B_i/B_j) + \alpha_i \log(\mu/p_i) - \alpha_j \log(\mu/p_j) + u_i - u_j.$$

We can formulate such relations for all possible pairs  $(i, j)$ , but these are obviously linearly dependent. Also, one has to take account of the fact that in general there exist nonzero correlations between the disturbances. For example, take  $i=1, j=2$  in (25), so that the disturbance becomes  $u_1(i) - u_2(i)$  in any period  $t$  (assuming that we deal with time series data)<sup>4</sup> Next, take  $i=1, j=3$  and hence  $u_1(i) - u_3(i)$ . Now if  $u_1(i)$ ,  $u_2(i)$  and  $u_3(i)$  are uncorrelated, the differences  $u_1(i) - u_2(i)$  and  $u_1(i) - u_3(i)$  are correlated and the covariance of these differences is the variance of  $u_1(i)$ . Therefore, as soon as one decides to estimate the  $\alpha$ 's by combining several relations (25) for different pairs  $(i, j)$ , one has to take account of the fact that, as in the case of three-stage least-squares (see above, page 42), there are correlations among the elements of the combined disturbance vector. This problem was attacked recently by Zellner [51] in a manner similar to the 3SLS method.

The analyses carried out by W. H. Somermeyer and his associates deal with consumption, both for time series and for cross-section data [35, 50], with the demand for imports and home production [34], and with the alternative uses of farm land depending on prices [36]. The statistical approach followed by them differs from the method outlined in the preceding paragraph.

### *Almost Additive Preferences*

Houthakker [17] argued that the demand functions implied by (23)–(24) can be considered as belonging to “additive preferences” in the following sense. Classical consumption theory is based on a utility function with the quantities  $q_1, \dots, q_n$  as arguments, which is maximized subject to the budget constraint  $\sum p_i q_i = \mu$ . This leads, under appropriate conditions, to unique demand functions which describe the  $q$ 's in terms of prices and income. By substituting these demand functions in the utility function, utility becomes a function of prices and income: it is the highest utility level attainable, given these prices and this income level. Now the demand functions (23)–(24) can be derived from such an “indirect” utility function with the following special property: it is the sum of  $n$  separate functions, each of which contains income and only one price.

This case of “separable” utilities is plausible enough when one has to do with goods which are characterized by little interdependence in the consumer's utility evaluation; but it is more plausible at the level of “direct” utility (depending on quantities, the  $q$ 's) than at the level of indirect utility. This was pursued further by Barten [2], who advanced the following theory of “almost additive preferences.” If utility is completely separable with respect to all  $n$  commodities (in the “di-

<sup>4</sup> When cross-section data are used,  $t$  stands for a household.

rect" sense), the  $n \times n$  matrix of second-order derivatives of the utility function,  $U$  say, is diagonal. When we accept the idea of decreasing marginal utilities, we can write  $U = -\bar{u}^2$  where  $\bar{u}$  is a diagonal matrix with positive elements on the diagonal. In the case of nonadditive preferences it is always possible to write  $U$  in the form  $-\bar{u}(I + \Theta)\bar{u}$ , where  $I$  is the  $n \times n$  unit matrix and  $\Theta$  a symmetric matrix with zeros on the diagonal. Thus  $\Theta$  can be regarded as measuring the degree of interdependence of the various goods in the consumer's utility scale. Now the inverse of  $U$  plays an important role in the demand functions. If the elements of  $\Theta$  are sufficiently close to zero, we can approximate as follows:

$$(26) \quad U^{-1} = -[\bar{u}(I + \Theta)\bar{u}]^{-1} \approx -\bar{u}^{-1}(I - \Theta)\bar{u}^{-1},$$

and preferences are regarded to be almost additive when (26) is considered to be a satisfactory approximation. It is easily seen from (26) that if the  $(i, j)$ th element of  $U$  vanishes, i.e., if the marginal utility of the  $i$ th commodity is independent of the  $j$ th consumption, the same element of  $U^{-1}$  vanishes too. This property is very convenient for the estimation of the price elasticities which will be considered now.

The utility function is maximized subject to the budget constraint in the conventional way, which implies the introduction of the Lagrangean multiplier  $\lambda$ , the marginal utility of income. This leads to the first-order equilibrium conditions, from which the demand functions can in principle be solved. By differentiating the first-order conditions with respect to income and prices, we obtain the following system of  $n+1$  equations:

$$(27) \quad \begin{bmatrix} U & p \\ p' & 0 \end{bmatrix} \begin{bmatrix} q_\mu & Q_p \\ -\lambda_\mu & -\lambda_p \end{bmatrix} = \begin{bmatrix} 0 & \lambda I \\ 1 & -q' \end{bmatrix},$$

where  $U$  is the matrix of second-order derivatives of the utility function which was discussed in the preceding paragraph,  $p$  is the column vector of prices,  $q_\mu$  the column vector of income derivatives of the demand function,  $Q_p$  the matrix of price derivatives, while  $\lambda_\mu$  and  $\lambda_p$  represent the derivatives of  $\lambda$  with respect to  $\mu$  and  $p$ , respectively. After some algebraic rearrangements we can express  $Q_p$  in terms of  $\lambda$ ,  $\lambda_\mu$ ,  $U$ ,  $q$  and  $q_\mu$ :

$$(28) \quad Q_p = \lambda U^{-1} - (\lambda/\lambda_\mu) q_\mu q'_\mu - q_\mu q'_p,$$

which expresses the price derivatives as the sum of three terms. The third is the income effect, the first two represent jointly the substitution effect. The off-diagonal elements of the first term represent the substitution effect insofar as this is due to the interaction of commodities in the consumer's utility evaluation; the second term deals with "overall"

substitution and expresses the fact that all commodities compete for the consumer's guilder.

Barten applied this theory to 14 commodity groups in the interwar and postwar period and permitted 11 elements of  $\Theta$  above the diagonal to differ from zero. He approximated his demand functions by a linear expression in the logarithms, so that the coefficients found are essentially elasticities. One difficulty in the estimation is that the coefficients are subject to nonlinear constraints; this follows directly from the second term on the right of (28), which contains products of income derivatives. Such nonlinear constraints have to be handled by an iterative procedure. Another difficulty is that a number of constraints deal with coefficients of different demand equations. This follows from the first term in the right-hand side of (28):  $\lambda U^{-1}$  is symmetric and hence the  $(i, j)$ th element, which occurs in the  $i$ th equation and describes part of the effect of the  $j$ th price, is equal to the  $(j, i)$ th element which has its place in the  $j$ th demand equation. It is, therefore, necessary to have two consecutive sets of computations, the first of which is on equation-by-equation basis and disregards constraints on coefficients of different equations, while the second takes such constraints into account by proceeding on equation system basis. Both sets are iterative (in view of the nonlinearity of certain constraints); the second set is essentially an application of Zellner's method as mentioned above, p. 45.

Barten added a third set of computations of a Bayesian nature to incorporate his prior beliefs on parameter values in a probabilistic manner. The approach will be illustrated here for the simple case of the demand for food (say):

$$(29) \quad \log F = \alpha + \beta_1 \log \mu + \beta_2 \log p + u,$$

where  $\beta_1$  is the income elasticity and  $\beta_2$  the price elasticity. Suppose now that we know (from previous experience, from introspection, or from any other source of information which is independent of the sample) that the income elasticity of food is likely to be between 0.4 and 0.6 and is very unlikely to lie outside the interval (0.3, 0.7). We can formalize such prior "uncertain" information approximately by saying that the income elasticity is subject to a prior distribution which is normal (say) with mean 0.5 and standard deviation 0.1. Thus, let  $r$  be the prior estimate of  $\beta_1$ , then

$$(30) \quad r = \beta_1 + v \quad \text{where} \quad Ev = 0, \quad Ev^2 = 0.01,$$

$v$  being the error of the prior estimate  $r$  which itself takes the value 0.5 in this case. The Bayesian procedure then implies that the sample information (29) and the prior information (30) are combined in the manner of Aitken's generalized least-squares. This is straightforward insofar as both kinds of information are linear in the parameters to be

estimated; there is a difficulty due to the fact that we need the ratio of the variance of  $u$  in (29) to the variance of  $v$  in (30), which is responsible for the fact that the theory is essentially of the large-sample variety. Reference is made to Theil and A. S. Goldberger [43] and Theil [42] and, for a slightly different set-up, to H. Raiffa and R. Schlaifer [32]. In Barten's case there are additional complications due to nonlinearities.

#### IV. *Models for Decision Purposes*

##### *Targets and Decision Rules*

The purpose of an econometric model may be confined to a better understanding of the working of the economy; it may also be to contribute to a more "rational" economic policy. Tinbergen was the first in the Netherlands who worked systematically in this area; a few years later other contributions were made by Theil. Since the relevant publications are all in English [41, 48], I shall confine myself to a brief outline.

Suppose we have a complete econometric model of which the reduced form exists. This reduced form describes the endogenous variables (e.g., consumption, investment, employment, and the like) in terms of lagged variables, exogenous variables, and random disturbances. None of the endogenous variables is controlled by the government, but some of the exogenous variables are, e.g., tax rates, government expenditure on goods and services, etc. Let us assume (to keep things simple) that all exogenous variables are government-controlled. Then the reduced form enables us to predict what will happen to the endogenous variables if these government variables are subject to given specified changes, i.e., if the government takes certain measures of economic policy. In this way the Central Planning Bureau is in a position to inform the government about the consequences of alternative lines of action. But the Tinbergenean approach turns things upside down. Tinbergen starts with given targets for the endogenous variables: a 5 per cent increase of employment in private industry, a constant price level of consumer goods, a  $\frac{1}{2}$  billion surplus on the balance of payments, etc. All these targets refer to next year, say. Suppose now that the number of such "target variables" is equal to the number of government-controlled variables. Then we can use the reduced form to compute the values which the latter variables should take in order that the former take their target values. In this way one uses the model to design a policy which leads to the desired targets.

It will be clear that this approach is subject to certain qualifications. For example, it is not always true that the number of target variables equals the number of government-controlled variables. If the former is larger there exists in general no solution; if it is smaller there are in gen-

eral infinitely many, so that it is then possible to take account of, say, preferences with respect to the values taken by controlled variables. A fundamental problem is that of uncertainty. It was mentioned at the beginning of the preceding paragraph that the reduced form contains random disturbances. It is only by neglecting such disturbances that we can arrive at unique values for the controlled variables in the decision problem; if we do wish to take account of random variation, there are no such values which lead to the targets with probability 1. A similar situation arises if the model contains exogenous variables which are not controlled, such as the import price level. The size of the population, etc. The values which these variables will take in the year in question have to be predicted, which leads to a type of uncertainty that is comparable to the residual variation discussed above. There is also the difficulty that models are never perfect, even apart from the random variation of the disturbances. They are estimated and the resulting coefficients are subject to sampling errors. There may also be specification errors: e.g., an equation may have been estimated in linear form whereas the "true" form is curvilinear.<sup>6</sup>

No approach is available which takes account of all these difficulties simultaneously. An attempt has been made by Theil to handle the uncertainty problem for given coefficients (without sampling and specification errors), but only for the case in which the decision-maker's preferences can be represented by a quadratic utility function. However, if this framework is accepted, the analysis can be extended rather far; in particular, the approach can be generalized to handle dynamic decision problems. This leads to a strategy or a decision rule according to which the appropriate values of the controlled variables are made dependent on the information which is or will be available at the moment when the relevant decision has to be taken. Several studies have been made dealing with macroeconomic applications of this approach [4, 5, 6, 7, 44, 45]. A related microeconomic set-up, dealing with production and work-force scheduling of a factory, has been worked out by C. C. Holt, F. Modigliani, J. F. Muth and H. A. Simon [13] and was reconsidered recently by C. Van de Panne [28, 29].

### *Quadratic Programming and the Optimal Use of Milk*

The final topic which I shall consider has also to do with decision-making and is also of the quadratic utility variety, but it is otherwise of a somewhat different nature. It deals with milk, of which too much is being produced in the Netherlands in the sense that the free-market

<sup>6</sup> For problems of specification and aggregation analysis, see [39, 40, 41]. See also Houthakker's comment [15] on Tinbergen's *On the Theory of Economic Policy* as well as Tinbergen's reply [47].

supply would exceed demand to such an extent that prices would not cover farmers' costs. This induced the Dutch government to subsidize farmers to an amount of more than 2 per cent of national income. Recently, S. L. Louwes, J. C. G. Boot and S. Wage [24] tried to design a policy by which this subsidy can be reduced. They distinguished between four products: consumption milk, butter, fat cheese, and meagre cheese (of which annual consumption will be denoted by  $x_1, x_2, x_3, x_4$  and prices by  $p_1, \dots, p_4$ ). Their objective was to maximize gross revenue,  $\sum p_i x_i$ , by an appropriate adjustment of prices. For this purpose they formulated linear demand functions, so that gross revenue becomes a quadratic function of prices:

$$(31) \quad Q(p) = a'p - \frac{1}{2}p'Bp,$$

where  $p$  is the price vector and  $a$  and  $B$  coefficient matrices, the former being of order  $4 \times 1$  and the latter a  $4 \times 4$  symmetric and positive-definite matrix. It is perfectly straightforward to maximize  $Q$  (the solution is  $p = B^{-1}a$ ), but the difficulty is that this solution implies a negative consumption of meagre cheese. It is, therefore, necessary to take account of appropriate constraints, which in this case take the form of linear inequalities. First, we have the nonnegative constraints on prices and quantities. Second, there are fat and dry matter requirements for each of the four products and these are also linear inequalities.

The problem is thus to maximize  $Q$  as defined in (31) subject to a number of constraints in linear inequality form, i.e., it is a quadratic programming problem. One might think that the unconditional maximum (which was reported above to violate one of the constraints) is worthless for the derivation of the constrained maximum; but this, as was shown in [46], is not the case. By imposing the violated constraint in equational form and proceeding in this way systematically, one can find the optimum after a number of steps.<sup>6</sup> In this case the solution is rather drastic compared with the observed present situation: the milk price should be doubled, the same applies to the price of fat cheese, the production of meagre cheese should be discontinued. Gross revenue is increased by such an amount that the increase in net revenue can be expected to make the government subsidy superfluous. However, the increase of consumer prices seems to be unacceptable from a social point of view. For this reason the authors decided to add a policy constraint according to which a weighted average of the relative increases in the prices  $p_1, \dots, p_4$  is not permitted to be larger than  $\pi$ , where  $\pi$  is a variable percentage. This is a problem of parametric programming and yields solutions for the optimal prices depending on  $\pi$ , i.e., depending on the degree to which one is willing to let the consumers of milk and milk products suffer.

<sup>6</sup> See also [8, 9, 10, 22].

Needless to say, this result is subject to a number of qualifications. No account is taken of uncertainty, the theory of stochastic programming being still in its beginning. To assume that the demand functions are linear is convenient because it leads to a quadratic objective function, but this is obviously only an approximation, particularly when prices are subject to sizable changes. In fact, these changes are sizable unless  $\pi$  is small; and since it seems plausible that the rate of change of demand with respect to price becomes smaller when the price becomes larger and larger, one may expect that the formulas used tend to underestimate the gross revenue of the solution.

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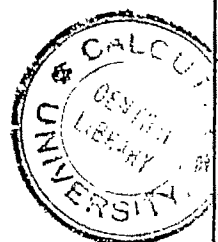
# AMERICAN ECONOMIC REVIEW

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SUPPLEMENT



## SURVEYS OF FOREIGN POSTWAR DEVELOPMENTS IN ECONOMIC THOUGHT

1. Some Postwar Contributions of French Economists to Theory and Public Policy, with Special Emphasis on Problems of Resource Allocation  
Jacques H. Drèze
2. Some Trends in Economic Thought in Denmark  
Jørgen H. Gelting
3. Survey of Economic Research in Postwar Japan—Major Issues of Theory and Public Policy Arising out of Postwar Economic Problems  
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## FOREWORD

The three essays appearing in this supplement of the *American Economic Review* are part of a series of papers recounting and assessing postwar developments in economic thought in leading professional centers where English is not the conventional language in use. This venture was made possible by a grant of the Ford Foundation to the American Economic Association. The Committee is pleased to be able to make these essays available to the membership.

In extending our invitation, the Committee asked each author to consider whether the impact of postwar policy problems had been significant to the development of thinking and research, or whether the evolution of professional work had taken a more detached course. We also urged him to concentrate upon a few major themes. Notwithstanding these suggestions, each author was given full freedom to develop his topic as he envisaged it. In consequence, each essay represents his personal judgment of what he considers to be the important developments in thought in the country or region with which he is concerned.

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# SOME POSTWAR CONTRIBUTIONS OF FRENCH ECONOMISTS TO THEORY AND PUBLIC POLICY

WITH SPECIAL EMPHASIS ON PROBLEMS OF RESOURCE ALLOCATION

By JACQUES H. DRÈZE\*

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Since the end of World War II France has been faced with an exceptional variety of economic problems, including postwar reconstruction and bottlenecks, inflation and balance of payment difficulties, labor

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problems, overseas wars and the loss of colonial territories, a series of nationalizations, the launching of nationwide planning, entry into the European Economic Community, etc. . . . As was to be expected, new policies have been developed to cope with at least some of these problems, and theorizing about the problems and policies has been abundant.

No choice of topics for survey from such rich material could do justice to the many contributors to French economic literature<sup>1</sup> in the postwar period. Forced to choose, and wishing to organize my review around a few widely-discussed topics rather than to enumerate individual endeavors, I have decided to survey the major contributions bearing on problems of resource allocation. This leaves out much more than is included<sup>2</sup> and results in a systematic bias for which my own tastes largely account. Indeed, this paper comes close to being a survey of contemporary quantitative economics in France. Considering the reservations of many French economists regarding the mathematical approach,<sup>3</sup> the bias is severe. I must admit it and accept the blame.

The bulk of this paper is divided into three largely independent sections. The first is devoted to a set of theoretical contributions that led to highly ingenious applications in the area of marginal cost pricing. The second covers some fundamental developments of the theory of intertemporal allocation. A few historical comments about the "French Marginalist School" serve as an introduction to these two sections. The third section describes the concepts and methods underlying the French approach to planning. It thus deals with public policy rather than with theory. To the extent that French planning has been more concerned with problems of resource allocation than with problems of economic stability, the main emphasis of this paper is maintained throughout the third section.

Each section can be read independently of the other two and to some extent is addressed to a different reader. The work surveyed in Sections I and II involves some rather technical and intricate arguments and relies heavily upon abstract theoretical concepts. The reader who has

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wishes all members of the French economic profession to share his expression of deep gratitude, both for the stimulation he has received from their work and for the personal courtesies which have facilitated his task. He also presents sincere apologies for his grave sins of omission in writing this survey.

Professors Robert Strotz, Peter Pashigian, and George Hildebrand gave the author the benefit of their penetrating comments on the first draft of this paper. The editorial assistance of Miss Nancy Connolly at the University of Chicago has been immensely helpful.

<sup>1</sup> That literature is restricted, under the narrow definition adopted for this paper, to work done and published in *France* (not in *French*). After all, I live in Belgium. . . .

<sup>2</sup> Comprehensive surveys, that do not overlap much with the present selection may be found in James [74], André Marchal [99], Piettre [146], and in a way in *Encyclopédie française* [57] to which a large number of contemporary French economists have contributed.

<sup>3</sup> See, e.g., Piettre [147].

not kept abreast of the literature in the areas under review is thus advised to regard these two sections (which are largely self contained) as material better suited for patient study than for cursory reading. Section I further differs from Section II in that I have attempted to sketch the reasoning underlying the major propositions about marginal cost pricing, whereas the section on intertemporal allocation is essentially a statement of concepts and results. My motivation has been to provide the interested reader of Section I with enough details to familiarize him with a frame of reasoning and to enable him to reconstruct the whole argument, should he so desire.<sup>4</sup> The complexity of the material covered in Section II precluded any such attempt. As for Section III (which is again largely self contained), its more descriptive nature makes it more accessible—and there frustration is likely to arise as a consequence of the paucity rather than of the intricacies of analytical developments.

Anyone who is acquainted with French economics will wonder why the work of François Perroux and his associates at the "Institut des Sciences Economiques Appliquées" is not discussed here. I owe them and my English-speaking readers a special apology. Although I was planning to devote a section of this paper to the work of Professor Perroux, I finally felt so hopelessly unprepared for this task that I had to give it up. In the process I enjoyed some very stimulating readings and was led to meditate upon some particular difficulties in the dissemination of knowledge.

Writing about the neo-classical tools of analysis, Perroux warns us that, by their very design, they *deny us the right* to handle certain matters:

... ce sont les compétitions assez impures et imparfaites *pour exister*; les rendements croissants, sans quoi les oeuvres humaines seraient trop tranquilles pour être vraies; les liaisons différentes des seules liaisons de prix sans quoi il faudrait imaginer ce monstre: une croissance sans groupements de producteurs, sans profits liés, sans investissements directs; les inégalités entre sujets et entre groupes organisés, combattues, renaissantes qui construisent ou démolissent les architectures de leurs hiérarchies innombrables.<sup>5</sup>

Those are precisely the matters with which Perroux has been concerned in some 30 books and 300 papers published over the last 30 years, and

<sup>4</sup> The material surveyed in Section I is by and large arranged in order of increasing complexity of the problems, so that the demands placed upon the reader are also increasing.

<sup>5</sup> Perroux [143], p. 173; an *approximate* translation (traduttore, traditore!) goes as follows: "... they are: competitions sufficiently imperfect to exist; increasing returns, without which the works of man would be too steady to be real; non-price relationships, in the absence of which one should invent that monstrosity: growth without producers' coalitions, without joint profits, without autonomous investment; inequalities among subjects and organized groups, fought, revived, that build or break the structures of their countless hierarchies."

more particularly, in his major postwar publications.<sup>6</sup> Never deterred by the difficulty of the subject, he exhibits an original understanding of modern societies,<sup>7</sup> develops new concepts which go beyond external economies<sup>8</sup> or bypass interpersonal comparisons of utility,<sup>9</sup> and applies them to some of the most urgent problems of our times.<sup>10</sup>

These writings are always thought-provoking—ideas flow in all directions beyond familiar borders, and their expression is too rich to be altered. That such writings should defy summary is perhaps understandable. It is also understandable, but unfortunate, that contributions of this sort are so slowly and incompletely “assimilated” by the profession.<sup>11</sup> The tasks of recasting Perroux’s ideas in terms more familiar to most economists and of developing rigorously normative as well as empirically testable implications of his theories are indeed quite formidable. Yet it must be hoped that they will be carried out some day—for otherwise the contributions of a remarkably original thinker might well be partly lost.

### *The French Marginalist School*

During World War II two graduates from the “Ecole Polytechnique,” Maurice Allais and Pierre Massé, renewed a long tradition of contributions to mathematical economics started by Cournot and the engineer Dupuit a hundred years before and more recently maintained by such well-known econometricians as F. Divisia and R. Roy.<sup>12</sup> Allais published a treatise on economic analysis dealing with a closed non-monetary economy. His “social efficiency theorem” [3] stating the Pareto optimality properties of a competitive equilibrium<sup>13</sup> and the emphasis laid upon a general equilibrium approach to policy problems had a profound influence upon the thinking of young mathematical economists in

<sup>6</sup> Only three papers of Perroux [134, 138, 140] have appeared in English; [137, 142] are good introductions to further readings; [143] is an important collection of selected papers, of which an English translation would certainly be a welcome addition to many libraries.

<sup>7</sup> For instance, in his analysis of “domination”—see [132].

<sup>8</sup> “Development poles” and “progress foci”—see [137].

<sup>9</sup> “The costs of man”—see [135].

<sup>10</sup> In particular, problems of development—see [141].

<sup>11</sup> Perroux has trained a number of productive *disciples* and his influence on ideas and institutions has been great; but his impact on the economic literature (outside of his immediate followers) has been rather slight. Few, if any, of his ideas have ever been submitted to the hard test of prolonged professional debate at the empirical as well as theoretical levels.

<sup>12</sup> See Dupuit [56], Divisia [53]: “General Introduction: the contribution of French engineers to economics,” and the foreword of Lesourne [81].

<sup>13</sup> More recent developments, e.g., Debreu [47], generalize the theorem of Allais without significantly altering its scope; Thionet [156] gives a good summary and discussion of the theorem; Allais [4] sketches the proof.

France.<sup>14</sup> The comprehensiveness and wealth of original derivations<sup>15</sup> of that treatise, which was written under wartime conditions with very restricted access to the economic literature, are simply amazing. Some of its developments (e.g., the analysis of equilibrium when increasing returns to scale prevail in some industries)<sup>16</sup> have not become standard yet; others (e.g., the discussion of units of measurement)<sup>17</sup> bear upon problems of broad scientific interest outside of economics.<sup>18</sup>

At about the same time Massé [104 and 105] was investigating the optimal decision rule for operating a hydroelectric dam; among other things<sup>19</sup> he was led to formulate the "principle of optimality" of dynamic programming,<sup>20</sup> develop some general properties of stochastic marginal analysis,<sup>21</sup> generate horizon rules for a class of stochastic dynamic problems, and to handle problems of constrained optimization. This study, bearing upon a large class of economic problems under uncertainty,<sup>22</sup> established an early bridge between operations research (of which Massé was a precursor) and economic analysis; as such, as well as through the emphasis it put upon problems of dynamics and uncertainty, it also had a profound influence upon contemporary thinking in France.

Shortly after the war the problems of reconstruction and of management of the newly nationalized industries (electricity, gas,<sup>23</sup> coal mining) gave Allais, Massé, and their colleagues, students, or followers ample opportunities for applying and developing their theories. Allais [5] approached from the viewpoint of social efficiency some problems of transportation that also retained the attention of M. Boiteux [28] and of the research staff at the nationalized railroad company<sup>24</sup> where R. Hutter [70] displayed a continued interest in applied economic analysis.

<sup>14</sup> Allais has proudly reminded me that Debreu, Boiteux, Malinvaud, Nataf, Lesourne, Verhulst, Maillet, Nahon, . . . , are former students of his.

<sup>15</sup> For a summary account of these original developments, see the preface to the 1952 edition, pp. 18-33.

<sup>16</sup> See [3], pp. 304-305.

<sup>17</sup> See [3], pp. 220-267.

<sup>18</sup> Allais has published extensively in areas not covered by this survey, ranging from monetary theory [11] to physics [12] or politics [15].

<sup>19</sup> Thus Massé [105], Vol. II, pp. 8 ff., discusses informally an axiomatic concept of cardinal utility along lines parallel to the independent development of von Neumann and Morgenstern.

<sup>20</sup> As outlined, for instance, by Arrow [17], p. 525.

<sup>21</sup> See Massé [105], II, p. 39, for the important theorem on equality between the marginal utility of a flow and the marginal expected utility of the residual stock; also Massé [106 and 108].

<sup>22</sup> Mostly problems of inventory management or investment.

<sup>23</sup> The gas industry was studied by Verhulst [166 and 167].

<sup>24</sup> "Société Nationale des Chemins de Fer" (hereafter referred to as SNCF).

A subsequent study by Allais [7] of the management of the nationalized coal mines proved equally stimulating.<sup>25</sup>

At the same time Massé [107] was moving from the problems of operating policies to those of investment policy for the nationalized electricity industry;<sup>26</sup> the complementary aspects of those two classes of problems played an important role in many subsequent studies. His analysis of stochastic inventory problems led to further work by himself [109 and 110] and others (e.g., G. Kreweras [76]) as well as to a particularly significant and ingenious application by P. Gardent [61, 62] who derived a decision rule for the national production, import and inventory holdings of coal.<sup>27</sup>

At EDF substantial interest was focused at that time on the problem of marginal cost pricing, to which G. Dessus [52] and Boiteux were rapidly making important contributions.<sup>28</sup> By then what has sometimes been referred to as the French "marginalist" or "mathematical" school was born; an important stream of scientific activity was under way that has developed continuously ever since.

This development has been unusual in many respects. For one thing, it has taken place largely outside of the traditional professional circles and channels. Members of this "school" did and do belong to the staffs of the engineering schools or statistics departments, to the research as well as the executive divisions of the nationalized industries, or to the administration, but not to the staffs of economics departments or economic research institutes. Many important papers appeared in technological journals (especially journals catering to public utility problems) which the economic profession does not usually read. Actually most members of this "school" are graduate engineers (occasionally mathematicians), as were its initiators, and they have received limited formal training in economics. The main points of contact with general economists could have been the Econometric Society meetings and the seminars of Professors Allais, G. Guilbaud and Roy—but few general economists ever attend such meetings, for lack of interest and/or mathematical background. It is worth remarking that limited communication with the economic profession and literature does not seem to have significantly hindered the work of the marginalist or mathematical school.<sup>29</sup> One cannot help wondering whether such communication was

<sup>25</sup> It is noteworthy that these two applied studies of Allais [5 and 7] were conducted at the request of the nationalized railroads and coal mines respectively. Did Hicks ever work for the nationalized British industries?

<sup>26</sup> "Electricité de France" (hereafter referred to as EDF). The same problem was attacked by Gibrat [64].

<sup>27</sup> The work of Gardent is taken up by Massé [112], pp. 337-353 of the English text.

<sup>28</sup> For references, see Section I, p. 8.

<sup>29</sup> From the outset, however, communication has been excellent between this group and

perhaps adequately replaced by the close contact with reality which dominated much of the work here under review.

Indeed, the continued interaction between theory and practice has been another remarkable feature of these developments. While the pure theorist Allais was consulted about the management of the coal mines, Massé or Boiteux, who had executive responsibilities at EDF, developed original contributions to decision or price theory. The theorists and the executives fortunately shared the view that there is no sound policy unless it is based upon a sound theory, whereas empirical relevance and verification make for sound theories. The fact that so much work has been motivated by empirical problems and that it eventually led to practical applications may partly account for the soundness of the theories.<sup>30</sup>

Still there is one respect in which the engineering background of the research has resulted in a severe limitation. Much of the work surveyed in this paper belongs to "normative" rather than to "positive" economics.<sup>31</sup> That the normative analysis was developed first is quite natural. Yet empirical concern with the positive aspects of nationalizations, pricing policies, and planning would be very desirable at this stage; little work has been done along these lines. Accordingly, we are still lacking an objective evaluation of the actual incidence of postwar policies—either micro- or macroeconomic. Such an evaluation is obviously essential to further progress.

The bulk of the French contributions to the *theory* of resource allocation can conveniently be grouped under two headings, namely, those dealing with the management of a firm or industry and those dealing with the (general equilibrium) conditions of social efficiency. Under the first heading are mostly studies concerned with cost minimization, price-output decisions, and investment policy.<sup>32</sup> Of these three domains, the second only will be reviewed here (Section I). The third, to which belong important theoretical<sup>33</sup> as well as applied studies,<sup>34</sup> has recently been the

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scientists in statistics and operations research. The connection with operations research, obvious in the case of Massé and more accidental for Allais [10], has been especially characteristic of the research divisions in the nationalized industries; some particularly brilliant contributions have come from SNCF, e.g., Matthys and Ricard [119].

<sup>30</sup> The exceptional intellectual qualities and close connections of people recruited (by contests) through the major engineering schools ("grandes écoles") has, of course, been another crucial factor.

<sup>31</sup> The most noteworthy exception to this statement is Allais; see, for instance, his interesting derivation of an objective test of rational management of the coal mines [7], Appendix 10; or the attempts at empirical testing of his monetary [11] and growth [13] models.

<sup>32</sup> Some studies escape this classification, in particular, those concerned with demand analysis for individual goods, of which remarkable instances are Mainguy and Mothes [90], or Mainguy and Nahon [91] for gas, and Morlat [122] for electricity.

<sup>33</sup> For instance, the surplus approach to investment decisions under increasing returns of Lesourne [80].

<sup>34</sup> In particular, the linear programming approach to electricity equipment policy over a

subject of a brilliant synthesis by Massé [112], now available in English.<sup>35</sup> As for cost minimization problems, they have been the subject of much applied research, most of which, however, falls rather in the province of operations research,<sup>36</sup> which is beyond the scope of the present survey.

Under the second heading the extensions of the theory of Pareto—optimality to intertemporal allocation and to uncertainty is first and foremost. Intertemporal allocation is the subject of Section II. The work on uncertainty (mostly concerned with decision-theory)<sup>37</sup> can be appraised through the proceedings of two symposia sponsored by the Centre National de la Recherche Scientifique [43, 46].<sup>38</sup> It is once more typical of the “French school” that work in this area has been influenced by practical problems (in particular, the treatment of risk and capital in the management of the nationalized utilities) and has been definitely less formal than the work in the same area by American economists.<sup>39</sup> However, the impact of that work on current orientation in the planning field is also beginning to be felt—as the final section of this paper indicates.

### I. *Marginal Cost Pricing*

#### *Concepts and Definitions*<sup>40</sup>

Much of the success of the French marginalist school in solving difficult practical problems in this area rests ultimately upon a sound and sometimes subtle understanding of the classical marginal cost concepts. Aside from the usual distinction between short-term and long-term cost curves, a further distinction between expansion and contraction costs has been repeatedly used.

Starting from the total cost curve for a given plant, its slope at any output defines the short-term marginal cost of that output. As plant sizes and designs vary, the envelope to the corresponding total cost curves defines a long-term total cost curve, whose slope at any output

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12-year span at EDF—Massé and Gibrat [118]; Massé [112], Chap. IV, VIII; Bessière [24]; Boiteux and Bessière [38].

<sup>35</sup> The related problem of depreciation has been the subject of several studies by Boiteux; see, e.g., Boiteux [36] and Lhermite [84].

<sup>36</sup> See footnote 29 above, and issues of the *Revue Française de Recherche Operationnelle*, 1957 to date, for references.

<sup>37</sup> A paper by Allais [8] deals explicitly with the extension of general equilibrium theory to uncertainty. The “classics” on this subject are a paper by Arrow [16], available only in French to date, and a later paper by Debreu, written in Paris in 1953, and reprinted in Debreu [47]. See also Baudier [23].

<sup>38</sup> See also the general exposition of Massé [112], Ch. 5.

<sup>39</sup> A keen scrutiny of that literature, however, has been conducted by Nataf (unpublished notes) and Thionet [160 and 164].

<sup>40</sup> See Allais [7], Annex II, Boiteux [27], Section I.1., or Hutter [70], I.

defines the long-term marginal cost of that output. If the optimum output of a given plant is defined as that for which its (short-term) total cost curve is tangent to the long-term total cost curve,<sup>41</sup> then short-term marginal cost is higher than, equal to, or lower than long-term marginal cost according to whether actual output is larger than, equal to, or smaller than optimum output. However, for a plant of rigid capacity<sup>42</sup> short-term marginal cost is indeterminate at maximum output; hence, the relation of short-term to long-term marginal cost is also indeterminate at that output.<sup>43</sup>

In defining long-term marginal cost as the slope of the envelope, one implicitly assumes that changes in output take place in a way that constantly permits perfect adjustment of plant design to the quantity produced (so that equality between short-term and long-term marginal costs holds at every point in time). To the extent that actual plant variations will not meet this condition, actual costs of expansion ("development") or contraction ("regression") in plant capacity<sup>44</sup> will not be equal to long-term marginal cost and will also frequently not equal each other.<sup>45</sup> Hence the need for the concepts of "expansion cost" and "contraction cost"—whose importance lies not at all in the (classical) introduction of a "middle-term" marginal cost concept between the short-term and long-term ones but in the explicit recognition of an asymmetry between plant expansion and plant contraction. Two reasons account for this. On the one hand, some fixed costs to be incurred in case of expansion cannot be recouped in case of contraction. On the other hand, the old production unit that would be abandoned in case of contraction<sup>46</sup> may be technically very different from the new unit that would be used in case of expansion. Hence, the (marginal) cost relevant

<sup>41</sup> This amounts to defining the optimum output of a given plant as that output for which that plant is optimum, in the long run.

<sup>42</sup> That is, for a plant whose total, average, and marginal cost curves become vertical at a finite output.

<sup>43</sup> The slope of the total cost curve for a given plant may also fail to be defined due to the presence of indivisible expenditures (a typical situation in railroading); let total expenditures increase by  $\Delta E$  whenever output increases by  $\Delta O$ ; if actual output may be regarded as a random variable, the indivisible expenditure may be treated as adding  $\Delta E/\Delta O$  to short-term marginal cost at every output—as shown by Hutter [70], I, p. 57.

<sup>44</sup> I am using here the word capacity as short cut for "size and design" when plant capacity is not rigid.

<sup>45</sup> This raises a problem for investment policy, not for pricing policy; indeed, the theory of sale at marginal cost is primarily concerned with the short term marginal cost for the existing plant; it has been convincingly argued by Boiteux [27], Section 2.3., that the desire to keep prices stable leads, when the plant is for some reason not optimal, to the principle of setting prices equivalent to what short-term marginal costs would be if the plant were constantly of optimum size and design.

<sup>46</sup> This is, of course, the least profitable ("marginal") unit, be it a machine, or plant, etc (For instance, a passenger car, or train, or track, etc.)

for an expansion may *in principle* be lower than as well as higher than or equal to the (marginal) cost relevant for a contraction decision.

It is easily shown that only the latter alternative (expansion cost not lower than contraction cost) is consistent with a rational investment policy. Indeed, if the savings brought about by disposing of an old production unit were to exceed the corresponding costs incurred in procuring (and using) a new unit, then the old unit should be discarded and replaced.<sup>47</sup> This would presumably lower (or leave unchanged) the contraction cost (since the unit just discarded was the least profitable one) and raise (or leave unchanged) the expansion cost in the absence of economies of scale (since the unit just added was the least costly one). This process should continue until no further substitution of new for old equipment can be profitable, i.e., until expansion cost becomes equal to (or higher than) contraction cost.<sup>48</sup>

When expansion cost is higher than contraction cost, middle-term marginal cost is indeterminate, and explicit reference must be made either to expansion cost or to contraction cost.<sup>49</sup> As will be seen, such reference is frequently called for in practical applications of the theory of marginal cost pricing as well as in investment policy.

### *Peak-load Pricing*

The marginal cost pricing problem for a non-storable commodity with periodic demand fluctuations<sup>50</sup> is a special case of the pricing problem for multiple products.<sup>51</sup> As such, it is one of the most natural extensions of the standard theory of marginal cost pricing. It is also one of the most important applications of the theory to the management of public utilities (transportation, mail, telecommunications, and power supply). Furthermore, it brings out neatly the interactions between pricing and investment policies—two topics that will be surveyed together here.<sup>52</sup>

<sup>47</sup> The savings and costs must, of course, be defined on an appropriate basis. Typically, the problem is not one of deciding whether or not an old unit should be replaced but rather *when* it should be replaced. Hence, "an appropriate basis" is frequently provided by a comparison of present versus future replacement. The timing of contraction raises difficult problems, some of which are touched upon below (under *Marginal Cost Pricing and Investment Policy*, pp. 24 ff.).

<sup>48</sup> For a graphical treatment of this point, see Lesourne [81], pp. 274–5 of the English edition.

<sup>49</sup> For a skillful application of the concepts to coal mining, see Lesourne [82]; several unpublished papers by Boiteux and Dessus [EDF documents PR3-PR9, 1950] deal with marginal costs for various techniques of electricity production.

<sup>50</sup> Periodic *supply* fluctuations (e.g., due to seasonality of rainfall), raise basically analogous problems; see, for instance, Boiteux [27], Section 4 of the French text.

<sup>51</sup> Indeed, the output at various points in time of a given plant provide a perfect example of multiple products; the analysis for a storable commodity retains this essential feature, but involves inventory fluctuations as well as additional intertemporal efficiency conditions; see, e.g., Mills [121].

<sup>52</sup> Some general comments about these interactions are given below.

The main points of the original study by Boiteux [27]<sup>53</sup> may be summed up somewhat informally as follows.

Let us consider first the simplest cost situation, namely, that defined by (1) constant returns to scale, (2) fixed plant capacity (see footnote 42, p. 9 above) and (3) short-term marginal costs that are constant and that do not depend upon plant size. It is readily verified that under these three assumptions the (short-term) total cost function, for a plant of capacity  $q_0$ , must be of the form:

$$(1) \quad f(q_0, q) = \begin{cases} \beta q_0 + \gamma q, & 0 \leq q \leq q_0 \\ \infty & q > q_0 \end{cases}$$

where  $q$  is output per unit of time;  $\beta$  is a "marginal capacity cost";  $\gamma$  is short-term marginal cost; and  $\beta + \gamma = \delta$  is long-term marginal cost (= expansion cost).<sup>54</sup> The long-term total cost function may be written in the form:

$$(2) \quad F(q) = f(q, q) = \delta q, \quad q \geq 0.$$

This cost situation is depicted in Figure 1, where the long-term total cost function  $F(q)$  is plotted together with a set of short-term total cost functions  $f(q_0, q)$  (drawn for different values of  $q_0$ ).

The unit of time must, for our purposes, be such that demand fluctuations *within* any given unit are either non-existent or irrelevant.<sup>55</sup> It is then convenient to start the analysis with a graph (Figure 2) describing the "load-curve" (quantity demanded during each unit of time) corresponding to a uniformly set price equal to  $\gamma$ , the short-term marginal cost. The load curve is established for a *period* of  $T$  *units* of time, the shortest period over which demand exhibits recurrent fluctuations.<sup>56</sup> Thus, according to Figure 2, if the price  $p(h)$  charged at (during unit of) time  $h$  is equal to  $\gamma$ , for  $h = 1 \dots T$ , i.e., for the whole period, then the quantity demanded at time  $h$  will be  $q(h)$ . It reaches a peak  $q^+ = q(H)$  at time  $H$ .

It follows that if  $p(h) = \gamma$ ,  $h = 1 \dots T$ , then the peak load will actually be  $q^+$  and the plant capacity required to meet demand is also  $q^+$ . Remembering that our total cost function is defined "per unit of time," the total cost *per period* will be:

<sup>53</sup> The same problem was studied subsequently and independently by Houkhakker [69], Steiner [154] and Hirschleifer [68].

<sup>54</sup> Indeed,  $f(q, q) = \delta q$  by (1);  $f(q_0, q) = \infty$ ,  $q > q_0$ , by (2);  $f(q_0, q) = \delta q_0 - \gamma(q_0 - q) = \beta q_0 + \gamma q$ ,  $0 \leq q \leq q_0$ , by (1) and (3).

<sup>55</sup> Thus, in electricity supply, the unit of time might be one hour, or even less.

<sup>56</sup> Thus, in electricity supply, this period might be one day; the daily load curve will, of course, shift with the day of the week and the season of the year, but this adds little analytical difficulty to our problem.

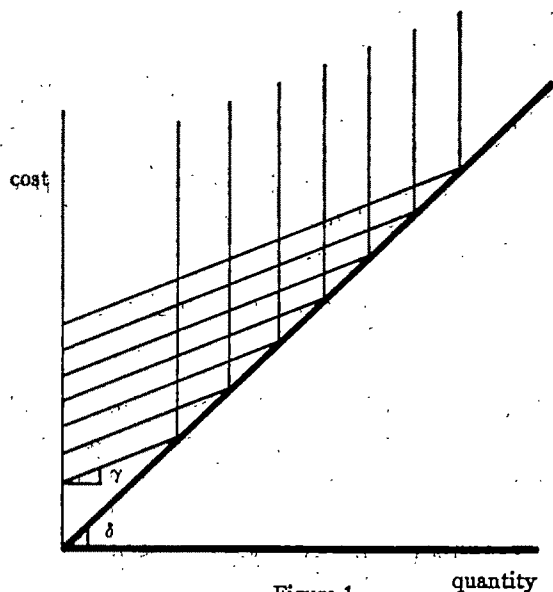


Figure 1

— long-term total cost curve  
 — short-term total cost curves for plants of given capacity

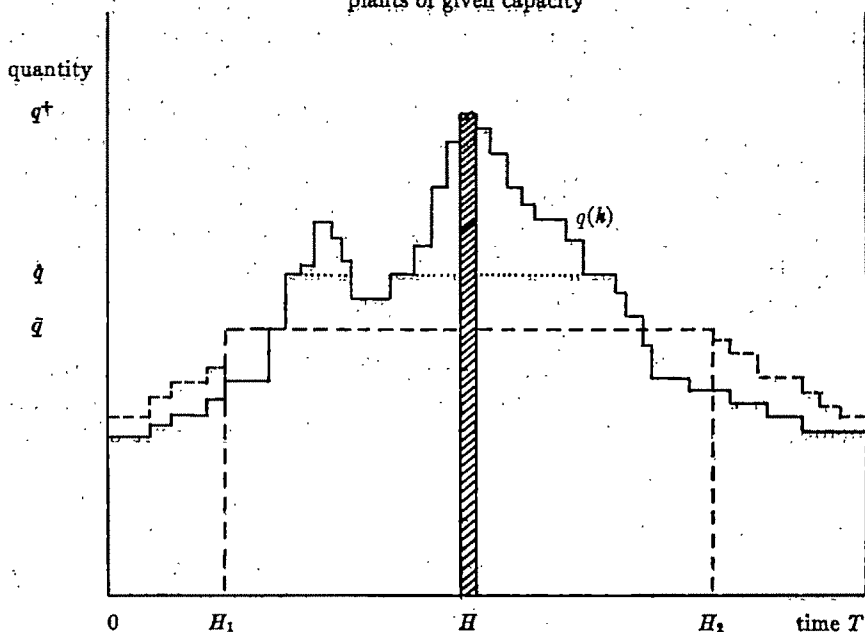


Figure 2

$$\begin{aligned}
 (3) \quad \sum_{h=1}^T f[q^+, q(h)] &= \sum_{h=1}^T [\beta q^+ + \gamma q(h)] = T\beta q^+ + \gamma \sum_{h=1}^T q(h) \\
 &= (T\beta + \gamma)q(H) + \gamma \sum_{\substack{h=1 \\ h \neq H}}^T q(h).
 \end{aligned}$$

Total revenue for the same period will be:

$$(4) \quad \sum_{h=1}^T p(h)q(h) = \gamma \sum_{h=1}^T q(h),$$

leaving a deficit in the amount  $T\beta q^+$  for the entire period (or  $\beta q^+$  per unit of time—where  $\beta q^+$  is also the “total capacity cost” per unit of time).

*If the quantities demanded were independent of the prices charged*

$$\left[ \frac{\partial q(h)}{\partial p(i)} = 0, h, i = 1 \dots T \right]$$

then marginal cost pricing would call for prices  $p(h) = \gamma$  whenever  $q(h) < q^+$ , i.e., for  $h = 1 \dots T$ ,  $h \neq H$ ; the price  $p(H)$  would simply be subject to the inequality  $p(H) \geq \gamma$  since short-term marginal cost is indeterminate at  $H$  [for  $q(H) = q^+$ ]. However, it is readily seen from (3) that the partial derivative of total cost for the period with respect to  $q(H)$  is  $T\beta + \gamma$ —and this obviates the indeterminacy of short-term marginal cost at  $H$ . Accordingly, if  $p(H) = T\beta + \gamma$ , peak demand will be priced at its marginal cost (= short-term marginal cost + marginal capacity cost), and total revenue will rise to

$$(T\beta + \gamma)q(H) + \gamma \sum_{\substack{h=1 \\ h \neq H}}^T q(h).$$

The firm will thus break even, as was to be expected under our assumption of constant returns to scale.

This last implication is, of course, irrelevant *per se*: it is not part of the conditions defining marginal cost pricing. What is more relevant is the fact that plant capacity is optimal, in the sense that the revenue associated *at market prices* with the last unit of capacity, namely  $T\beta + \gamma$ , is precisely equal to the marginal cost of keeping ( $T\beta$ ) and using ( $\gamma$ ) that same unit. Should *this* equality fail to hold, then plant capacity should be adjusted (upward if marginal revenue at market prices exceeds marginal cost, downward in the opposite case).<sup>57</sup> The fact that a break-even situation automatically results from optimal pricing *and* investment policies (under our special assumption about costs and de-

<sup>57</sup> Indeed, capacity is just another input, and its marginal revenue product should equal its marginal cost, at market prices. The marginal revenue (of capacity) at market prices is simply the sum of the prices, over those periods where the last unit of capacity is utilized.

mand) serves as a convenient way of checking the joint optimality of these policies (and possibly also as a convenient way of implementing them).

Now let the quantity demanded at time  $h$ ,  $h=1 \dots T$ , be a function say,  $q[h, p(h)]$ , of the price  $p(h)$  charged at that time, but not of the prices  $p(i)$ ,  $i \neq h$ , charged at other times. Under these conditions, if  $p(H) > \gamma$ , the quantity demanded at that time,  $q[H, p(H)]$ , will be less than  $q^+ = q[H, \gamma]$ . The crucial question to ask when  $p(H) = T\beta + \gamma$ , is the following: Is  $q[H, T\beta + \gamma]$  less or not less than the next highest demand—which, in the example of figure 2, is  $q(H+1) = q[H+1, \gamma]$ ? Indeed, if  $q[H, T\beta + \gamma] \geq q[H+1, \gamma]$ , then setting plant capacity at  $q[H, T\beta + \gamma]$  and setting the prices at levels  $p(h) = \gamma$ ,  $h \neq H$ ,  $p(H) = T\beta + \gamma$  still provides a solution, with prices equal to marginal costs, capacity at its optimal level and total costs equal to total revenue for the period at the new level:

$$(5) \quad \gamma \sum_{\substack{h=1 \\ h \neq H}}^T q(h) + (T\beta + \gamma)q[H, T\beta + \gamma].$$

However, if  $q[H, T\beta + \gamma] < q[H+1, \gamma]$ , then under the price structure stated above, a plant capacity of  $q[H+1, \gamma]$  is called for, and short-term marginal cost becomes indeterminate at  $H+1$  but falls back to  $\gamma$  at  $H$ ! The latter pitfall could be bypassed by setting  $p(H)$  at whatever level—say,  $p^*(H)$ ,  $T\beta + \gamma > p^*(H) > \gamma$ —is required in order to peg the quantity demanded at time  $H$  at the new level  $q(H+1)$  of plant-capacity. Indeed, short-term marginal cost would then become indeterminate both at time  $H+1$  and at time  $H$  since  $q[H, p^*(H)] = q(H+1)$  by construction. Total cost would now be equal to

$$(6) \quad T\beta q(H+1) + \gamma q[H, p^*(H)] + \gamma \sum_{\substack{h=1 \\ h \neq H}}^T q(h)$$

and would exceed total revenue by an amount  $[T\beta + \gamma - p^*(H)]q(H+1)$  since total revenue would now be equal to

$$(7) \quad p^*(H)q[H, p^*(H)] + \gamma \sum_{\substack{h=1 \\ h \neq H}}^T q(h).$$

From (6) and (7) we can see that only a partial solution to the problem has been reached; indeed, the revenue associated, at market prices, with the last unit of capacity<sup>58</sup>—namely,  $p^*(H) + \gamma$ —falls short of covering the marginal cost of keeping that same unit and operating it during the two units of time  $H$  and  $H+1$ —namely,  $T\beta + 2\gamma$ . Plant capacity is thus excessive.

<sup>58</sup> See footnote 57, p. 13.

As plant capacity is further reduced below  $q(H+1)$ , both  $p(H)$  and  $p(H+1)$  have to be adjusted upward so as to peg the quantities demanded at times  $H$  and  $H+1$  at the new level of plant capacity—and the resulting revenue at market prices of the last unit of capacity has to be compared with the marginal cost of keeping and operating that same unit. Clearly, successive reductions of plant capacity will be associated with increased revenues at market prices—since the prices at times  $H$  and  $H+1$  will have to be raised in order to peg demand there at successively lower levels. The relevant marginal cost, on the other hand, remains constant by assumption so that the margin between the two amounts narrows. If that margin fails to vanish before the plant capacity is cut back to the level  $q(H-1)$  (see Figure 2), then the *three* prices  $p(H-1)$ ,  $p(H)$  and  $p(H+1)$  will have to be raised further, and capacity cut further. The introduction of period  $H-1$  into the calculation [for its contribution  $p(H-1) \geq \gamma$  to revenue at market price and its contribution  $\gamma$  to the marginal cost of the last unit of capacity that is now used during a third unit of time] can only speed up the narrowing of the margin. Eventually a level of capacity, say,  $\hat{q}$  on Figure 2, will be reached, such that, when  $p(h) = \gamma$  for all  $h$  where  $q[h, \gamma] = q(h) < \hat{q}$ , and  $p(h) = \hat{p}(h) \geq \gamma$  for all  $h$  where  $q(h) \geq \hat{q}$ , with  $\hat{p}(h)$  such that  $q[h, \hat{p}(h)] = \hat{q}$ , then:

$$(8) \quad \sum_{h=1}^T \hat{p}(h) = T\beta + \sum_{h=1}^T \gamma$$

$h \ni q(h) \geq \hat{q}$   $h \ni q(h) \geq \hat{q}$

(8) expresses precisely the equality between the marginal cost of keeping and operating the last unit of capacity (right side) and the associated revenue at market prices (left side).

Total cost for the period is now equal to

$$(9) \quad T\beta\hat{q} + \gamma \sum_{h=1}^T q[h, \hat{p}(h)] + \gamma \sum_{h=1}^T q(h)$$

$h \ni q(h) \geq \hat{q}$   $h \ni q(h) < \hat{q}$

$$= \left[ T\beta + \sum_{h=1}^T \gamma \right] \hat{q} + \gamma \sum_{h=1}^T q(h).$$

$h \ni q(h) \geq \hat{q}$   $h \ni q(h) < \hat{q}$

The corresponding total revenue is equal to

$$(10) \quad \sum_{h=1}^T \hat{p}(h)q[h, \hat{p}(h)] + \gamma \sum_{h=1}^T q(h)$$

$h \ni q(h) \geq \hat{q}$   $h \ni q(h) < \hat{q}$

$$= \hat{q} \sum_{h=1}^T \hat{p}(h) + \gamma \sum_{h=1}^T q(h).$$

$h \ni q(h) \geq \hat{q}$   $h \ni q(h) < \hat{q}$

In view of (8), the firm will again break even—a condition that could be used directly to locate  $\hat{q}$  if one so desired. Furthermore, substituting from (8) into (9) yields (10) as an alternative expression for total cost. The partial derivatives of (10) with respect to  $q(h)$ ,  $h=1 \cdots T$ , are either  $\gamma$ =short-term marginal cost where  $q(h) < \hat{q}$ , or  $\hat{p}(h)$  when short-term marginal cost is indeterminate (because the plant is operated at full capacity). Thus, all the prices reflect marginal costs.

Should one wish to solve the pricing problem for *given* plant capacity, it would clearly suffice to set  $\hat{q}$  equal to that capacity, and again to adopt prices  $\hat{p}(h)=\gamma$  where  $q(h) < \hat{q}$ ,  $\hat{p}(h)=\hat{p}(h)$  such that  $q[h, \hat{p}(h)]=\hat{q}$  where  $q[h, \gamma] \geq \hat{q}$ . A comparison of the marginal cost and the marginal revenue of capacity—see (8)—would then reveal whether existing capacity is excessive or defective.

Finally, let the quantity demanded at time  $h$  be a function of all prices  $p(i)$ ,  $i=1 \cdots T$ . Repeating step by step the argument just outlined, we must simply add that when  $\hat{p}(H)$  is set at a level higher than  $\gamma$ , not only will the quantity demanded at time  $H$  decrease, but the quantities demanded at other times will increase (decrease) by substitution (complementarity)<sup>69</sup> relative to the levels given in Figure 2 for  $\hat{p}(h)=\gamma$ ,  $h=1 \cdots T$ . The process of searching for a solution [prices  $\hat{p}(h)$  and capacity  $\hat{q}$ ] by successive approximations is not fundamentally different—only somewhat more laborious—to the extent that prices for different periods have to be adjusted *simultaneously* in order to peg demand during the peak interval (that now extends from  $H_1$  to  $H_2$  on Figure 2<sup>69a</sup>) at the constant level  $\hat{q}$ . The properties of the solution, however, are unchanged—prices are set equal to  $\gamma$  outside of the peak interval; the set of prices  $\hat{p}(h)$ ,  $h=H_1 \cdots H_2$  must be such as to jointly peg the demand at the constant level  $\hat{q}$  over that interval *and* below the level  $\hat{q}$  outside of that interval. The level  $\hat{q}$  of plant capacity is still determined by the condition (8) that now reads:

$$(11) \quad \sum_{h=H_1}^{H_2} \hat{p}(h) = T\beta + \sum_{h=H_1}^{H_2} \gamma.$$

This condition still implies that total cost for the period should be equal to total revenue. The interpretation of the prices  $\hat{p}(h)$  as marginal costs is unchanged; the pricing problem for *given* plant capacity would again be solved by setting  $\hat{q}$  equal to existing capacity.

In summary, peak-load pricing must be such as to both *depress* and *spread* the peak, while equating the marginal revenue at market prices and the marginal cost of keeping and using the last unit of capacity. Under our simple cost structure, the last condition automatically brings about a break-even situation. In principle, the pricing policy that meets

<sup>69</sup> Substitution will, of course, be the rule, complementarity the exception.

<sup>69a</sup> In general, the peak interval need not be continuous.

these conditions is determinate—there are as many conditions imposed upon the unknown prices and capacity as there are unknowns to determine—but no systematic analysis of existence and uniqueness of the solution has ever been published, to the best of my knowledge.<sup>60</sup>

Let us now consider briefly the consequences of relaxing our restrictive assumptions about costs. When plants do not have a fixed capacity, short-term marginal cost is well defined at all outputs, and prices  $p(h)$  must be equal to short-term marginal costs for all  $h$ . Accordingly, it will be typically impossible to keep output constant during a peak interval<sup>61</sup> and prices constant outside of that interval.<sup>62</sup> On the other hand, peaks may now be handled by overloading the plant so long as it is economical to do so, as well as by adjusting its size and design.

Although this need not complicate the pricing problem for a *given* plant size and design, it does complicate substantially the investment problem. The conditions of optimal investment can still be worked out mathematically, of course, but their economic interpretation and the accompanying rules of empirical implementation and verification become somewhat cumbersome. A special case of real practical interest<sup>63</sup> has been worked out by Boiteux. Let the total cost curves  $f(q_0, q)$  have the "congruence" property that short-term marginal costs are the same: (1) at output  $q$  for a plant with optimum output  $q_0$ ,<sup>64</sup> and (2) at output  $q+\epsilon$  for a plant with optimum output  $q_0+\epsilon$ ; and this for all  $q$  over the relevant range [that is,

$$\left. \frac{\partial f(q_0, q)}{\partial q} \right|_{\hat{q}} = \left. \frac{\partial f(q_0 + \epsilon, q)}{\partial q} \right|_{\hat{q} + \epsilon},$$

identically in  $\hat{q}$ ]. It can then be shown (cf. Boiteux [27], Mathematical Appendix) that the conditions of optimal investment require that *long-term marginal cost be equal to the average price* (= short-term marginal cost) *charged over the period*

$$\left[ \text{that is, } \left. \frac{\partial f(q_0, q)}{\partial q_0} \right|_{q=q_0} \text{ def } \left. \frac{\partial f(q_0, q)}{\partial q} \right|_{q=q_0} = \frac{1}{T} \sum_{h=1}^T p(h) \right].^{65}$$

This condition is, of course, very convenient for practical applications and it has been systematically used at EDF.<sup>66,67</sup>

<sup>60</sup> The subsequent studies [68, 69, 154] do not provide such an analysis either.

<sup>61</sup> A result that has come about so far because of the fixed capacity assumption.

<sup>62</sup> A result that has come about so far because of the constant short-term marginal cost assumption.

<sup>63</sup> At least in electricity supply—see footnote 67.

<sup>64</sup> See footnote 41 p. 9.

<sup>65</sup> In the general case, long-term marginal cost is a *weighted* average of the prices—the weights being functions of second-order partial derivatives of the cost function as well as of the elasticities and cross-elasticities of demand.

<sup>66</sup> See below under *Applications*, p. 34.

<sup>67</sup> In practice, the "congruence" assumption needs to hold only at the margin of the equip-

*Pricing under Stochastic Demand Conditions*<sup>68</sup>

Short-run fluctuations in demand occur frequently in random as well as in periodic patterns. In the case of a non-storable commodity,<sup>69</sup> the pricing problem raised by random fluctuations in demand is quite different from the problem of peak-load pricing. One might expect that, when the life of the equipment is long relative to the period over which demand exhibits significant fluctuations (around a known average), investment decisions would be guided by the same principles—no matter whether the fluctuations be random or periodic. As we shall see, this is not always so. As for pricing decisions, the basic difference is the following: peak-load pricing calls for a *known* periodic schedule of prices, whereas pricing at short-run marginal cost under stochastic demand conditions would call for *stochastic* price variations.

Stochastic short-run price variations are frequently ruled out on economic, administrative or legal grounds—and such has almost invariably been the case in the public utilities field. The combination of short-run price rigidity and short-run random fluctuations in demand must then result in a combination of (1) some form of demand rationing;<sup>70</sup> and (2) short-run fluctuations in output, to be met either by overloading a plant of flexible capacity or by building an adequate safety margin into a plant of fixed capacity. Both of these consequences are costly—either to the consumers or to the producers. Whenever consumers can reduce the amplitude of the short-run random fluctuations of their demand, it would obviously be desirable that the price structure induce them to do so. The question thus arises: Can some marginal cost (or “welfare loss”) be attached to the random variations of an *individual* consumer’s demand? If so, can a *non-stochastic* form of price discrimination reflect that marginal cost?

A satisfactory answer to that question is still missing, for lack of a workable extension to uncertainty situations of the theories of efficiency

ment. What matters is that additional investment should result in a *horizontal displacement* of the short-term marginal cost curve. Such would automatically be the case if the investment took the form of adding to the equipment a new unit of fixed capacity with low short-term marginal cost (so that it would be fully used throughout the period), or again, if the investment took the form of replacing some of several “congruent” old production units by more modern equipment. See the graphical illustration given by Boiteux, [34], p. 10, or [37], p. 92.

<sup>68</sup> Stochastic *supply* conditions raise related, but substantially different, pricing problems. See the brief comment of Boiteux [31], p. 226 and [37], p. 87.

I am grateful to Peter Pashigian for a very helpful discussion of the issues discussed under this heading.

<sup>69</sup> In the case of a storable commodity, such fluctuations will typically result in a combination of inventory, output and price variations—see, e.g., Mills [121].

<sup>70</sup> For example, queuing, priorities, quality deterioration, and so on.

and Pareto optimality.<sup>71</sup> Such an extension would indeed be needed in order to specify the *kind* of market prices that could bring about an efficient allocation of resources.<sup>72,73</sup>

Using a more pedestrian approach, Boiteux [29] has sketched a partial answer to our problem. His analysis rests upon the following assumptions: (a) the (short-term) total cost function, for a plant of capacity  $q_0$ , is of the form:<sup>74</sup>

$$(12) \quad f(q_0, q) = \begin{cases} \beta q_0 + \gamma q, & 0 \leq q \leq q_0 \\ \infty & q > q_0; \end{cases}$$

(b) there are a large number ( $N$ ) of individual consumers, each characterized by a demand (at a given time and price)  $q_i$ , a random variable with known expected value  $\bar{q}_i$  and (finite) variance  $\sigma_i^2$ ,  $i=1 \dots N$ . These random variables are "sufficiently analogously and independently distributed" (given the other elements of the problem) for aggregate demand to be approximately normally distributed with expected value

$$\bar{q} = \sum_{i=1}^N \bar{q}_i \quad \text{and variance} \quad \sigma^2 = \sum_{i=1}^N \sigma_i^2. \quad 75$$

Under these assumptions, the probability that aggregate demand will exceed a given level, say  $\bar{q} + k\sigma$ , can be read from a table of the standardized normal density function; conversely, to any positive probability  $\epsilon$  there corresponds a level of aggregate demand  $q(\epsilon)$  having probability  $1-\epsilon$  of not being exceeded, namely,  $\bar{q} + k(\epsilon)\sigma$ , where  $k(\epsilon)$  can be read from the same table.<sup>76</sup> Accordingly, *if a public utility were to operate under the constraint that it should be able to meet aggregate demand with probability  $1-\epsilon$ ,*<sup>76a</sup> it should then build a plant of capacity  $q_c(\epsilon)$ ,

<sup>71</sup> The references mentioned in footnote 37 above go some way toward providing a theoretical solution to this problem, but much remains to be done at the theoretical level and almost everything remains to be done at the level of applications.

<sup>72</sup> Two distinct allocation problems are at stake, namely, the allocation of *inputs* between this and other industries, and the allocation of *output* among consumers; the two problems are, of course, related.

<sup>73</sup> On this point, see the discussion by Allais, de Finetti, Kreweras, Marshack and Wold (pp. 227-230 in CNRS [43]) of some suggestions made by Boiteux [31]. Among other things, Boiteux suggests that each consumer might specify with what probability he expects his demand to be met, and what loss would result to him from lack of service; the producer would quote his price as a function of both the probability of service he is willing to guarantee and the penalty he would pay in case of shortage; why a public utility should sell insurance (or gambles!) as well as electricity is not altogether clear to me.

<sup>74</sup> This cost function was introduced above in equation (1) p. 11.

<sup>75</sup> This assumption involves reliance upon the "Central Limit Theorem"; Boiteux's paper does not develop or discuss formally the exact conditions imposed upon the individual demands.

<sup>76</sup> Thus,  $k(.025) = 1.96$ ,  $k(.005) = 2.58$ , etc.

<sup>76a</sup> The relaxation of that constraint is considered below.

and its total cost function would be, for outputs not exceeding  $q_o(\epsilon)$   
 $= \bar{q} + k(\epsilon)\sigma$ ;

$$\begin{aligned}
 f[\bar{q}(\epsilon), q] &= \beta q_o(\epsilon) + \gamma q \\
 &= \beta[\bar{q} + k(\epsilon)\sigma] + \gamma q \\
 (13) \quad &= \beta \left[ \sum_{i=1}^N \bar{q}_i + k(\epsilon) \left( \sum_{i=1}^N \sigma_i^2 \right)^{1/2} \right] + \gamma \sum_{i=1}^N q_i^*
 \end{aligned}$$

where  $q_i^*$  is the quantity actually sold to the  $i$ -th consumer. Thus,

$$q_i^* = q_i \text{ for all } i \text{ if and only if } \sum_{i=1}^N q_i \leq q_o(\epsilon).$$

Since no specific policy for handling shortages is considered, I shall assume that actual sales are proportional to individual demands, the factor of proportionality being 1 in the absence of shortage, and  $q_o/q$  in case of shortage.<sup>77</sup>

Total costs are thus a function of sales  $q_i^*$  and of two parameters of the density functions of individual demands—their means  $\bar{q}_i$  and either their variances  $\sigma_i^2$  or their standard deviations  $\sigma_i$ . Furthermore, it is readily verified that total costs are given by a non-linear function, homogeneous of the first degree in the quantities  $\bar{q}_i$ ,  $\sigma_i$  and  $q_i^*$ . The partial derivatives of (13), or “marginal costs,” are:

$$(14) \quad \frac{\partial f}{\partial \bar{q}_i} = \beta, \quad \frac{\partial f}{\partial \sigma_i^2} = \frac{\beta}{2} \frac{k(\epsilon)}{\sigma}, \quad \frac{\partial f}{\partial \sigma_i} = 2\sigma_i \frac{\partial f}{\partial \sigma_i^2} = \beta k(\epsilon) \frac{\sigma_i}{\sigma}, \quad \frac{\partial f}{\partial q_i^*} = \gamma.$$

Accordingly, if actual sales, expected demand and the *standard deviation* of demand are priced at marginal cost, consumer  $i$  should be charged an amount (incur an expenditure  $E_i$ ) equal to

$$(15) \quad q_i^* \frac{\partial f}{\partial q_i^*} + \bar{q}_i \frac{\partial f}{\partial \bar{q}_i} + \sigma_i \frac{\partial f}{\partial \sigma_i} = \gamma q_i^* + \beta \bar{q}_i + \beta k(\epsilon) \frac{\sigma_i^2}{\sigma} = E_i(q_i^*, \bar{q}_i, \sigma_i).$$

The total revenue to the utility is then

$$(16) \quad \gamma \sum_{i=1}^T q_i^* + \beta \sum_i \bar{q}_i + \beta \frac{k(\epsilon)}{\sigma} \sum_{i=1}^T \sigma_i^2 = \gamma q + \beta[\bar{q} + k(\epsilon)\sigma].$$

The utility will thus exactly break even—a result that is perhaps not surprising, under homogeneity of the first degree of the total cost function.

What happens if actual sales, expected demand and the *variance* of

<sup>77</sup> Actually, in the case of electricity supply, a shortage may take the form of a quality differential (lower tension) affecting all customers indiscriminately and entailing no measured discrepancy between the quantity sold and the quantity demanded.

demand are priced at marginal cost? From (14) we see that the  $i$ -th consumer should then be charged an amount equal to:

$$(17) \quad q_i^* \frac{\partial f}{\partial q_i^*} + \bar{q}_i \frac{\partial f}{\partial \bar{q}_i} + \sigma_i^2 \frac{\partial f}{\partial \sigma_i^2} = \gamma q_i^* + \beta \bar{q}_i + \frac{\beta}{2} k(\epsilon) \frac{\sigma_i^2}{\sigma} = E_i(q_i^*, \bar{q}_i, \sigma_i^2) \\ \leq E_i(q_i^*, \bar{q}_i, \sigma_i)$$

so that the utility's total revenue shrinks to:

$$(18) \quad \gamma \sum_{i=1}^N q_i^* + \beta \sum_{i=1}^N \bar{q}_i + \frac{\beta}{2} \frac{k(\epsilon)}{\sigma} \sum_{i=1}^N \sigma_i^2 = \gamma q + \beta \left[ \bar{q} + \frac{k(\epsilon)\sigma}{2} \right]$$

A deficit amounting to  $\frac{1}{2}\beta k(\epsilon)\sigma$  must now be faced: under marginal cost pricing of  $q_i^*$ ,  $\bar{q}_i$  and  $\sigma_i^2$  only half of the expenditures brought about by the excess capacity margin  $k(\epsilon)\sigma$  are covered.

This situation may seem to involve an element of ambiguity; indeed, it looks as if the *principle* of marginal cost pricing leaves room for indeterminacy when we come to *applications*. The ambiguity might be obviated by claiming that the combination of marginal cost pricing *and* constant returns to scale should result in a break-even situation—so that (15) and not (17) provides the right pricing formula. Whatever the reasons, this is also the formula advocated by Boiteux [29], Boiteux and P. Stasi [39] or F. Bessière [25] and applied by EDF (see, e.g., Boiteux [37]).

Surprisingly enough, a closer look at the problem reveals this choice to be erroneous. This can be seen as follows. From the marginal costs given under (14), we may compute the marginal rates of transformation for the producer:

$$(19) \quad \frac{\partial f / \partial \sigma_i}{\partial f / \partial \bar{q}_i} = - \frac{d\bar{q}_i}{d\sigma_i} \bigg|_{f=\text{const.}} = \frac{k(\epsilon)\sigma_i}{\sigma}, \\ \frac{\partial f / \partial \sigma_i^2}{\partial f / \partial \bar{q}_i} = - \frac{d\bar{q}_i}{d\sigma_i^2} \bigg|_{f=\text{const.}} = \frac{k(\epsilon)}{2\sigma}$$

Similarly, from the price formula (15) we may compute the marginal rates of substitution for the  $i$ -th consumer:

$$(20) \quad \frac{\partial E_i(q_i^*, \bar{q}_i, \sigma_i) / \partial \sigma_i}{\partial E_i(q_i^*, \bar{q}_i, \sigma_i) / \partial \bar{q}_i} = - \frac{d\bar{q}_i}{d\sigma_i} \bigg|_{E_i(\bar{q}_i^*, \bar{q}_i, \sigma_i) = \text{const.}} = \frac{2k(\epsilon)\sigma_i}{\sigma}, \\ \frac{\partial E_i(q_i^*, \bar{q}_i, \sigma_i) / \partial \sigma_i^2}{\partial E_i(q_i^*, \bar{q}_i, \sigma_i) / \partial \bar{q}_i} = - \frac{d\bar{q}_i}{d\sigma_i^2} \bigg|_{E_i(\bar{q}_i^*, \bar{q}_i, \sigma_i) = \text{const.}} = \frac{k(\epsilon)}{\sigma}.$$

Comparing the results under (19) and (20), we notice that *marginal cost pricing of sales, expected (or average) demand and the standard deviation of demand does not bring about equality between the marginal rates of*

*transformation and of substitution*—so that consumers will not substitute mean for variance (standard deviation) of their demands until their marginal rate of substitution becomes equal to the ratio of marginal costs. Thus, this pricing formula fails to accomplish a major goal of marginal cost pricing (efficient allocation of resources).

However, if we use price formula (17), the marginal rates of substitution are equal to the marginal rates of transformation—as the reader can easily verify—so that *marginal cost pricing of sales, expected demand and variance of demand leads consumers to substitute mean for variance until their marginal rate of substitution becomes equal to the marginal rate of transformation*, as desired. This is accordingly the correct application of the marginalist principles.

The appearance of a deficit under the correct pricing policy may be explained as follows. Under constant returns to scale in production, the *average* cost of meeting each individual demand with preassigned probability *decreases* when two groups of consumers, whose demands are *imperfectly correlated*,<sup>78</sup> may be considered as a single group for the purpose of planning excess capacity.<sup>79</sup> Indeed, denoting the standard deviations of the two groups by  $\sigma_1$  and  $\sigma_2$ , respectively, the covariance between the two groups by  $\sigma_{12}$  and the overall standard deviation by  $\sigma$ , we know that  $\sigma = (\sigma_1^2 + \sigma_2^2 + 2\sigma_{12})^{1/2} \leq \sigma_1 + \sigma_2$ , with equality arising if and only if  $\sigma_{12} = \sigma_1\sigma_2$  (or  $\rho_{q_1q_2} = 1$ ). Accordingly, the capacity level required to serve (with given probability of shortage) the overall group, namely,  $\bar{q} + k\sigma$ , is less than the sum of the corresponding capacity levels for the individual groups, namely,  $(\bar{q}_1 + k\sigma_1) + (\bar{q}_2 + k\sigma_2) = \bar{q} + k(\sigma_1 + \sigma_2)$ , whenever  $\rho_{q_1q_2} < 1$ . We thus see that *uncertainty about demand will typically transform* (technologically) *constant returns to scale into* (economically) *increasing returns to scale*<sup>80,81</sup> This is one more difficulty to be faced in extending general equilibrium analysis to uncertainty situations.

In order to complete the explanation, there remains only to point out that marginal cost pricing of  $q_i^*$ ,  $\bar{q}_i$  and  $\sigma_i$  results in a break-even situation (in spite of the implicit presence of increasing returns to scale) because it amounts to *price-discrimination*. Indeed, the standard deviations of individual demands are priced at a rate proportional to  $\sigma_i$  [see (14) and (15)], that is, at a rate that varies from consumer to con-

<sup>78</sup> When demands are not independent, the pricing problem is somewhat more complicated, but the reasoning in the text is still valid.

<sup>79</sup> In other words, when these two groups of consumers may be served from the same, as opposed from distinct, facilities.

<sup>80</sup> For another example of this phenomenon, see Manne [98].

<sup>81</sup> When demands are perfectly negatively correlated, constant returns to scale (at a lower average cost) may be restored—provided the variances of the two groups are equal. This limiting case is rather uninteresting but forces one to speak of “imperfectly correlated” demands instead of the more specific “not perfectly positively correlated” demands.

sumer.<sup>82</sup> There is, of course, no difficulty in covering costs under increasing returns to scale when this kind of price discrimination is regarded as an admissible policy.

In practice, the utility may neglect the discrepancy between the quantity demanded  $q_i$  and the quantity actually delivered  $q_i^*$ ,<sup>83</sup> and measure  $\bar{q}_i$  and  $\sigma^2_i$  by the average and variance of an individual consumer's demand over time. To these two parameters will correspond respectively the price parameters

$$\delta = \beta + \gamma \quad \text{and} \quad \zeta = \frac{\beta k(\epsilon)}{2\sigma}.$$

For any given  $\epsilon$ , the unknowns  $q_\epsilon$ ,  $\bar{q}$  and  $\sigma$  will then be determined simultaneously by the equation:

$$(21) \quad q_\epsilon = \bar{q} + k(\epsilon)\sigma, \quad \bar{q} = \sum_{i=1}^N \bar{q}_i(\delta, \zeta, \epsilon), \quad \sigma = \left[ \sum_{i=1}^N \sigma_i^2(\delta, \zeta, \epsilon) \right]^{1/2}.$$

Once more, questions of existence and uniqueness of the solution remain to be examined. In principle, a solution may exist for any  $\epsilon$ , so that  $\epsilon$  must be determined on independent grounds. Indeed, a reduction in  $\epsilon$  may be viewed as an improvement of the quality of the product. Since by construction only half the cost of the improvement will be borne by customers, the profitability of the required investments cannot serve as a guide here. A market solution would probably exist if the probability of shortage  $\epsilon$  could be varied [together with the price  $\zeta(\epsilon)$  attached to variance] from one individual to the next.<sup>84</sup> Short of achieving such flexibility, some estimate of the consumer's surplus associated with variations of  $\epsilon$  would be needed in order to choose a probability of shortage.<sup>85</sup>

Unsatisfactory as the treatment of this problem may be, it has at least the merit of bringing into sharper focus some very real difficulties that

<sup>82</sup> By contrast, it should be remembered that peak-load pricing results in prices that vary over time but apply identically to all consumers. See also the comments on this point by Hirschleifer [68]. This particular form of price discrimination also explains why the marginal rate of substitution in (20) differs from the price ratio.

<sup>83</sup> As was pointed out in footnote 77, this discrepancy may be difficult to measure; it will at any rate be small when  $\epsilon$  is itself small.

<sup>84</sup> Some discussion of that problem is offered by Boiteux [29], but the general problem remains to be attacked.

<sup>85</sup> The same indeterminacy would arise if the problem were stated in terms of a fixed plant capacity  $q_\epsilon$ —a statement that might be appropriate for short-term analysis. Let a unit of capacity be valued at the "shadow-price"  $\hat{\beta}$ , so that the parameters of the price policy become

$$\delta = \hat{\beta} + \gamma, \quad \zeta = \frac{\hat{\beta} k(\epsilon)}{2\sigma}$$

and the parameters of aggregate demand become  $\bar{q}(\delta, \zeta, \epsilon)$ ,  $\sigma(\delta, \zeta, \epsilon)$ ; for any arbitrary value of  $\epsilon$  one should now investigate the existence (and uniqueness) of a value  $\hat{\beta}$  such that  $\bar{q}(\delta, \zeta, \epsilon) + k(\epsilon) \sigma(\delta, \zeta, \epsilon) = q_\epsilon$ . In principle, a solution may exist for any  $\epsilon$ —so that fixing plant capacity does not eliminate the indeterminacy of the quality parameter.

arise when one attempts to apply marginal analysis to uncertainty situations. The added difficulties connected with increasing returns are discussed further under the next two headings.

*Marginal Cost Pricing, Budget Balancing and Investment Policy*

When marginal costs are decreasing, as for instance in railroading, marginal cost pricing inevitably leads to a deficit. Yet the condition of budget balancing has also been advocated by Boiteux [28] as implying an optimal *disinvestment* policy for the railroad industry. The argument runs as follows:

Consider a particular railroad line, with given existing facilities, that is subject to competition from road carriers. It simplifies exposition, without significantly altering the nature of the problem, to assume that the supply of road transportation services is perfectly elastic at the price  $p^{86}$  equal to minimum long-run average cost for road carriers. Accordingly,  $p$  provides an upper limit for the prices to be charged, and marginal costs to be incurred, by the railroad. Let furthermore the demand for transportation services at price  $p$  generate a total revenue per unit of time in the amount  $\gamma$ —the assumption that revenue is constant over time, entailing again no significant loss of generality.

If the transportation services demanded at price  $p$  can be supplied by the railroad at a *full* cost, including adequate replacement charges on *all* its equipment (tracks, stations, rolling stock, . . .) that does not exceed  $\gamma$  per unit of time, then it will be economical to operate the railroad indefinitely. Road transportation will then come in only when the railroad is saturated.<sup>87</sup>

If, on the other hand, the full cost to the railroad exceeds  $\gamma$  per unit of time, but the variable cost (excluding replacement charges) does not, then the line will ultimately have to be abandoned, and the traffic diverted to road carriers. In that case, *some but not all* replacements of railroad equipment are economical (e.g., the costs of replacing the rolling stock periodically can be absorbed, but not those of replacing the tracks). Two questions must then be answered. What replacements are economical? When should the line be abandoned? Clearly, these two questions must be answered simultaneously—and the answers will also depend upon the pricing policy adopted by the railroad while in operation. Again, it will simplify exposition without altering the nature of the problem if we assume that prices will be set at the level  $p$  immedi-

<sup>86</sup>  $p$  is typically a *vector* of prices for differentiated services (first and second class passenger transportation, a schedule of commodity rates, etc.); whenever price, or  $p$ , is mentioned in the text, a vector interpretation is implicit.

<sup>87</sup> Alternatively, road services may be introduced for different kinds of traffic, excluded from the present analysis.

ately. This policy will typically maximize revenue for the railroad<sup>88</sup> while providing its customers with information about the long-run equilibrium price level.<sup>89</sup>

We may now compute, for *each* future date  $T$ , the *present* value of the *minimum* cumulated total cost  $M_T$  of keeping the railroad in operation until  $T$  and not later. Thus consider first the value of time 0,  $V_0$ , of the equipment in use at that time;  $V_0$  is an opportunity cost equal to the net savings that would accrue to the railroad from using some of that equipment elsewhere, plus the resale value on the market of the equipment for which the railroad would have no economical use elsewhere. Next consider the minimum costs of operation, at the traffic level corresponding to  $p$ , during periods  $t=1 \cdots T$ , say,  $\delta_t(T)$ ;<sup>90</sup> these costs include appropriate replacement charges for all those pieces of equipment that it will *pay to replace knowing that the line is to be abandoned at  $T$* .<sup>91</sup> The dependence of the replacement decisions upon the date of abandonment is, of course, the reason for expressing these costs as a function of  $T$ . Finally, let  $V_T$  stand for the value of the equipment (old and new) at time  $T$ . Then:

$$(22) \quad M_T = V_0 - \frac{V_T}{(1+i)^T} + \sum_{t=1}^T \frac{\delta_t(T)}{(1+i)^t}.$$

where  $i$  is the appropriate discount factor.<sup>92</sup>

This expression may be compared (for given  $T$ ) with the alternative cost, namely, the present value of the cumulated costs of an equal amount of road transportation services. Under our assumptions, that cost is also the revenue to the railroad, say  $R_T$ , if it charges the price  $p$  from now on.  $R_T$  is thus defined as follows:

$$(23) \quad R_T = \sum_{t=1}^T \frac{\gamma}{(1+i)^t}$$

In Figure 3, both  $M_T$  and  $R_T$  are graphed—as functions of  $T$ —so as to reflect a situation where the *full* cost to the railroad exceeds  $\gamma$  per unit of time (the asymptotic value of  $M_T$  exceeds that of  $R_T$ ).

Boiteux argues that the railroad should be kept in operation until, but not beyond, the time  $\theta$  at which  $M_\theta = R_\theta$ . The fact that, for any  $T < \theta$ ,

<sup>88</sup> The demand curve for the railroad has a kink at  $p$ —in view of road competition.

<sup>89</sup> This is the more relevant information for location decisions, for instance.

<sup>90</sup> Thus, the  $t$ -th period runs from time  $t-1$  until time  $t$ .

<sup>91</sup> And given their opportunity or market value at  $T$ ; such replacements may be regarded as “defensive investments.” See Lamfallussy, [77].

<sup>92</sup> What discount factor should be used by the nationalized industries has been the subject of much thought and debate. See, e.g., the discussion by Allais and Boiteux, pp. 86 and 98 in CNRS [46], and the treatment by Massé [112].

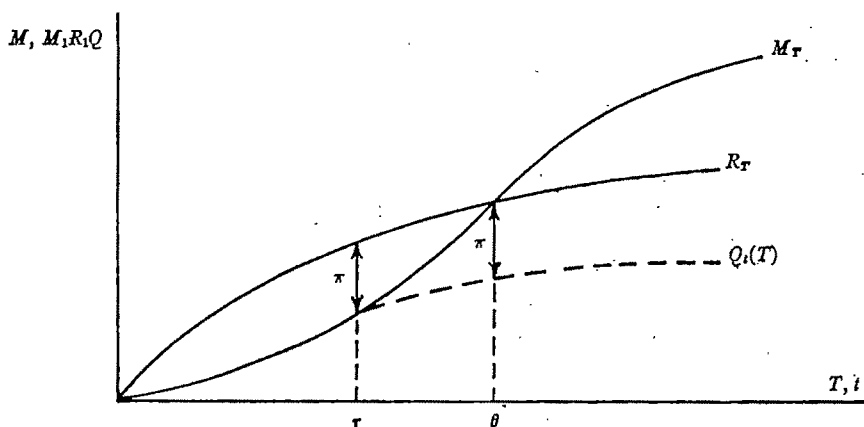


FIGURE 3

$M_T < R_T$  is taken as an indication that the railroad is more economical than road transportation there—and conversely for  $T > \theta$ . Since the total revenue to the railroad, at prices kept equal to  $p$  over the period  $0-\theta$ , has a present value  $R_\theta$ , it follows that abandoning the line at time  $\theta$  is financially equivalent to abandoning it immediately. If the condition is imposed on the railroad to break even, insofar as this particular line is concerned,<sup>83</sup> with a capital charge  $V_0$ , it has no choice but to *decide now* to abandon the line either immediately or at time  $\theta$ . Hence the break-even condition, it is claimed, leads to an optimal disinvestment policy.

The conclusion would certainly be correct if it were indeed so that  $\theta$  is the optimal time at which to abandon the line. However, such is not the case: the optimal time,  $\tau$ , is not defined by the condition  $M_T = R_T$  but rather by the condition

$$(24) \quad \frac{\partial M_T}{\partial T} = \frac{\partial R_T}{\partial T},$$

Indeed, define

$$(25) \quad Q_i(T) = M_T + R_i - R_T = M_T + \gamma \sum_{s=T+1}^i \frac{1}{(1+i)^s}, \quad i > T$$

as the total cost to the community of providing the transportation services demanded at prices  $p$  up to time  $i$ , if the railroad is kept in operation until the time  $T$  (at which road transportation takes over). Minimizing  $Q_i(T)$  with respect to  $T$  calls for condition (24), the eco-

<sup>83</sup> Boiteux discusses the relation between breaking even in the economic sense under discussion here and in the accounting sense. The difference between "economic" and "accounting" costs can be sizeable when long-lived investments are involved and the country is experiencing severe inflation.

nomic interpretation of which is that the railroad should be kept in operation so long as the *marginal* cost of extending its life by one period is less than the *alternative* cost of switching to road transportation. (By contrast, Boiteux compares the *total* cost of operating the railroad to its alternative.) In this sense optimal disinvestment policy is still governed by marginalist principles<sup>94</sup>—but it does not entail breaking even.

Indeed, the present value of total revenue up to time  $\tau$ ,  $R_\tau$ , exceeds the present value of total cost,  $M_\tau$ , by an amount  $\pi = \text{MAX}_t (R_t - M_t)$ . Thus, the current value of the equipment to the railroad may be set equal to

$$(26) \quad V_0 + \pi = R_\tau - \sum_{t=1}^{\tau} \frac{\delta_t(\tau)}{(1+i)^t} + \frac{V_\tau}{(1+i)^\tau},$$

which is its value under optimal use. To say that this particular line would break even if the equipment were currently valued at  $V_0 + \pi$  would, of course, beg the question.

What pricing policy should be adopted under condition of budget balancing will be reviewed next.

#### *Welfare Pricing under a Budget Balancing Constraint*

What should a public utility do when marginal cost pricing leads to a deficit but the utility is under legal obligation to balance its budget?<sup>95</sup> Several solutions to that problem have been advocated. Prior to World War II, the pricing policy of the French railroads was based upon the principle of price discrimination advocated by Colson [42]: tariffs *ad valorem* were the rule—this being supposedly equivalent to taxing inelastic demands and subsidizing elastic demands—except when the pressure from other means of transportation imposed competitive prices. Reacting against this practice, Allais advocated in his early transportation study [5] that all prices should be *proportional to marginal costs* irrespective of demand elasticities. The factor of proportionality would be so chosen as to bring about a break-even situation. This solution was presented by Allais as a “first approximation” to a “lesser evil.” Indeed, he indicated a theoretical preference for the “Hotelling solution,” i.e., marginal cost pricing and a government subsidy equal to the deficit; but he declared that solution psychologically (i.e., politically?) un-

<sup>94</sup> The equipment will indeed be used until marginal cost is equal to marginal revenue at market price—a situation that obtains at time  $t$ ; “marginal” is defined here in terms of time periods; that is, the unit of quantity is one time-period’s output.

<sup>95</sup> The analysis in this section applies also to the case where marginal cost pricing leads to a positive profit, but the utility is under legal obligation to pass its profits on to consumers via price reductions. It is not altogether clear that second-order conditions would be satisfied in both cases—a problem that remains to be investigated.

feasible and economically inadvisable—for lack of appropriate incentives to cost minimization in the absence of a budget restriction. The main argument put forward by Allais in favor of this proportionality rule was ease of application.<sup>96</sup> This, and the alleged advantages of keeping the ratio of two prices equal to the ratio of the corresponding marginal costs, is also the reason why he continued at a later date to prefer the principle of proportionality (of prices to marginal costs) to the following alternative solution which he then regarded as more appealing: namely, to introduce whatever price discrimination is called for, to the effect that prices of *consumer goods only* be proportional to “what they would be under generalized marginal cost pricing.”<sup>97</sup>

After further contributions by Boiteux [30] and Debreu [48] in the surplus tradition, a more satisfactory treatment was introduced by Boiteux [33] who used a “second best” approach.<sup>98</sup> Boiteux studied an economy consisting of two “sectors,” a “competitive” sector where marginal costs and marginal productivities are equated to market prices, and a “public” sector (consisting of either a single or several firms) where the same equalities hold with respect to a set of “shadow prices” that need not correspond to market prices but must be such that the public firm breaks even under the rules just mentioned. Boiteux studied the first-order conditions of constrained Pareto optimality for such an economy. Those conditions turned out to be derivable and susceptible of a reasonably straightforward interpretation, at the cost of some ingenuity.

In the case of a single budget constraint (imposed upon a single public firm, or alternatively upon the public sector as a whole), the analysis runs as follows.

Let there be  $m$  consumers,  $v$  firms belonging to the private sector and  $n$  goods or services, with market prices  $p_i$ ,  $i = 1 \dots n$ . The net quantities of good  $i$ , respectively, bought by consumer  $k$  and sold by firm  $h$  are denoted by  $q_i^k$  and  $\chi_i^h$ ,  $i = 1 \dots n$ ,  $k = 1 \dots m$ ,  $h = 1 \dots v$ . The consumption of consumer  $k$  is restricted by the budget constraint

$$(27) \quad \sum_{i=1}^n p_i q_i^k = r^k \quad k = 1 \dots m$$

<sup>96</sup> A sketchy theoretical derivation given in footnote (3), p. 10 of Allais [5] is unconvincing and was not referred to by the author in subsequent discussions.

<sup>97</sup> See Allais [7], p. 101; see also further comments by Allais in the preface to Lesourne [81], p. XXIII. Whether the later solution could be made operational has not been established.

<sup>98</sup> This approach was independently discussed at the same time by Lipsey and Lancaster [85]. Boiteux's analysis of a specific second best problem is carried out within a more complete and general model; as such, it remains—to the best of my knowledge—the fullest treatment of a second best problem in the literature to date.

The production of firm  $h$  is constrained by the production function

$$(28) \quad f^h(x_1^h, \dots, x_n^h) = 0.$$

The public firm (sector) sells the net quantity  $y_i$  of good  $i$ ,  $i=1 \dots m$ . Its production is constrained by the production function

$$(29) \quad g(y_1, \dots, y_n) = 0$$

whose partial derivatives  $\partial g / \partial y_i$  will be denoted by  $g_i$ ,  $i=1 \dots n$ .

The market clearing conditions are thus

$$(30) \quad y_i = \sum_{k=1}^m q_i^k - \sum_{h=1}^v x_i^h \quad i = 1 \dots n.$$

Together they imply that the sum of the transfer and property incomes of the consumers,  $r^k$ , be equal to the net profits of the private and public firms:<sup>99</sup>

$$(31) \quad \sum_{i=1}^n \sum_{k=1}^m p_i q_i^k = \sum_{k=1}^m r^k = \sum_{i=1}^n p_i y_i + \sum_{i=1}^n \sum_{h=1}^v p_i x_i^h$$

To these traditional conditions is now added a budget constraint for the public firm:

$$(32) \quad \sum_{i=1}^n p_i y_i = b$$

where  $b$  is some given number (possibly 0). First-order conditions for Pareto optimality are then derived, under the foregoing conditions (29), (30), (32), and the assumptions that (1) all consumers maximize their satisfaction, given their budget constraints (27) and (2) all private firms maximize their profits, given their production functions (28) and *given the market prices*  $p_1 \dots p_n$ .<sup>100</sup> The parameters to be determined by these first-order conditions are the  $n$  prices  $p_1 \dots p_n$ , the  $n$  public outputs  $y_1 \dots y_n$  and the  $m$  transfers  $r^1 \dots r^m$ —i.e.,  $2n+m$  parameters constrained by the  $n+2$  relations (29), (30) and (32). There are accordingly  $m+n-2$  “free” parameters in the system.

After eliminating (by means of the relevant first-order conditions) the  $m$  transfer parameters  $r^k$ , Boiteux shows that the remaining first-order conditions consist of (29), (32) and the following:<sup>101</sup>

$$(33) \quad \sum_{i=1}^{n-1} \left( g_i \frac{p_n}{g_n} - p_i \right) (ij) = \beta y_j, \quad j = 1 \dots n-1$$

<sup>99</sup> (31) is obtained by multiplying both sides of (30) by  $p_i$  and summing over  $i$ .

<sup>100</sup> Thus monopolistic exploitation is ruled out as well as external economies in production or consumption.

<sup>101</sup> My conditions (33) are the conditions (22) of Boiteux [33], p. 31, after correcting for an error of sign and adopting a different normalization rule; the error of sign arises from condi-

where:

$$(34) \quad (ij) = \sum_{k=1}^n \left( \frac{\partial q_i^k}{\partial p_j} + q_j^k \frac{\partial q_i^k}{\partial r^k} \right) - \sum_{k=1}^n \frac{\partial x_i^k}{\partial p_j}$$

is the partial derivative with respect to the  $j$ -th price of the *compensated* demand function for the public firm's output of the  $i$ -th good; and where  $\beta$  is a constant that must be chosen so as to bring about compatibility among the conditions (29), (32) and (33).<sup>102</sup>

The interpretation of (33) goes as follows: Let  $\pi_i, i=1 \dots n$  denote the shadow prices to which the public firm equates marginal costs and productivities. Using the  $n$ -th good as numéraire,<sup>103</sup> we may set  $p_n = \pi_n$  and write the marginal conditions as follows:

$$(35) \quad \frac{g_i}{\pi_i} = \frac{g_n}{\pi_n} = \frac{g_n}{p_n}, \quad i = 1 \dots n-1.$$

Accordingly,

$$(36) \quad g_i \frac{p_n}{g_n} - p_i = \pi_i - p_i = t_i, \quad i = 1 \dots n,$$

which defines  $t_i$  as the discrepancy between the shadow price and the market price of good  $i$ ;  $t_i$  may thus be interpreted as a "toll" levied by the public firm on good  $i$ , or alternatively, as the tax that should be imposed on the public firm's sales or purchases of good  $i$  in order to bring about equality between marginal costs and productivities on the

tions ( $\partial p_i$ ) and ( $\partial y_i^k$ ), *loc. cit.*, pp. 28-29, that give to  $\beta^k$  incompatible signs; I have corrected in ( $\partial p_i$ ) by changing all the signs in the conditions ( $\beta^k$ ), p. 28; the correction affects furthermore the last line of p. 28 and conditions (16), (18), (20), (22) and (24). The correction also led me to replace the normalization " $p_n = -\mu_n$ ," at the top of p. 30 by the alternative: " $p_n (1-\beta) = \mu_n$ ," which entails certain other advantages. Other misprints in [33] include the following: condition (14), p. 28, should read:

$$\sum_i p_i (ij)^k = 0;$$

conditions ( $\mu_i$ ), p. 28, should read:

$$\sum_k q_i^k(P, r^k) - \sum_k x_i^k(P) - \sum_k y_i^k = 0;$$

conditions ( $\partial y_i$ ), p. 29, should read:  $\mu_i = \phi^i g_i^i + \beta^i p_i$ .

<sup>102</sup> Actually,

$$\beta = \frac{p_n}{b - p_n y_n} \sum_{i=1}^{n-1} \left( p_i - g_i \frac{p_n}{g_n} \right) (in).$$

If the  $n+1$  parameters  $y_1, \dots, y_n$  and  $\beta$  are determined by (29), (32) and (33), then equations (30) can be solved for the prices, and the transfers can in turn be determined from the relations (not reproduced here) on the basis of which they were eliminated.

<sup>103</sup> It is implicit in the statement of the problem that *every one* of the  $n$  goods enters, either as an input or as an output, in the public firm's production function (29). One of the accounting prices may thus be chosen arbitrarily.

one hand and market prices net of taxes on the other hand.<sup>104</sup> Thus, (33) is a set of  $n-1$  equations that are linear in the  $n-1$  unknown tolls  $t_i$ ,  $i=1 \dots n-1$ :

$$(33') \quad \sum_{i=1}^{n-1} t_i(ij) = -\beta y_j \quad j = 1 \dots n-1.$$

Boiteux gives an interpretation of these conditions by showing that the tolls must be proportional to the (market) price differentials that would bring about *identical relative* differentials of the *compensated* demands (supplies) to the public sector. He regards this property as being the most natural starting point for empirical application of the conditions. Such applications would be facilitated by the important (and eminently sensible) further property that, for perfect substitutes, relative tolls should be identical (hence, absolute tolls proportional to marginal costs).<sup>105</sup>

A somewhat more direct interpretation of these conditions comes from noting that a profit maximizing policy on the part of the public firm would call for the first-order conditions.<sup>107</sup>

$$(37) \quad \sum_{i=1}^{n-1} \left( g_i \frac{p_n}{g_n} - p_i \right) \frac{\partial y_i}{\partial p_j} = y_j \quad j = 1 \dots n-1.$$

<sup>104</sup> Boiteux shows explicitly that the shadow prices  $\pi_i$  are also the marginal costs and productivities, when the latter are again computed in terms of the shadow prices. Thus, if good  $i$  is produced by the public firm, then  $\pi_i$  is its marginal cost in terms of shadow factor prices.

<sup>105</sup> The conditions can also be written in terms of relative tolls and "compensated" elasticities:

$$(33'') \quad \sum_{i=1}^{n-1} \frac{t_i}{p_i} \left[ -\frac{p_i}{y_i} (j i) \right] = \beta, \quad j = 1, \dots, n-1.$$

Indeed,  $(i j) = (j i)$ . See Boiteux [33], pp. 27-28. The reader may find it instructive to work out the implications of (33'') for the special case where  $(j i) = 0$ ,  $i \neq j$ .

<sup>106</sup> The situations typically referred to by Allais are not too remote from this case; indeed, he refers quite naturally to alternative means of transportation or sources of energy in discussing the merits of his policy recommendations; see footnote 97.

<sup>107</sup> Indeed, if one solves (29) explicitly for  $y_n = y_n(y_1 \dots y_{n-1})$ , so that

$$\frac{\partial y_n}{\partial y_i} = -\frac{g_i}{g_n}$$

and uses again  $p_n$  as a numéraire, the profit maximization problem becomes:

$$\text{Max}_{p_1 \dots p_{n-1}} \sum_{i=1}^{n-1} p_i y_i + p_n y_n(y_1 \dots y_{n-1}) = F(p_1 \dots p_{n-1})$$

The first order conditions are then:

$$\frac{\partial F}{\partial p_j} = y_j + \sum_{i=1}^{n-1} p_i \frac{\lambda y_i}{\partial p_j} + p_n \sum_{i=1}^{n-1} \frac{\partial y_n}{\partial y_i} \frac{\partial y_i}{\partial p_j} = 0, \quad j = 1, \dots, n-1.$$

These conditions are equivalent to (37) where

$$\frac{\partial y_n}{\partial y_i} \text{ has been replaced by } -\frac{g_i}{g_n}.$$

Thus, the first-order conditions for the constrained Pareto optimum differ from those for profit maximization by: (1) the introduction of the constant  $\beta$  on the right-hand side; and (2) the replacement of the partial derivatives  $\partial y_i / \partial p_j$  by the corresponding partial derivatives ( $ij$ ) of the *compensated* demand functions on the left-hand side.

The most natural way to introduce  $\beta$  in the profit maximization conditions (37) consists of replacing

$$\frac{\partial y_i}{\partial p_j} \text{ there by } \frac{1}{\beta} \frac{\partial y_i}{\partial p_j}; \text{ for } 1 > \beta > 0,^{108}$$

this would amount to saying that elasticities of demand for outputs (supplies of inputs) are "blown up" by a factor  $1/\beta > 1$ . Under this modification, and ignoring momentarily the second discrepancy (2), the public firm would behave as a profit maximizer who overestimates the elasticities by a constant factor of proportionality  $1/\beta$ . It would thus levy tolls geared to demand (supply) elasticities—but to a lesser degree than would a monopolist. The rationale for taking elasticities into account<sup>109</sup> would seem to be that optimal tolls should be such as to equate the marginal losses of "surplus" to consumers *per dollar contributed to meet the budget constraint* (32) and not per dollar spent.<sup>110</sup>

As for the compensated nature of the demand (supply) functions, it is due to the fact that the approach of Boiteux consists in deriving *simultaneously* a set of tolls  $t_i$  (or shadow prices  $\pi_i$ ) *and a set of transfer payments*  $r^k$ ,<sup>111</sup> where the sum of the transfer payments is constrained by (31). (31) may also be written, in view of (32) as:

$$(31') \quad \sum_{k=1}^m r^k - \sum_{i=1}^n \sum_{k=1}^v p_i x_i^k = b$$

This formulation brings out more explicitly the fact that all profits (losses) of the public firm are automatically redistributed among the consumers. These income effects are thus taken into account. If rules of property income formation were specified for the consumers,<sup>112</sup> and the transfers disposed with, then the optimal tolls<sup>113</sup> would become simply proportional to the profit maximizing tolls—i.e., to the tolls that

<sup>108</sup> It can be shown that  $\beta$  would have to lie in that interval if  $b$  exceeds the profits to the public firm that would obtain under unconstrained Pareto optimality—i.e., if, for instance,  $b=0$  but the public firm operates under increasing returns.

<sup>109</sup> This was initially urged by Colson and later rejected by Allais, as mentioned above.

<sup>110</sup> That is, per dollar of net *marginal* and not average revenue.

<sup>111</sup> The rules governing the transfer payments are not stated by Boiteux. They can be derived, however, and have an interesting welfare interpretation.

<sup>112</sup> That is, rules of distribution among consumers of the profits of the business sector.

<sup>113</sup> They would, of course, no longer be given by conditions (33) but rather by a slight modification thereof.

would be charged by a private monopolist taking these property-income effects into consideration.

The previous paragraph brings out two important points. On the one hand, if the transfers (compensations) are *not* paid, then the tolls defined by (33) are no longer Pareto optimal. On the other hand, if the transfers are paid, then it is not necessary for the public firm to know what they are: all that matters is an estimate of the elasticities of the compensated demand (supply) functions. To that extent, decentralization of decisions is preserved.<sup>114</sup>

The extension to several public firms, subject to independent budget constraints, reveals first that the tolls levied on a given pair of goods or services must be in the same ratio for all firms; i.e., the toll structure of two firms are identical up to a proportionality constant.<sup>115</sup> The additional conditions, although they generalize quite naturally the single-firm conditions, are somewhat more intricate.<sup>116</sup>

To conclude, a major point of departure from previous solutions lies in the use of shadow prices for inputs as well as for outputs—hence in different factor proportions at all levels of output. This point had probably been overlooked because so much previous discussion centered around pricing problems for public monopolies that buy factors on competitive markets; it becomes vitally important in the case of a country like France where a nationalized electricity company buys coal from nationalized coal mines in order to produce electricity to sell to nationalized railroads. If a single budget constraint could be imposed upon the nationalized sector,<sup>117</sup> then the marginal cost of coal would be equal to its marginal productivity in electricity generation, when electricity is “priced” at a marginal cost equal to its marginal productivity for the railroads (e.g., in hauling coal!). With separate budget constraints, it would still be true that the marginal rate of transformation between coal and electricity would be the same in all three firms.

More broadly, the significance of Boiteux’s paper as a contribution to the theory of second best is worth stressing. The results surveyed here can be extended into a full interpretation of the constrained optimum position, including not only the transfer rules but also the specification of marginal social values for commodities and capacity in the public firm and an evaluation of the marginal social cost of the budget balanc-

<sup>114</sup> Yet, the actual tolls play a role in the determination of the transfers.

<sup>115</sup> The proportionality constants being such as to bring about budget balancing for each firm.

<sup>116</sup> In the interpretation of Boiteux, the tolls levied by a firm must now be proportional to the price differentials that would bring about differentials of the income-compensated demands (supplies) to the public *sector* proportional to the outputs (inputs) of *that firm*.

<sup>117</sup> Either explicitly, or implicitly, thanks to transfer payments between the nationalized firms.

ing constraint.<sup>118,119</sup> Furthermore, an interesting analogy between the problem treated here and an altogether different problem involving a budget balancing constraint suggests itself. One might indeed think of the two productive sectors as being not a private and a public one but rather a domestic and a foreign one. The budget balancing constraint imposed upon the public sector could then be reinterpreted as a condition of equilibrium on the balance of payment, and the tolls would provide a "scientific tariff." I have not attempted to compare Boiteux's results with those reached in the international trade literature—but will venture the guess that the attempt should prove rewarding.

### *Applications*

A systematic discussion of applications of the theory surveyed in this section to the management of French public utilities would be a paper in itself. The papers by T. Marshak [103] and J. R. Nelson [130] and Chapter X in J. Lesourne [81] provide such a discussion. I shall limit myself to a few general comments.

The principle of marginal cost pricing of a peak load was advocated by Allais [5] in connection with railroad passenger traffic—the objective being to depress and spread the peaks of national holidays. Yet to the best of my knowledge, no actual application to passenger traffic has ever been attempted. Instead, the main field of application of peak-load pricing has been electricity.

The determination of the marginal and expansion costs of production, transportation, and delivery of electricity, taking into account the geographical location and the daily and seasonal as well as random fluctuations of demand, is no easy task. It was successfully carried out at EDF on the basis of the original studies by Boiteux [27, 29], and according to principles of application developed by Boiteux and Stasi [39] for production and transportation, and by Bessière [25] for delivery.<sup>120</sup> In developing these principles, the authors assume that optimal production, investment, and transportation decisions result in (a) equality between short-term and expansion marginal costs, and (b) equality between marginal costs at all points upon correction for net transportation costs.<sup>121</sup>

The result has been a new tariff, known as the "green tariff"—which

<sup>118</sup> This fuller interpretation is veiled in Boiteux's formulations by the sign error and misleading normalization mentioned in footnote 101. Its development would deserve separate treatment.

<sup>119</sup> As was pointed out to me by L. Hurwicz, the analysis could profitably be extended to situations where the public sector does not buy or sell *all* the goods, but only a subset of them.

<sup>120</sup> I have expressed above, my reservations about the treatment of stochastic demand fluctuations in these applications.

<sup>121</sup> Net transportation costs are positive or negative depending upon the *actual* direction of transport.

is described and commented upon by Boiteux [34, 35] and Massé [111]. In most cases, its application results in three charges, the first a lump sum, the second proportional to electricity consumption, and the third a function (sometimes more, sometimes less than proportional) of the peak quantity for which delivery is guaranteed.<sup>122</sup> Three different combinations of the second and third charges are available.

By penalizing peak demands, this tariff has resulted in substantial reductions of the fixed investments of *Electricité de France*: Massé [111] estimates the reductions at 100 millions of dollars.<sup>123</sup> Optimal reaction to the tariff on the part of users has been studied by M. Algan and M. Simonard [2], who use a marginalist approach; this closes the circle!

In the field of transportation, important applications have also taken place. The theoretical studies on pricing under a budget constraint led the French railroads (SNCF) to revise some of their policies.<sup>124</sup> In a first stage (1947), rates and loading conditions were adjusted to the loadability of commodities, improving by 30 percent the average pay load per car and hence the competitiveness of the system.<sup>125</sup> More recently (1962), differentiation of rates according to marginal costs on each route has been introduced. Another major concern has been with the allocation of traffic between alternative means of transportation. An interesting example is discussed by P. Maillet [88].<sup>126</sup>

## II. Intertemporal Allocation

In 1947 Allais published a two-volume work, *Economie et intérêt*, in which he advocated several rather drastic policy measures: namely, a frozen wage rate for unskilled labor, a depreciating currency, 100 percent reserve requirements, and collective ownership of land—these being regarded as prerequisites for the success of further policies (such as compulsory retirement provisions) aimed at maintaining a zero rate of

<sup>122</sup> Three different combinations of the second and third charges are available. It is interesting to see that part of the fixed costs are absorbed by the second, not the third, element—cf. Bessière [25].

<sup>123</sup> As pointed out by Nelson [130], the financial implications of marginal cost pricing in France are blurred by the severe inflation experienced in the country. Although the tariff was designed so as to bring about budget-balancing, one really does not know whether this condition was actually met. What marginal cost pricing achieves, under constant returns to scale and inflation is, in principle, twofold: (1) by equating short-term and long-term marginal costs, it provides a revenue sufficient to cover the full costs of marginal investments; (2) by charging automatically to the consumers the exact differential in expenditure that displacements of demand would bring about, it protects the firm against a deterioration of its financial position due to shifts in demand. Cf. Boiteux [32, 34].

<sup>124</sup> See Hutter [71, 72, 73].

<sup>125</sup> This figure was quoted to me in private correspondence by R. Hutter.

<sup>126</sup> The valuation of human lives and of time, respectively, have been discussed by Abraham and Thédié [1] and Lesourne [83] in connection with road investments; a comparison of risks is given by Roy [151].

interest. The underlying justification was a set of optimality conditions for intertemporal allocation; these conditions were shown to call for a capital endowment identical to that which a competitive equilibrium with a zero rate of interest would bring about.<sup>127</sup>

Commenting fifteen years later upon this problem of an optimal capital endowment (capitalistic optimum), Allais [14] wrote:

For my part, I have met in this area two sorts of economists. For the first, the existence of a capitalistic optimum for a zero rate of interest is considered as a completely mistaken proposition; for the second, it appears as a commonplace truth, a sort of truism, that does not deserve any attention at all.

I would not be surprised if the first group consisted of economists denying any *empirical relevance* to the *criterion* of "optimal capital endowment," whereas members of the second group might be concerned only with the *price implications* of the criterion, not its relevance. I hope to show here that both views are simultaneously sound.

More recently, J. Desrousseaux [49] has defended the equally unconventional view that, even in a world of certainty, there should exist differences among the marginal productivities of capital reached in different industries. Again, the justification came from unconventional optimality conditions for intertemporal allocation. It will accordingly prove convenient to review from the standpoint of optimality criteria the contributions to the theory of intertemporal allocation due to Allais, Desrousseaux, and E. Malinvaud.<sup>128</sup>

### *Efficiency and Pareto Optimality*

Malinvaud [92]<sup>129</sup> studies *programs* extending over an infinite number of discrete time periods for an economy whose sets of production possibilities for each period are convex.<sup>130</sup> A program is a complete physical specification of the inputs, outputs, consumptions, and assets of all the decision units of the economy at all times. Two main criteria are used for comparing programs: namely, *efficiency* and *Pareto optimality*. Loosely speaking, a program is efficient if there is *no other feasible pro-*

<sup>127</sup> *Economie et intérêt* is a much broader study than this paragraph might suggest; it is indeed a systematic text on intertemporal allocation, investment and money that contains many original developments aside from those surveyed here.

<sup>128</sup> Interest in optimal capital endowments has more recently been revived in the United States, for instance, in connection with the "golden rule of accumulation" introduced by Phelps [145b] and with the work on turnpike theorems and optimal economic growth—see, e.g., Koopmans [75c]. No attempt will be made here to integrate the two bodies of literature. The informed reader will undoubtedly notice some striking similarities as well as the definite originality of the French contributions.

<sup>129</sup> This fundamental paper is discussed at some length by Koopmans [75], I.4.

<sup>130</sup> The technology may, of course, vary from period to period, as may the available natural resources and labor inputs.

gram<sup>131</sup> providing at least as much to consume of everything at all times and requiring no more labor of any kind at any time. As is well known,<sup>132</sup> this definition implies intertemporal conditions; efficiency within every time period is not a sufficient condition for intertemporal efficiency.

Pareto optimality of efficient programs is defined in the usual way, with reference to the infinite set of present *and future* consumers, whose preferences are taken as given. Thus, a program  $P_1$  is Pareto optimal if there is not other feasible program  $P_2$  such that  $P_2$  is strictly preferred to  $P_1$  by at least one individual whereas  $P_1$  is strictly preferred to  $P_2$  by no individual.

The criterion of efficiency refers only to the allocation of *given* resources in *production*. The criterion of Pareto optimality refers, in addition, to: (1) the allocation of resources (including labor services) between production and consumption; (2) the allocation of resources among individuals (born or unborn) and for every individual among time periods.<sup>133</sup> Again, intertemporal conditions are implied that go beyond those imposed by efficiency.

The price implications of efficiency and Pareto optimality, for infinite programs that satisfy a few rather weak conditions, have been stated by Malinvaud [92]. By a price system is meant a set of discounted prices (present values), one for each commodity in each time period.<sup>134</sup> The main price implications of efficiency are the following: (1) To every efficient program, there corresponds a price system<sup>135</sup> in terms of which the net value of the program is larger than or equal to the net value of any other feasible program that becomes *identical* with it beyond some *arbitrarily distant* time  $T$ ,<sup>136</sup> (2) The production plan assigned by the efficient program to every firm (production unit) has, in terms of the same price system, a net value larger than or equal to the net value of any other feasible plan for the same firm, given identity of the two plans

<sup>131</sup> "Feasibility" is relative not only to technology and natural resources but also to initial assets.

<sup>132</sup> See, e.g., Dorfman, Samuelson, and Solow [55], Chap. 12.

<sup>133</sup> Remember that a program specifies the consumptions and assets of all individuals at all times.

<sup>134</sup> If  $p_{it}$  is the present value of one unit of commodity  $i$  to be delivered at time  $t$ , then its undiscounted price at time  $t$ , say  $\tilde{p}_{it}$ , can be defined as  $\tilde{p}_{it} = p_{it}/\beta_t$ , where  $\beta_t$  is a proportionality factor independent of  $i$ ; ( $\beta_t$  may, for instance, be chosen so as to keep constant the undiscounted price of a *numéraire* commodity); the rate of interest at time  $t$  is then

$$r_t = \frac{\beta_t}{\beta_{t+1}} - 1.$$

<sup>135</sup> Possibly several price systems.

<sup>136</sup> By means of this device, Malinvaud has reduced the comparison problem to finite dimensionality. The awkwardness of comparisons among infinite programs is well illustrated by some recent work of Koopmans [75b].

beyond  $T$ .<sup>136a</sup> The price implications of Pareto optimality extend proposition (2) into the field of consumption: the consumption plan assigned by a Pareto-optimal (and hence efficient) program to every consumer has a smaller value than *any* preferred consumption plan that would become identical with it beyond  $T$ , in terms of the price system associated [by (1)] with the Pareto-optimal program. Conversely, if a program is efficient and such that for every consumer any preferred consumption plan would be more expensive (in terms of a price system associated [by (1)] with the efficient program), then that program is also Pareto optimal.

The proposition according to which *any* competitive equilibrium is (efficient and) Pareto optimal holds for intertemporal but finite programs. The extension to infinite programs involving an infinite number of individuals remains to be formulated.

### *Allais Optimality*

The relevance of Pareto optimality has been questioned on two grounds: (a) the preferences of unborn consumers must be taken into account; "... this implies in particular that a competitive economy will realize a Pareto optimum only if, among other conditions, the future consumers are already well represented on the market;"<sup>137</sup> (b) the preferences of consumers among present and future consumption may be biased.<sup>138</sup> An intermediate criterion, weaker than Pareto optimality but stronger than efficiency, that escapes these particular objections, has been suggested by Allais [6] and later referred to as the criterion of "Allais optimality."<sup>139</sup>

Although a rigorous statement of this criterion is quite involved,<sup>140</sup> an indirect statement may be suggested informally as follows: consider an *efficient* program and an associated price system  $p_u^*$ ,  $i = 1, \dots, n$ ,  $t = 1, \dots, T$ , where  $T$  is arbitrarily large; this efficient program is Allais optimal if there exists for each consumer  $k$  a set of coefficients  $\beta_{kt}^*$ ,  $t = 1, \dots, T$ , such that any preferred consumption program for consumer  $k$ <sup>141</sup> *would be more expensive at prices  $\beta_{kt}^* p_u^*$* . This should be equivalent to the statement that an efficient program  $P_1$  is Allais optimal if there exists no program  $P_2$ , such that (a) for all  $t = 1, \dots, T$ , the

<sup>136a</sup> The converse proposition, stating the efficiency of a program that meets conditions (1) and (2) for *some* price system, is considerably more intricate, due to the technical difficulty of ruling out indefinite storage of assets.

<sup>137</sup> Malinvaud [97], p. 147.

<sup>138</sup> Allais [6], p. 163, puts considerable emphasis on this second point.

<sup>139</sup> Malinvaud [97], p. 147.

<sup>140</sup> See Malinvaud [97], pp. 147-48.

<sup>141</sup> That again becomes identical beyond  $T$ .

aggregate value at prices  $p_u^*$  of the consumption of *every individual* during period  $t$  is the same under  $P_2$  as under  $P_1$ ,<sup>142</sup> (b) at least one individual strictly prefers  $P_2$  to  $P_1$ , but no individual strictly prefers  $P_1$  to  $P_2$ .<sup>143</sup> By contrast, Pareto optimality implies that there exists no program  $P_3$  such that (a') the aggregate value at prices  $p_u^*$  of the *consumption plan* (for  $t=1 \dots T$ ) of every individual is the same under  $P_3$  as under  $P_1$ ,<sup>144</sup> (b') at least one individual strictly prefers  $P_3$  to  $P_1$ , but no individual strictly prefers  $P_1$  to  $P_3$ .

The criterion introduced by Allais may have more intuitive appeal than the definition suggests. It amounts to accepting consumer preferences in the sphere of atemporal allocation, but not in the sphere of intertemporal allocation; this would be the case under a system of forced savings in an otherwise competitive economy. Accordingly, problems of intertemporal allocation become formally equivalent to distribution problems: the same consumer at different times is treated as several consumers, whose consumptions generate mutual "external economies," but among which transfers of income (resources) are regarded as interpersonal transfers.<sup>145</sup>

Allais optimality is, of course, a relatively weak criterion—it is much weaker than Pareto optimality, although definitely stronger than efficiency. The only question which its acceptability raises, in my opinion, is the following: if consumer preferences regarding the allocation of their consumption between this and the next period are not taken into account, why should their current preferences regarding the allocation of next period's consumption among individual items be respected?<sup>146</sup> If

<sup>142</sup> Thus, if individual  $k$  consumes under program  $P_i$  a quantity  $q_i^k(P_i)$  of good  $i$  at time  $t$ , the condition (a) is that

$$\sum_i p_{it} q_{it}^k(P_i)$$

should be the same, for all  $k$  and all  $t=1 \dots T$ , under  $P_2$  as under  $P_1$ .

<sup>143</sup> See Allais [6], pp. 165–66. It should be obvious that Pareto optimality is a special case of Allais optimality, with  $\beta^k_{kt}=1$ .

<sup>144</sup> Thus, (a') requires that

$$\sum_{t=1}^T \sum_i p_{it} q_{it}^k(P_i)$$

be the same, for all  $k$ , under  $P_3$  as under  $P_2$ .

<sup>145</sup> By contrast, much recent work on intertemporal allocation has been conducted in terms of aggregate consumption per period—thus disregarding the very intertemporal complementarities taken into account by Allais optimality, but stressing the allocation over time of an individual's aggregate consumption, which Allais optimality treats as a distribution problem.

<sup>146</sup> Again, why should the preferences of unborn consumers be better known in the second case than in the first?

consumers are apt to regret tomorrow the amount of today's savings, they are equally apt to regret today's commitment to more bread and less wine tomorrow.<sup>147</sup>

### *Stationary Programs and Optimal Capital Endowments*

The additional criteria used by Allais and (following him) Desrousseaux are more conveniently introduced if some properties of *stationary* programs are stated first. Assuming the self-renewing natural resources and labor services to be available at a constant rate of flow and the production possibilities sets of all periods to be identical, stationary programs are easily defined by the condition that economic activity remains unchanged from one period to another.<sup>148</sup>

As shown by Malinvaud [92], efficiency of a stationary program implies the existence of a *stable* price system and a *constant* rate of interest in terms of which net profits<sup>149</sup> are maximum for every firm. That rate of interest is equal to the marginal productivity of capital.<sup>150</sup>

In a stationary program, the quantity of every commodity carried over from one period to the next is the same at all times. Calling "capital endowment" a (stationary) vector of such quantities, we may now define an *optimal capital endowment* by a natural extension of the efficiency concept, namely:<sup>151</sup> an *efficient stationary* program is associated with an optimal capital endowment if it is efficient compared with all other stationary programs, whatever their capital endowment may be, but otherwise feasible, i.e., compared with all other "technologically feasible" programs.<sup>152</sup> Thus, if the capital endowment of a stationary program is not optimal, there exist additions to or *subtractions from* that capital endowment which would permit *permanent* increases of at least one consumption.<sup>153</sup> One may wonder why subtractions are mentioned in this connection. The reason is that in a stationary program the initial capital endowment must be *maintained forever*: no "capital con-

<sup>147</sup> The commitment is made irreversible at the aggregate level, by the investment decisions embodied in a program.

<sup>148</sup> Hence all stocks and flows are constant in time.

<sup>149</sup> That is, the discounted value of outputs minus the current value of inputs, which is the same for all periods.

<sup>150</sup> This is strictly true only under an appropriate continuity assumption; in general, the rate of interest is at least (most) equal to the marginal productivity of a positive (negative) investment.

<sup>151</sup> Malinvaud [92], p. 262.

<sup>152</sup> Thus there is no other stationary program—whatever its capital endowment may be—that provides at least as much to consume of everything at all times and requires no more labor of any kind at any time, within the assumed technology. The extension of the efficiency concept thus consists in relaxing the feasibility constraints, insofar as they reflect availability of initial assets—while imposing stationarity.

<sup>153</sup> Alternatively, permanent reductions of at least one labor input.

sumption" is permitted.<sup>154</sup> If too much capital has accumulated, relative to the available natural resource and labor services, the required provision for capital maintenance and replacement may imply a reduction in consumption or an increase in labor inputs.

It should therefore be no surprise that an optimal capital endowment, if it exists, implies a rate of interest and a marginal productivity of capital both equal to zero.<sup>155</sup> Indeed, in such a situation, an addition to the capital endowment could not result in any increment in consumption, the gross increase in production being at best exactly sufficient to provide for capital maintenance and replacement.

Whether or not *finite* optimal capital endowments exist is an open question—everything depends upon the available technology.<sup>156</sup> Necessary and sufficient conditions for capital to become a free good<sup>157</sup> have not been investigated yet, to the best of my knowledge.

### *Proportional Programs*

Can optimal capital endowments be defined for non-stationary programs? Yes—provided some condition is introduced to prevent capital consumption. Thus, *proportional programs* are defined by the condition that all stocks and flows should grow at the same rate, usually called *expansion rate* (a function of time). When all the primary factors (i.e., the self-renewing natural resources and labor) grow proportionately at some rate, then a proportional program is the most natural extension of the stationarity concept. This permits the following definition: an *efficient proportional program*<sup>158</sup> is associated with an optimal capital endowment if it is efficient compared with all other *technologically feasible* proportional programs<sup>159</sup> (whatever their capital endowment may be).

Programs of proportional *exponential* growth have been studied closely for linear models (characterized by constant returns to scale), such as that of J. von Neumann [168] or W. Leontief [79].<sup>160,161</sup> The relations between the expansion rate and the interest rate in a von

<sup>154</sup> If capital consumption were permissible, a state of satiation should be reached in order for additional capital to be worthless.

<sup>155</sup> Allais [6], p. 184; Malinvaud [92], lemma 8.

<sup>156</sup> See the comments by Koopmans [75], pp. 122 ff; Allais [6], pp. 196 ff. asserts that finite capital endowments exist for economies characterized by currently available technologies.

<sup>157</sup> In the sense of a zero price (marginal productivity) of capital; as pointed out in footnote 154, this does not mean satiation of wants.

<sup>158</sup> It is well known that a proportional program for which the rate of growth is maximal (in some sense) is not necessarily efficient; see Malinvaud [96].

<sup>159</sup> That is, compared with all other programs, whatever their capital endowment may be, but otherwise feasible.

<sup>160</sup> An interesting approach to the optimal equipment problem in a dynamic Leontief model has been developed by Faiveley [57b]; see also the summary by Massé [112], Chap. IX.

<sup>161</sup> The "golden rule path" of Phelps [145b] also falls in this category.

Neumann model have been investigated by Malinvaud [96], who shows that the rate of interest exceeds the expansion rate when consumption exceeds the inputs of primary factors. The converse proposition could be interpreted to mean that when the marginal productivity of capital (interest rate) is less than the expansion rate, the proportional growth of capital must be partly supported by reducing consumption (below the inputs of primary factors). Although Malinvaud does not address himself to that question, one would be tempted to conclude that if an efficient proportional program of exponential growth with optimal capital endowment exists, it must imply a rate of interest and marginal productivity of capital equal to the expansion rate.<sup>162</sup> This is a very reasonable proposition, considering that the *marginal productivity of capital is also the maximal rate at which growth of a marginal unit of capital can be self-sustaining (self-financing) in the absence of interest charges*. In other words, if interest charges are replaced by a growth condition on the initial endowment, the rate of growth plays a role analogous to the rate of interest—at least under constant returns to scale. Such would also seem to be the most natural interpretation of a similar result recently announced by Allais [14].<sup>163</sup>

### *Stationary Marginal Modifications*

Desrousseaux [49]<sup>164</sup> has attacked a version of the general case by means of an additional criterion, called "regularity." In applying this criterion, Desrousseaux opens an avenue of generalization of the concept of optimality of capital endowment—a generalization that may, in fact, be restated without explicit reliance upon regularity.<sup>165</sup>

<sup>162</sup> Malinvaud [97], Chap. V, discusses further the relation between the rates of expansion and of interest, using an approach of Hicks' [67b]. An unpublished paper by Malinvaud, "On the Rate of Interest of Efficient Proportional Growth Programs," Berkeley, 1961, should also prove relevant to the optimal capital endowment problem.

<sup>163</sup> That paper explicitly assumes exponential proportional growth of the primary factors but does not explicitly restrict attention to proportional programs; neither are returns to scale assumed constant; the paper is in the nature of an abstract, so it is hard to understand fully the implications of the announced results; Allais [14], p. 727, refers to a forthcoming book on these matters.

<sup>164</sup> The analysis of Desrousseaux is further developed in several stimulating unpublished monographs ("Caractéristiques globales des économies quelconques et taux d'intérêt optimal dans les mondes évolutifs," May, 1961; "Productivité des investissements et taux d'intérêt différenciés," March, 1962; "Réflexions sur la théorie de l'évolution économique, ses hypothèses, ses concepts et ses conclusions," August, 1962), and the author has announced to me in private correspondence forthcoming additional results.

<sup>165</sup> Applied to a single economic parameter, the criterion of regularity calls for minimizing the absolute value of the time derivative of that parameter. The idea is, of course, to rule out fluctuations, and one may regard the work of Desrousseaux as a step in the direction of a *joint* analysis of growth and fluctuations. Desrousseaux [50] offers an interesting discussion of the role of indivisible investments in the business cycle; some comments on the dynamic instabilities of a competitive economy are also given in [49] as well as in the unpublished monograph, "Réflexions sur la théorie de l'évolution économique," mentioned in footnote 164.

Following Allais [6, 14], Desrousseaux relies upon the Jevonian and Austrian concepts of a "characteristic curve" and of average periods of production and amortization for a capitalistic production process.<sup>166</sup> The simplest model is one in which there is a single primary factor, labor; labor can be used in alternative production processes that "mature" (i.e., result in consumption) after different periods of time. Capital is accumulated by shifting labor from processes with shorter periods of maturation to processes with longer periods, thus lengthening the average period of production. If a production process is defined by its maturation period, and if the level at which that process is operated is measured by the required labor input, then a function specifying the *distribution of labor inputs by maturation periods* gives a summary description of economic activity at a given time. When labor inputs at time  $t$  are expressed as ratios to aggregate labor inputs at that time, the corresponding function will be called "distribution function" for  $t$ .

For an economy with *given* aggregate labor inputs at all times,<sup>167</sup> the condition that the distribution function be independent of time (stationary) may be regarded as a condition of proportional growth.<sup>168</sup> Efficiency of a *stationary* distribution function is then defined by the property that no other distribution function (applied *indefinitely in the past* as well as in the future!) would permit more consumption at some time and at least as much consumption at all times. This property, because it applies to the past as well as to the future, has been regarded as equivalent to optimality of the capital endowment.

Desrousseaux [49, paragraph 234] shows that, if *efficient stationary* distribution functions exist, they imply equality at all times between the rate of interest and the rate of growth of the primary factor (labor), the latter rate being, of course, a function of time. Furthermore, Desrousseaux considers the class of programs for which any *stationary marginal modification* of the distribution functions (themselves no longer assumed stationary) would be inefficient.<sup>169</sup> By this is meant the following: given a set of distribution functions for all times (past and present), there is no other set of distribution functions, differing *infinitesimally*

<sup>166</sup> The reader who is unfamiliar with these concepts may find the following few paragraphs rather obscure. The best I can do is to refer him to Allais [6] for a systematic exposition. See also the comments on that approach by Malinvaud [92], historical note, and [97], *in fine*.

<sup>167</sup> That labor inputs are given does not mean that they are constant, or even growing at a constant rate.

<sup>168</sup> It will be noticed, however, that although all production processes are operated at levels proportional to aggregate labor inputs, the same proportionality does not hold for consumption and assets unless labor services grow at an exponential rate; this is so because consumption and assets are by definition proportional to a weighted average of past labor inputs; this is one of the troublesome peculiarities of the model under discussion.

<sup>169</sup> Desrousseaux is led to consider such programs by imposing the condition of regularity upon the *relative* labor inputs in every production process; my understanding is that the condition *actually* used is regularity of rates of change of the relative inputs; this distinction is immaterial from the standpoint of the results mentioned in the text.

from the former in a *stationary* way (by the same infinitesimal amounts at all times), and defining a program with respect to which the former is not efficient. If programs that are efficient with respect to such stationary modifications exist, they again imply equality at all times between the rate of interest and the rate of growth of the primary factor.<sup>170</sup>

As the example in the last paragraph shows, the concept of "optimality of capital endowment" can be extended to non-proportional programs by considering *constrained* marginal additions to the capital endowment—the constraints coming in to prevent capital consumption. (The constraints would thus bear upon replacement and growth of the marginal addition to the capital endowment.) The criterion of optimality of that capital endowment could then be stated (conditionally upon specific constraints) as follows: an efficient program is associated *at all times* with an optimal capital endowment if there is no marginal addition to (or subtraction from) that capital endowment at any time that would permit at least as much consumption of everything at all times and more of something at some time, *under the constraints imposed upon replacement and growth of the additional capital*. Thus the choice of constraints *defines* the criterion of optimality of capital endowment!

Precisely what constraints could be used meaningfully is an open question. If the condition were imposed that the additional capital should grow at a rate equal (at all times, or on the average) to the marginal productivity of the existing capital, then the existing capital endowment (whatever it may be) would automatically be found optimal under quite general conditions. Allais and Desrousseaux use a condition of growth (of a marginal addition to capital) at the same rate as the exogenous primary factor. (When that primary factor is labor, the marginal additions to capital result in a constant addition to capital per worker.) The optimality of such a criterion is by no means evident. If several primary factors were growing at different rates, the criterion would become ambiguous.<sup>171</sup> This difficulty is akin to that encountered if a criterion of maximum growth were to be defined for nonproportional programs.<sup>172</sup>

<sup>170</sup> This is my interpretation of the results reached by Desrousseaux [49], paragraphs 232–233. The concepts used by Desrousseaux require very careful handling and my interpretation may be incorrect. The correspondence between the models of Allais and Desrousseaux and the more familiar model of Malinvaud has never been systematically explored, to the best of my knowledge. Pending such clarification, it is hard to tell just how general is the result mentioned in the text.

<sup>171</sup> The solution chosen by Desrousseaux, namely, to impose different rates of growth for different capital goods, depending upon the rates of growth of the primary factors to which they are related, is definitely unconvincing. By anticipation of the next section, it may be pointed out that such a criterion would generally be inconsistent with Allais optimality and eventually even with efficiency.

<sup>172</sup> See Malinvaud [97], p. 149.

*Relevance and Compatibility of the Criteria*

As indicated above, Pareto optimality implies Allais optimality, which in turn implies efficiency, and optimality of capital endowment implies efficiency. So much is clear. Is there more to say?

Let us first remark that Pareto or Allais optimality clearly define policy-oriented criteria, whereas optimality of capital endowment (no matter how defined) depends *at a given time* upon *past* accumulation and is thus, *as of that time*, a matter of fact and not of policy. The first use to which the criterion could be put is thus evaluation of the available capital equipment. If one wished to go beyond this, for instance, to advocate policy measures aimed at accumulating an optimal capital endowment (as defined by appropriate constraints on capital consumption), the consistency of such policies with the criteria of Pareto and Allais optimality would have to be considered. For a class of Pareto optimal programs, namely, those implying a distribution of resources among consumers regarded as socially acceptable, it will typically happen that the class contains no program conducive to what has been independently called an optimal capital endowment. The class of Pareto optimal programs could conceivably be enlarged to a corresponding class of Allais optimal programs, namely, programs implying similar distributions of overall resources among consumers but less preferred intertemporal allocation of these resources by at least some consumers. This broader class might well contain programs that are conducive to an optimal capital endowment at some time or another (existence theorems in this area would be welcome). Allais' policy recommendation is that some such program (e.g., a program leading to the desired optimal capital endowment after 20 years) be adopted and the implied intertemporal allocation imposed upon consumers. But to say that consumer preferences for intertemporal allocation may be ill-defined or biased is not to say that *any* intertemporal allocation is as good as *any* other. Again, a socially acceptable subclass should be defined—and the question raised whether it contains programs conducive to an optimal capital endowment. However, by this time, social preferences among different intertemporal allocations for individual consumers are introduced. I think that the criterion of optimality of capital endowment could be treated similarly: the constraints entering its definition could be regarded as the expression of similar social preferences. It is my conjecture that the *concept* of optimality of capital endowment would then become redundant—but might (have) serve(d) a useful technical role in clarifying the technical implications of alternative social preferences for intertemporal allocation.<sup>173</sup>

<sup>173</sup> The foregoing is an attempt to assess the potential usefulness of the criteria of Allais optimality and optimality of the capital endowment in the hypothesis of meaningful social preferences among different intertemporal allocations for individual consumers. The para-

### III. National Planning

#### *What is French Planning?*

French planning has frequently been described as being "indicative."<sup>174</sup> In his preface to Perroux's recent book on the fourth plan [144], Massé, who is currently serving as *Commissaire Général au Plan*, specifies further: "... French planning is not imperative, but more than indicative; *active planning* would be more to the point; for it implies the initiation, within the general framework of the plan, of a string of special actions aimed at making the plan effective."

This concept of "active planning" is definitely intriguing for a foreigner like myself, who reads that a French four-year plan is a *law* (hence should be enforceable), but a law that defines the plan itself as being merely "a *framework* for the investment programs" and "a *guide* for economic development and social progress." That "law" is generally said to have worked well, although statements to that effect are almost never backed by conclusive empirical tests.

In very broad terms, the plan is a summary description of the (market) economy projected four years into the future and accompanied by a set of commentaries and recommendations. As such, it may be regarded as a giant marketing study at the national level. Because it is conducted at the national level, it may take into account macro-economic equilibrium relations (between saving and investment, imports and exports, . . . ), but it must also remain fairly aggregative. The prospective aggregate accounts, as well as the recommendations, provide a frame of reference for the decisions to be taken over that period by the government, by firms, and by individuals. The idea behind *indicative* planning is that such a frame of reference is indispensable for many decisions, and something may be gained by having as many decisions as possible taken within the *same* frame of reference.<sup>175</sup> The idea behind *active* planning is to associate many decision makers with the preparation and revisions of the projection. It is thus *hoped* that the projection, to the extent that it *forecasts and influences* their decisions, brings about enough consistency among decentralized plans and consensus among decentralized objectives to become substantially *self-realizing*.

Of course, planning concepts, methods, and instruments have evolved over the fifteen years since Jean Monnet, the father of French planning,

graph should not be understood as an advocacy of such a scheme by the present writer, who yet fails to be convinced of its merits.

<sup>174</sup> Just what is meant by "indicative planning" is not altogether clear; the term is often used as a shortcut for the description of French planning—a tradition which I shall follow here.

<sup>175</sup> That something may also be lost, should the "frame of reference" be erroneous, is obvious; an empirical analysis of the consequences of forecasting errors by the coal and steel community should prove instructive.

directed the preparation of the first plan. The major evolution came from the economic conditions to which the general objectives of the successive plans were geared.<sup>176</sup>

The *first plan* (1947–1952/53) aimed at speeding reconstruction and eliminating bottlenecks in the postwar economy by assigning priority to the expansion of six basic industries: coal and electricity, steel and cement, transportation and farm machinery (to which oil and nitrates were added as the period of the plan was extended into 1953). The achievement of the objectives was made easier by Marshall aid.

The *second plan* (1954–1957) was concerned with spreading development and progress throughout the economy (including agriculture). The emphasis was shifted from capacity toward productivity, with due attention to scientific research, personnel training, and the like. These objectives were attained and in many cases exceeded, but at the cost of inflation and balance of payments disequilibrium.

The *third plan* (1958–1961) had to cope with the above difficulties as well as with the 1957–58 recession. By taking monetary and international<sup>177</sup> problems into account, as well as by its ambitious production and investment objectives,<sup>178</sup> the third plan had a definitely broader scope than its predecessors.

The *fourth plan* (1962–1965) innovates further by treating explicitly the income distribution problem (with particular attention for the underprivileged and the choice between work and leisure), as well as by the priority given to social forms of investment—education, health, cultural, and urban equipment. Regional development is also included.<sup>179</sup>

If the differences in emphasis reflect the evolution of economic conditions, the progressively more comprehensive and ambitious objectives of the plans reflect partly the improvement of the tools and techniques used, partly the underlying transformations of the “economic order” and growing moral authority of the *Commissariat au Plan*. During the same time, the administrative organization and staff have not changed much, and the number and scope of instruments of authoritative implementation have gone down.

A brief description of the techniques, organization, and instruments of the plan is given under the next headings, introducing some comments about current orientations.<sup>180</sup>

<sup>176</sup> See, e.g., Bauchet [21], pp. 61 ff; Massé [115], II; Perroux [144], Introduction.

<sup>177</sup> 1958 is also the year during which the European Common Market was established.

<sup>178</sup> They were realized only for the revised second half of the plan.

<sup>179</sup> Regional economics has been a field of intense activity in postwar France; Boudeville [40, 41], Gendarme [63], and Perroux [134] are typical references to a literature that is constantly growing.

<sup>180</sup> A comparison of the French approach with that used in other Western European countries, like Italy or the Netherlands, would be very helpful—but goes beyond the scope of this survey; see Wellisz [169].

### *Tools and Techniques*

National accounts provide most of the basic data that are used for the preparation and revision of the plans.

Although it was invented in France with the *Tableau Economique* of François Quesnay, national accounting was almost nonexistent there before the war. The merit of its development into an important, and in many ways original, construction goes to another graduate of the *Ecole Polytechnique*, Claude Gruson, who created the *Service des Etudes Economiques et Financières* (SEEF) in the Ministry of Finance and was remarkably successful in making efficient use of rather scarce means.

The development of national accounts in France has been unusual in at least two respects. On the one hand, the technical and frequently arid nature of the subject did not deter a large group of first-rate economists from taking interest in it and writing about it.<sup>181</sup> Suggestions and criticisms have thus come from many directions, while information about the new tool and its possibilities became widespread. On the other hand, Gruson, himself an excellent theoretician,<sup>182</sup> was able to secure the collaboration of exceptionally capable analysts who contributed to the clarity of the system and the originality of its applications.<sup>183</sup> The two most outstanding instances, in terms of both the quality of their analytical work and the continuity of their association with SEEF, are André Nataf<sup>184</sup> and Pierre Thionet.<sup>185</sup>

While national income statistics are gathered by SEEF and the National Statistical Bureau,<sup>186</sup> money flows,<sup>187</sup> input-output tables,<sup>188</sup> and

<sup>181</sup> Some basic references are Allais [9], Gruson [66], Maillet [86], Malinvaud [95], Jean Marchal [100, 101], Perroux [133], Perroux, Uri and Marczewski [145], Prou [148]; the last reference contains historical notes; most of these contributions go beyond exposition or discussion of accounting techniques to build a bridge between national accounts and economic analysis.

<sup>182</sup> Witness [65].

<sup>183</sup> Thus, a yearly publication of SEEF, *Etudes de Comptabilité Nationale* [153], contains sometimes brilliant theoretical contributions, e.g., Thionet [162], Fourgeaud [59].

<sup>184</sup> Nataf has made very significant contributions to the theory of aggregation. See [125, 126, 127, 128] and [60]. Other major French contributions to the theory of aggregation are due to Guilbaud [67], Malinvaud [93, 94] and Thionet [158, 161].

<sup>185</sup> Thionet has written extensively on sample surveys and sampling theory—see, for instance, [157, 159, 163]—as well as on economic subjects.

<sup>186</sup> Institut National de la Statistique et des Etudes Economiques—INSEE.

<sup>187</sup> The French tables of money flows register, for some 40 types of monetary assets, the changes in the balance sheet (assets and liabilities) of the treasury, banking system, financial intermediaries, local administrations, households, business firms, overseas territories, and the rest of the world; for a detailed description, see Barthélémy [19].

<sup>188</sup> In 1951 two rectangular matrices were set up in terms of 112 "industries" and 157 "commodities," with consolidations into 37 industries and 65 commodities, and again into 12 industries and 12 commodities; one matrix gave a breakdown of the inputs of each industry into groups of commodities, the other a similar breakdown for outputs; in 1956, a classical square input-output matrix (with 66 sectors and a consolidation into 16 sectors) was

"economic budgets" are assembled at SEEF. Economic budgets<sup>189</sup> are prospective national income accounts serving as a framework for the treasury's budget and as a step in the preparation and revision of the plans. Furthermore, budget studies are carried out by a separate institution<sup>190</sup> in close cooperation with SEEF and INSEE.<sup>191</sup>

In very broad outline, these sources of information are utilized in the following way:<sup>192</sup> given a hypothesis about its rate of growth,<sup>193</sup> a projection of GNP is computed for the terminal year of the plan. Comparing this figure with the labor force forecast gives an indication of productivity changes which, in turn, serve as a basis for estimating investment requirements.<sup>194</sup> Subtracting these and prospective public expenditures<sup>195</sup> from the GNP figure yields aggregate consumption as a residual. Aggregate consumption expenditures are then broken down into commodity groups, on the basis of income elasticities (corrected for trends, demographic evolution, income redistribution, etc.). This step, together with an autonomous breakdown of public expenditures and investment, leads to a vector of final demands. A prospective input-output matrix (with coefficients adjusted for technical progress) is then called upon to translate final demands into gross output and employment figures for industrial sectors. The whole analysis is carried out at constant prices.<sup>196</sup>

Needless to say, the actual computations are much more elaborate than this outline reveals. Yet many subjective elements enter into the procedure: the figures for public and private consumption, the investment requirements and the technical coefficients are adjusted at various stages in the process of making the plan. The originality of French planning consists precisely in associating a large number of qualified and concerned outsiders with the preparation of the plan, through in-

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used; for more details see, e.g., Mailliet [87, 89]. Regional input-output matrices have also been set-up, starting with Bauchet [20].

<sup>189</sup> See Mayer [120], Betout-Mossé [26].

<sup>190</sup> "Centre de Recherches et de Documentation sur la Consommation," CREDOC; see Rottier [149], Rottier and Salembien [150].

<sup>191</sup> Among independent studies providing or analyzing quantitative information in related areas, a special mention should be made of (1) the attempt by Divisia, Dupin and Roy [54] to evaluate national wealth; (2) the work of Fourastié [58] on prices and productivity; and (3) the work of Jean Marchal and his associates on distribution of the national income, Marchal and Lecaillon [102], Latil [78], Tavitian [155], Tiano [165], etc.

<sup>192</sup> For further details, see Gruson [66], Bauchet [21], pp. 149-160, or Betout-Mossé [26]. Paelinck and Wallbroeck [131] describe the closely parallel techniques used in Belgium.

<sup>193</sup> The role of such hypotheses is further discussed below.

<sup>194</sup> Largely in terms of marginal capital output ratios, adjusted for substitution effects.

<sup>195</sup> Estimated directly on the basis of desired extensions of the public services, demographic changes, etc.

<sup>196</sup> Foreign trade is voluntarily left out here in order to simplify exposition; see Perroux [144] on this point.

stitutions shortly to be described. The presence of so many subjective adjustments deprives the projection of built-in consistency. This gives autonomous significance to the final consistency tests bearing upon employment and productivity, savings and investments, etc. . . .<sup>197</sup> Such tests are followed by further adjustments. How well alternative hypotheses regarding the rate of growth of GNP meet these tests is an important element of appreciation.

All told, the projection remains very coarse in several respects: it describes a terminal equilibrium, without consideration for the path by which it is to be reached or for the broad orientation of future plans; it runs in terms of highly aggregated sectors and of aggregate consistency tests (equilibrium is only roughly studied for regional employment markets, or for specific monetary markets); very few alternatives are explored.<sup>198</sup> Refinements are under way in most of these respects.

### *Men and Organization*

The French *Commissariat Général du Plan* is a relatively small administrative body with a total staff of some 100 employees and no official authority or financial means. It must rely upon outside administrative departments (such as INSEE and SEEF) for most of the data and studies required by its mission; thus, the regular government administration is automatically linked to the preparation of the plan.

Furthermore, the *Commissariat* is assisted by 25 *Commissions de Modernisation*, an original institution created by Monnet in order to turn the plan into a collective venture and to preclude bureaucratic sclerosis. Each commission consists of 30 to 50 appointed members, contributing their time without compensation and belonging mostly to three categories: public servants, business leaders, and labor leaders. Technicians, university professors, and representatives of other social groups join *study groups* into which the commissions divide to concentrate on particular problems.<sup>199</sup> Altogether, some 3,500 persons participate in the activities of the commissions and groups.

Of these 25 commissions, 20 are called "vertical" and each of these is responsible for a particular industrial sector (for example, iron and steel or chemistry) while five "horizontal" commissions, working across industries, are respectively concerned with labor, finances and general equilibrium, regional development, research, and productivity.

The work of the commissions in connection with the fourth plan (1962-1965) has progressed roughly as follows:<sup>200</sup> In June, 1960, each

<sup>197</sup> Additional tests involving prices are being developed and are discussed below—see footnote 219.

<sup>198</sup> These shortcomings are discussed at length by Perroux [144].

<sup>199</sup> A single commission may thus create up to 50 study groups.

<sup>200</sup> See Betout-Mossé [26].

vertical commission received from the *Commissariat* (1) *directions*, generally quantitative, regarding the rate of growth of GNP, the balance of payments, etc. . . . ; (2) *orientations*, of a more qualitative nature, regarding the development of social overhead capital, the length of the working week, and so on; (3) a *questionnaire* calling for the following data concerning the industrial sector(s) of the commission in 1965:

production and its uses (intermediate, final, outside . . . demand)  
 imports and exports  
 inputs from other sectors and sales to other sectors  
 investments, by types (plant, equipment . . . ) and source of financing  
 employment, by skills and by regions;

(4) past and *provisional accounts* for all industrial sectors—the past accounts coming from current statistics, the provisional accounts from preliminary theoretical calculations (of the type outlined above).

At this stage, about 250 industrial sectors were distinguished—to be consolidated later into 65, and finally, 28 aggregates. The commissions were expected, first of all, to examine past accounts, to reconcile them (if possible) with data supplied by their members (and originating in administrative departments, industrial and labor federations, business firms, and so on) and to suggest corrections if needed. This serves the double purpose of checking data currently used by the *Commissariat* and securing uniformity of concepts and definitions between the *Commissariat* and the commissions. Next, each commission was expected to fill the questionnaire with data again supplied (partly or wholly) by its members, working on the basis of the general directions and orientations mentioned above. How this can be done varies substantially from sector to sector. In those areas corresponding to activities of nationalized enterprises (power, transportation, public works), the forecasts and plans of the firms are directly used. In sectors where oligopoly situations prevail (e.g., automobile) much depends upon the willingness of the firms to disclose their plans and information. The experience seems to be that investment data are definitely easier to obtain than production estimates.<sup>201</sup> In competitive sectors (agriculture, manufacturing, services), statistical data and subjective evaluations by representative individuals (expected to consult with customers, suppliers, competitors) prevail. In all cases, the “provisional accounts” received from the *Commissariat* are supposed to be replaced by or amended through outside information.

At the same time, horizontal commissions received similar documents and performed a similar task. Employees of the *Commissariat* or collaborating departments (e.g., SEEF) check the work of the commissions,

<sup>201</sup> Perroux [144], p. 22, sees a danger in this collaboration of monopolistic firms in the plan: how could the planning authorities effectively curb monopoly power after that?

sometimes putting data into more appropriate form, sometimes supplying data not directly available to members, eliminating double counts or filling holes.

On this basis, a first synthesis was assembled by February, 1961, that was to serve a double role: (1) provisional revision of the central directives and orientations, in the light of the accounts supplied by the commissions; and (2) performance of consistency tests on these accounts (e.g., by comparing inputs and outputs as given by the buying and selling sectors or production figures as given by complementary sectors), and on their consolidation (investment, employment, GNP, foreign trade).

As the two sets of revisions are prepared by the *Commissariat* and the commissions (with growing importance attached to the horizontal commissions), a second synthesis emerges (May, 1961) upon consolidation of the 250 into 65 sectors and deeper examination of the macro-economic equilibrium relations. This calls both for statistical routine work and for arbitration of incompatibilities between the views of different commissions.

Once these data are regarded as final, they are aggregated again into 28 sectors and used to prepare the plan itself, with its commentaries and recommendations—an impressive document that was made available to congressmen in November, 1961. Although little room is left by then for modifications in the plan, attempts are currently under way to associate more closely the political authorities with the preparation of the plan by giving them responsibility for the early decisions about directions and general orientations.<sup>202</sup>

### *Instruments of Active Planning*

Once the plan has become a law, what instruments are at the disposal of the government in order to make it effective? In theory, there is a very simple answer to that question: no *special* means of economic control or rights of interference are attached to the existence of the *Commissariat au Plan* or the plan itself. Thus, only the ordinary tools of economic policy are available, and in France as in other Western countries, there are more carrots than sticks among these. Moreover, the plan is not really binding on the government and public services themselves, even when they are directly concerned.

However, in practice, it would seem that at least one important means of direct implementation has been effective and may be expected to remain so, namely, investment by the state and nationalized enterprises. In 1960 this accounted for 38 percent of metropolitan gross fixed

<sup>202</sup> See Massé [116].

investment.<sup>203</sup> If we add investments financed by public funds or by financial intermediaries subject to public control, each accounting for eight percent of the total, we see that *at least* 54 percent of total gross investment<sup>204</sup> was under public control. Since the Finance Minister has declared that the government was *in principle* under obligation to finance by priority the public investments called for by the plan,<sup>205</sup> at least that much is under control.

For the rest, it is through the usual decisions of economic policy that appropriate incentives or correctives can be created. In that connection, the main virtue of the plan is to provide a *consistent framework* within which these decisions can be taken. In many countries, such a framework is provided only by the yearly state budget, which is notoriously inappropriate.

Many of these incentives and correctives aim at influencing the decisions of business firms. The plan, however, is supposed to affect them directly in three ways:<sup>206</sup> by its influence upon their expectations; by the influence of public investment upon their demands and by the spontaneous adherence expected to result from participation in commissions and study groups. The first two points are obvious.<sup>207</sup> Few observers question their role in the recovery and rapid growth of the French economy since the war. As for the third point, although it is readily seen how it could be effective in isolated situations,<sup>208</sup> it has so far never been put to the hard test of facts: how effective this adherence would prove in cases of conflict between private interests and the orientations of the plan remains to be seen. Much would seem to depend on price and wage considerations, about which the plan has little to say so far.<sup>209</sup> The same reservations are in order in connection with the behavior of labor and consumers (savers). As more and more free wills and political interests are concerned, the prospects for "spontaneous co-

<sup>203</sup> According to figures reproduced by Bauchet [21b], p. 142.

<sup>204</sup> Indeed, financing by bonds is also subject to an element of control.

<sup>205</sup> Quoted by Perroux [144], p. 106.

<sup>206</sup> Massé [115], p. 14.

<sup>207</sup> The following personal memory, quoted by Massé [115], p. 6, in connection with the first plan, is a perfect illustration: "I remember the shock created at Electricité de France, where I had just been appointed Director of Equipment, by the target set at 39.5 billions Kwh, whereas prewar peak consumption was barely 21 billions. Without the coordinated effort announced by the plan, our skepticism would have rejected that ambitious goal." The target was accepted and nearly reached.

<sup>208</sup> Thus in location decisions, social welfare considerations (external economies and distributive effects) are likely to be more naturally taken into account by those who have been concerned with regional development aspects of the plan.

<sup>209</sup> Thus: would firms create the additional capacity called for by the plan, instead of striving for monopoly profits or in spite of pessimistic price expectations? I, for one, remain to be convinced.

operation" become dimmer. The emphasis by the fourth plan on income policy, in spite of its tentative nature, affords some ground for hope. Again, the desirability of empirical testing of the effectiveness of the plan should be emphasized.

### *Current Orientations*<sup>210</sup>

In several recent papers, Massé [113, 114, 115, 116] has exposed the conception underlying his current endeavors as *Commissaire Général au Plan*. Three major ideas are recurrent in these writings.

The first one is that French planning should remain indicative. Additional means of central direction and coercion are nowhere advocated. "Changing our policy with respect to means would foster neither human promotion nor, after all, efficiency."<sup>211</sup>

The second idea is that the plan is and should become more and more an *instrument of conscious social choice*. Massé takes at the same time a humble and ambitious view of his task when he writes:

Choice among ends is a political act that goes beyond . . . the programmer's task. The latter must, however, submit sufficiently clear, precise and timely alternatives for all democratic procedures to be open. Clear enough to be understandable by the common man. Precise enough for political decisions to escape ambiguity. Timely enough, so that these decisions do not entail reconsideration of practically completed plans.<sup>212</sup>

In this connection, Massé has stressed particularly choices between present and future consumption, between work and leisure, between goods of individual consumption and collective services, between individual freedom and economic benefits,<sup>213</sup> as well as the traditional problems of income distribution, full employment versus monetary stability, and so on. By exploring within a general equilibrium framework the economic implications of such choices and connecting them with specific policy measures, it is hoped that more conscious and more consistent choices may be made. If French planning proves successful in that respect, the achievement will be remarkable, and the consequences may be tremendous. A particular difficulty stems from uncertainty considerations. To choose between an immediate advantage, for example, increased consumption or faster growth, and an abstract, immaterial one, such as a higher (subjective) *probability* of maintaining balance of payments

<sup>210</sup> Perroux [141], I, describes a policy concerned with the real factors of growth and fluctuations of which active planning might be regarded as an instance. A more formal analysis, linking French planning to the decomposition algorithm of linear programming, has recently been presented in an unpublished monograph by Malinvaud [97b].

<sup>211</sup> Massé [115], p. 24.

<sup>212</sup> Massé [115], p. 20.

<sup>213</sup> In connection with labor mobility, for instance.

equilibrium, is not easy, and the temptation is great to give systematic preference to the former.<sup>214</sup> Massé's appealing approach to this problem is to develop alternative plans, all of which would satisfy the equilibrium constraints at the same probability level.<sup>215</sup>

Obviously, the procedure described in the preceding pages for preparing a plan cannot be used for exploring systematically many alternatives—hence the need to rely upon theoretical computations for the construction of such alternatives. This brings us to the third idea underlying Massé's recent writings, namely, that much remains to be gained, within the framework of indicative planning, by heavier reliance upon formal optimization models at the level of the *Commissariat*. It is certainly revealing that three of the four papers under discussion<sup>216</sup> start with a reference to the work done at EDF in connection with investment and pricing and its progressive development from marginal toward global, and from partial toward integrated approaches. The duality property associating a price system with the quantitative optimum and the equality there between short-term marginal costs and expansion costs<sup>217</sup> are repeatedly referred to,<sup>218</sup> and they obviously constitute, in Massé's opinion, important analytical tools of decentralized planning, so far unexploited.<sup>219</sup> Perhaps equally revealing are the references to the

. . . intellectual stimulation derived from programming efforts, and in particular, from the difficulties and possible failures of initial attempts. Literary reasoning admits of imprecisions and obscurities. Cal-

<sup>214</sup> Such also seems to have been the case in connection with the fourth plan; see Massé [115], p. 21.

<sup>215</sup> See Massé [114], Section V.

<sup>216</sup> That is, Massé [113, 114, 116].

<sup>217</sup> See pp. 9 and 10.

<sup>218</sup> Massé [114], p. 15 and [115], p. 14.

<sup>219</sup> The only systematic attempt at exploring the price implications of a plan is the suggestive model of Nataf and Thionet [164]. This "middle-term model with variable prices" may be described as a means of verifying the existence of a vector of *relative* prices compatible with (i) the quantitative projections embodied in the plan, (ii) a set of accounting identities to be verified for the terminal year in terms of these prices, and (iii) a set of relations expressing the monetary behavior of the government, firms and households—relations that are expressed (as a first approximation) independently of these relative prices. Thus:

(a) the price level is left unspecified—a useful limitation;

(b) price elasticities are completely ignored—a severe limitation! The structure of the model is the following: A set of accounting identities and behavior relations for each industrial sector is reduced to a system of equations (I) (one by sector) that are linear (but not homogeneous) in the prices (or rather price indices) of sectorial outputs, with coefficients involving the direct and indirect tax rates as well as behavior parameters. Similarly, accounting identities and behavior relations for the government, households, and the rest of the world are reduced to a system of inequalities (II), linear in the same prices, with coefficients involving the same tax rates as well as behavior parameters. Thus, to any set of tax rates and behavior parameters, there corresponds a unique set of relative prices by equations (I), and these

culatation, which forces searching of the shadow areas, leads to better and more clearly-expressed ideas, even when one finally comes back to literary exposition. . . . Furthermore, formal models have logical properties that can be used for exploration and consistency tests. . . .<sup>220</sup>

These properties of formal techniques should be used for technical sub-optimization with respect to such variables as choices of activities (domestic production versus imports), of investment programs (more or less capitalistic techniques for alternative sectors), of investment types (machines versus education), and so on.<sup>221</sup> They also seem to be the only currently available techniques by means of which some longer-range considerations could be brought to bear upon current plans.<sup>222</sup> Two special research bureaus, CERMAP and CEPREL,<sup>223</sup> respectively devoted to mathematical methods of planning and long-range forecasting, have recently been set up to provide the *Commissariat du Plan* with appropriate scientific support.

It is my conviction that important developments may be expected in the future: the significant contributions made to the management of public utilities will be extended to the level of national planning.<sup>224</sup> In this way, French planning might come one step closer to the normative economist's ideal: to combine efficiently the welfare properties of decentralized market mechanisms with conscious social choices about ends, informed evaluation of future events by men of judgment, and technical optimization of policy decisions by men of science.<sup>225</sup> As for the positive economist, he now is apt to regard current developments in France as

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prices may or may not satisfy the inequalities (II), as well as general plausibility conditions (non-negativity, of course; dispersion of prices for close substitutes, etc.). If no satisfactory price vector exists, the tax rates and behavior parameters may be regarded as inconsistent.

One of the most crucial and least convincing relations of the model imposes *proportionality* between the gross investment of a sector, as given by the plan, and gross profits (the latter implying prices through the input-output relations, and affecting the government's tax receipts and the property income of households). While the behavior relations are rather rough, the accounting identities are sometimes luxurious; they entail projected money flows tables—see footnote 187 above. Serious analytical difficulties would, of course, be associated with explicit treatment of price elasticities; accordingly, numerical exploration of alternative parameter values is more fruitful at present. This might well be the type of model for which analogue computers would be ideally suited!

<sup>220</sup> Massé [114], pp. 1 and 2.

<sup>221</sup> See Massé [114], Section III.

<sup>222</sup> The experience at EDF in connection with long-range investment planning, and the advisability to push current research and experimentation with tidal and nuclear power, has revealed the usefulness of a linear programming approach to explore substitutions that a less integrated model could not reveal; see Massé [122], Ch. IV.

<sup>223</sup> CERMAP, *Centre de Recherches Mathématiques pour la Planification*, 19 rue de Passy, Paris 16, is headed by A. Nataf; CEPREL, *Centre d'Etude de la Prospection Economique à Moyen et Long Termes*, 16-18, rue Berthollet, Arcueil, is headed by J. Bénard.

<sup>224</sup> [97b] is a step in that direction.

<sup>225</sup> The hope that a more scientific approach to policy formation should prove helpful is, of course, basically an expression of confidence in the ability and objectivity of the people involved.

providing only an interesting field of investigation rather than an abundant literature of direct interest to him.

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<sup>27</sup> It is perhaps worth noting that the 88 journal articles by contemporary French economists quoted in this paper have appeared in 39 different places (28 domestic and 11 foreign), with no single French journal accounting for more than 9 papers out of the 88! More efficient communication channels could undoubtedly be devised.

## RECENT TRENDS IN ECONOMIC THOUGHT IN DENMARK

By JØRGEN H. GELTING\*

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The dominating economic aim in Denmark in the postwar period has been to achieve both a high and rising level of economic activity and equilibrium in the balance of payments. In 1956 the ministry of finance, under a decision by parliament, established a commission on which served a number of economists in addition to representatives of labor organizations and several other pressure groups. This committee reviewed the situation, under its terms of reference, in order to ascertain to what extent recent economic difficulties were due to a lack of co-operation between public and private organizations and institutions which wield a major influence on economic development and to explore how improved cooperation might be secured in the future. Among the principal conclusions stated by the committee in its report were: (1) Denmark's failure since the early 1950's to achieve rapid economic growth was due in large part to stagnating foreign demand for agricultural exports and an unfavorable development in the terms of trade; and (2) The damage from these external factors had been intensified by the efforts of labor and other organized groups to increase their money incomes, resulting in a rising domestic cost level.<sup>1</sup>

### *I. Monetary Theory and the Problem of Inflation*

The above analysis was followed by a comprehensive study of price developments in relation to employment by E. Hoffmeyer in 1960 [13].

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<sup>1</sup>The committee largely confined its analysis to developments in the postwar period and ignored the fact that at the beginning of that period the cost level in Denmark had increased appreciably in relation to most of its close competitors.

From a scrutiny of the historical record, and in particular of the post-war period, it was concluded that despite the close and quantitatively important interrelations of the Danish economy and the world market, price developments in Denmark normally cannot be explained as a direct consequence of international conditions which, although not without importance, must take second place in the analysis of the problem of inflation. Domestic price developments are viewed as the result of a struggle between employers and employees for larger shares in total income. The two principal preconditions that Hoffmeyer considers essential for a successful stabilization policy are: first, an understanding that the distribution of income is crucial in the problem of inflation and, second, an agreement between the major pressure groups concerning the distribution of income and the means to its implementation. In line with this basic view, relatively little attention is paid in the analysis to the repercussions peculiar to an open economy or indeed to the effects on employment and resource allocation of changes in wages or prices.

Diametrically opposed to this approach is the analysis of inflation [24] (1954) and general monetary theory [23] (1948) evolved by Jørgen Pedersen, who already played a leading role in the development of modern monetary and, in particular, fiscal theory [20] during the interwar period in Denmark. Fundamental to the analysis here is the proposition that in the circuit flow of income there is a basic difference between the position of, and the behavior patterns relating to, the contractually agreed income of wage earners on the one hand and the income of entrepreneurs on the other hand. As a residual magnitude, the latter cannot be directly determined by the recipients in a way analogous to that in which wages are agreed upon.

The general level of prices depends on three magnitudes: the level of wages, the marginal productivity of labor, and the degree of monopoly. The degree of monopoly depends in turn on institutional, psychological, and sociological factors. The marginal productivity of labor is governed by natural conditions, the stock of equipment, technical knowledge, and the level of employment. The level of wages remains then as the only purely monetary element among those factors which determine prices. Whereas a change in the distribution of income may be called forth by a change in the level of employment or in the degree of monopoly, the level of wages in a closed economy (assuming a completely elastic supply of credit) may be changed arbitrarily without any change in the level of employment or the allocation of resources. However, prices and residual incomes will increase or decrease in proportion to the change in wages. Essentially this analysis is based on a

fairly simple model where labor is the only variable factor of production and where, in consequence, the real wage rate is a—declining—function of the level of employment.

Apparently this analysis by Pedersen differs radically from that of Hoffmeyer (1960) [13]. After a detailed discussion of the cumulative effects of the automatic cost-of-living adjustments of wages in Denmark, Hoffmeyer concluded that following an initial increase in money wages the rate of induced price and wage increases will diminish rapidly, presumably with the implication that real wages will have increased. An analogous conclusion was reached by P. Norregaard Rasmussen in 1956 in discussing the effect on the price level of a rise in import prices on the basis of an input-output matrix with imports and labor as the only two primary factors [30]. In this instance the conclusion follows necessarily from the restrictive assumptions of the analysis. Whereas Hoffmeyer's analysis would appear simply to ignore repercussions between real wages and employment, the conclusion reached by Pedersen is explained by the different setting of the problem—the relationship between wages and prices at a constant level of employment and by the assumption that given the state of technical knowledge, the material means of production, the market structure, and so on, a definite functional relationship holds between the level of employment and the real wage rate. Under these assumptions the validity of Pedersen's conclusion is not confined to the case of a closed economy. In an open economy also, the level of money wages may be considered the only monetary element in the price system in the sense that under given conditions of technique, market structure, and so on, any change in the general level of wages must be accompanied by a proportional change in the price level—including the prices of foreign exchange—if the level of employment is to remain constant.

In conformity with this view, the equilibrium rate of exchange is defined as that rate which, in the absence of trade restrictions and at given employment levels, secures equilibrium in the balance of payments. If maintenance of a certain position of the balance of payments on current account is desired, rates of exchange will have to be adjusted in conformity with the changes in wages at home relative to wages abroad, insofar as such changes are not offset by changes in efficiency.

A natural consequence of Pedersen's theoretical model is the definition of inflation as a rise in wages, however brought about, provided that the banking system is willing to supply sufficient finance so that unemployment does not result.

Closely related to this definition is the view referred to above that

the respective roles played by wages and entrepreneurial income in the monetary system are fundamentally different. Elaborating this point Pedersen gives the following example. Suppose that in a country much dependent on foreign trade there is a great deterioration in the terms of trade or some other disturbance of the technical conditions of production, or that the employment of a rapidly growing population requires a great expansion of capital equipment leading to the development of so-called bottlenecks. In both cases there will be a rise in the level of prices which is technically, and not monetarily, caused, and which would lead to equilibrium if wages were kept constant [24, p. 71]. A crucial question here is whether an expansionary process that encounters a physical barrier due to a shortage of capital equipment while labor is still unemployed will automatically come to a halt and a new equilibrium be established if money wages do not rise. To establish the parallel to the normal case of inflation at full employment, the assumption must be of course that the prospective rate of profit has risen for some reason and that the supply of credit is elastic. As output rises and full capacity output is approached, prices will rise both absolutely and in relation to wages since the marginal productivity of labor falls. If unemployment is still considerable, wages may be assumed to remain constant. Since income is shifted from wages to profits, excess demand for commodities may be removed and equilibrium attained if the propensity to spend is lower for entrepreneurs than for wage earners (and *rentiers*). However, the rise in prices has raised the rate of profit and although the fall in wages in terms of commodities will induce a reduction in the capital intensity of new investment, the net effect may be that as prices rise total demand for consumption and investment will increase as fast as or faster than the value of total supply so that the gap between demand and supply is not closed.

Adopting the point of view that inflation should not be measured by commodity prices but by the price of labor, i.e. wages, Pedersen claims that: "it is obvious that the effect of the movement of wages on the behaviour of entrepreneurs is the same whether the rise in wages exceeds or falls short of the rise in effectivity." [24, p. 73]. However, this could hardly be correct; commodities can be stored, labor cannot. If wages after having been stable start rising by 4 per cent per year and in one case productivity is unchanged so that prices rise at the same rate, while in another case productivity also rises so that prices remain stable, entrepreneurs will in the first instance, but not in the second, be induced to accelerate purchases if the rate of interest is unchanged.

In line with contemporary theory, Pedersen defines the rate of interest as the price of money. Since the supply of money is ultimately

determined by the monetary authorities, the rate of interest becomes a policy parameter. In relation to the economic system, the rate of interest is, in other words, an exogenous magnitude depending upon the action of public authorities. It does not follow that the level of the rate of interest may be fixed completely arbitrarily without serious consequences. If the rate of interest is held below a certain level, the result will be uncontrolled inflation. If in contrast the rate of interest is raised above a certain level, economic activity will contract to a politically intolerable degree. The propensity to consume and the inducement to invest, which according to theories along classical lines determine the rate of interest, influence the position of the ceiling and the floor to the rate of interest as defined above. However, the decisive point is that the system contains no effective mechanism which automatically adjusts the rate of interest when either the propensity to consume or the inducement to invest changes.

It is difficult to judge whether there is a difference of principle or only of emphasis between this point of view and that presented by Hoffmeyer in 1958 [12]. After consideration of both historical evidence and of a Keynesian model, Hoffmeyer concludes that in the long run the rate of interest is determined by real economic conditions although in the short run the policies pursued by the monetary authorities may be of considerable importance.

Drawing in part on his own research, Pedersen [25] observes that the rate of interest charged for bank loans has little or no influence on the volume of bank credit. On the other hand, credit rationing by banks is deemed irrational from every point of view as causing great disturbance to the economy. It is not concluded from this that credit policy as an instrument for the control of economic activity should be abandoned. The demand for bank credit is largely a function of the rate of interest ruling in the capital market. The central bank may therefore control the demand for credit through open market operations. However, it is held that the central bank should make no attempt to correct temporary disturbances in economic activity or in the balance of payments, but should see its only objective in the long-run adjustment of investment demand to the resources available in a fairly fully employed economy.

Based largely on Pedersen's analysis, J. H. Gelting, in 1948 [6], discussed fiscal policy as an instrument for controlling the level of economic activity. Representing fiscal, credit and exchange rate policies by the three policy parameters: using  $t$  for level of taxation,  $i$  for interest rate, and  $v$  for the rate of exchange, the interrelations of the three branches of economic policy may be illustrated by the equations

( $F$  = foreign balance,  $K$  = capital exports, other symbols conventional):

$$(1) \quad Y = C(Y, t) + I(Y, i) + F(Y, v)$$

$$(2) \quad K(i) = F(Y, v)$$

Given the desired level of activity and thus of national income (at given constant wage rates), the above equilibrium conditions leave one degree of freedom in the choice of the values of policy parameters—any two may be represented as functions of the third. Based on Danish national income data for the interwar period, an attempt was made to ascertain (1) values of the marginal propensities to consume, invest, and import; and (2) the elasticity of investment with regard to the rate of interest for which a numerically high value of about two was found. These results were combined with a fairly detailed multiplier analysis in a discussion of the effects of alternative fiscal policies and the degree of built-in stability. As was usual in the Danish discussion of the early postwar period, much was made of the greater predictability of the effects of fiscal as compared with credit policy. The most important (or, at least, theoretically most interesting) argument was that the fundamental relation between the rate of interest and the desired stock of capital is of little help in predicting the effect of a *change* in the rate of interest on the *rate of change* of the stock, i.e. investment.

Observing that a number of propositions implied by the quantity theory of money would become less questionable if the word "income" was substituted for "money," Pedersen, in 1944 [22], suggested a corresponding terminological change. Earlier K. Philip [26] had made a highly original attempt to base the analysis of the activity effects of fiscal policy on the changes produced in the cash position of the private sector. Although this approach may have induced, to some extent, an underestimation of the differences between various types of fiscal intervention, it was not necessarily a consequence of the method adopted.

Recently, J. Vibe-Pedersen [32] (1961, 1962) contributed to monetary theory through detailed elaboration of the multiplier analysis. Preliminary to a comparison of the effects of direct and indirect taxes, he discusses the form of the consumption function and, in particular, the implications of the existence and non-existence of money illusion is discussed. The main analysis is based on a disaggregated model in which distinction is made between entrepreneur and wage earner households, investment and consumer goods production, and within the last sector between the production of material goods and services. An increased generality is imparted to the analysis by dropping the restric-

tive assumption of constant prices relative to wages. Thus, real and money income multipliers may be distinguished and the distributional effects of economic expansion are analyzed in terms of the two crucial elasticities of output in respect to employment and prices as related to output.

Pursuing disaggregation along rather different lines, I. Grünbaum [10] (1945) discussed the conditions of monetary equilibrium in terms of the *ex-ante*—*ex-post* analysis of the Swedish school. By showing that in spite of overall equality between *ex-ante* savings and investment, the coexistence of opposite discrepancies between demand and supply in the markets for goods and for factors (labor) might give rise to cumulative deviations from equilibrium, Grünbaum's paper proved fruitful by providing one of the leading ideas in Bent Hansen's "Theory of Inflation" [11] (1951). Although under normal conditions the coexistence of excess demand in the labor market and excess supply in the market for goods would appear highly improbable, this construction was presumably relevant to conditions in some western European countries, and in some of the less developed countries during the early postwar period.

A considerable number of both historical and theoretical studies of the Danish money and capital markets and their institutions have appeared since World War II, in particular "Bankerne og Samfundet" [34] (1955), T. Glud [8 and 9] (1951 and 1957), Hoffmeyer [14] (1960), E. Rasmussen [28] (1955), Uldall-Hansen [31] (1959), and P. Winding [33] (1958).

K. Bjerke and N. Ussing [1] made a study of the Danish national product 1870-1950. Methodological problems of the input-output analysis were discussed by Nørregaard Rasmussen [30] (1956). The input-output structure of the Danish economy in postwar years was investigated by Leo Meyer, Leif Heltberg, and O. Bak Olsen [18] (1962).

## II. *Theory of Wages*

In 1954, P. Milhøj [19] traced the development of wages in Denmark from 1914 to 1950, partly on the basis of previously unpublished material. An audacious attempt was made to construct a series of figures on the share of wages in national income on the basis of indices of wages and of total employment. Although the results may have a large margin of error due to the absence of systematic labor force statistics and of adequate wage statistics in Denmark, the movements in the share of wages in national income mostly conform to theoretical expectations. Rather surprisingly, however, it was found that during

the depression of the 1930's the share of wages in the national income declined substantially from 1930 to 1933. Part of the loss appeared to be restored in the following years despite the expansion of employment under difficult conditions, characterized by unfavourable terms of trade and a relatively lower foreign trade turnover than in the preceding decade. The implications for the distribution of real income in these rather uncertain movements in the share of wages were modified by the sharp fall from 1930 in the relative prices of food. During World War II, when employment increased strongly at the same time productivity was unfavourably affected by dwindling supplies of imports, the wage share declined to a minimum for the whole period. After the war, the wage share rapidly reattained the level of the late 1920's. Developments in more recent years are covered by Hoffmeyer [13]. It appears that the increase in the share of wages continued at a slow rate until the mid-1950's. For the period from the mid-twenties to the end of the 1950's, there was little over-all trend in the wage share. However, this stability was the net result of a pronounced decline in the share of wages in agricultural income due to the migration of workers out of agriculture as well as to a moderate increase in the share of wages in income produced by other industries. At least in the later part of the period, this rise did not reflect an increase in the share of production workers' wages but rather a relative rise in the number of administrative personnel.

Principal attention was given by P. Milhøj [19] (for the period of 1914-1950) to the evolution of wage structure, which among the organized trades shows a remarkable stability, although generally with a slight reduction in wage differentials between skilled and unskilled workers and between the different skilled trades. In covering the post-war period, A. H. Dahl [2] also finds a considerable stability in the industrial wage structure. Repeated attempts have been made to effect general wage agreements which would narrow differentials between skilled and unskilled workers. In addition the automatic cost of living adjustment of wages in Denmark tends to reduce wage differentials. Nonetheless, shortly after each new agreement, market forces, operating through wage drift, have largely re-established the previous wage structure and in the end have produced little change in relative wage differentials.

However, in certain sectors of the labor market previously characterized by relatively low wages and a low degree of organization, wages have increased strongly, resulting in a pronounced reduction of wage differentials. Most important among these is the field of agricultural wages. The fundamental analysis of this development was provided by

Pedersen in 1945 [21] in a discussion of the implications of full employment for the cost of living. A permanently higher level of employment outside agriculture would call forth a migration of workers from agriculture to other industries which would result in a narrowing of wage differentials and a consequent adjustment in the relative price of food.

The discussion of the evolution of wage structure has been closely connected with that of the extent and causation of wage drift, which has attracted interest mainly as a presumed major cause of Denmark's balance of payments difficulties. Pedersen, in his analysis of inflation, [24] makes a distinction between autonomous inflation due to the action of trade unions and induced wage increases caused by excess demand in the labor market—evidently a distinction corresponding quite closely to that between cost-push and demand-pull inflation. Investigating early postwar developments, A. Lund [16] found that the extent of wage drift varied with the level of employment and that, insôfar as the results of wage drift had not been codified in wage agreements, a subsequent decrease in employment might lead to a reduction of wages by removing wage increases over and above agreement rates.

In 1959, Dahl [2], who was interested mainly in the question of wage structure, viewed wage drift primarily as the way in which market forces, i.e. relative scarcities of different types of labor, corrected the wage structure established through wage agreements. Hoffmeyer [13] made an intensive investigation of the question of wage drift. Although it is not denied that the state of market demand is an important influence, other factors may be equally or even more important, such as the system of wage payment or the wage policy of the individual firm. As appears from a subsequent discussion (Gelting [7], Hoffmeyer [15]), these negative findings relating to the influence of demand are in large part to be explained by the fact that they are based on a period in which the level of unemployment in Denmark was comparatively high. If the period of observation had been extended to include subsequent years in which the level of employment rose appreciably, the influence of the state of market demand presumably would have been judged to be higher. It would appear then that the extent of wage drift is determined predominantly by the state of demand when, and only when, the level of unemployment is comparatively low—a conclusion which accords with the experience in other countries.

### III. *Farm Economics*

H. Gad [4] investigated the development of the agricultural labor force since the beginning of the 19th century. In a subsequent work

[5] a more detailed account is given of the changes in the size and structure of the Danish agricultural labor force and of the changes in agricultural wages from the middle of the 1930's. The movements in agricultural wages are explained by the decline in employment in agriculture, the increase in prices of farm products, changes in technical conditions inside agriculture, and developments in wages and employment outside agriculture.

Primary interest attaches to an audacious attempt to determine the demand curve for labor in Danish agriculture on the basis of farm accounts. The analysis covers six five-year periods from the middle of the 1920's to the middle of the 1950's. It was noted that on farms above 30 hectares the marginal input of labor is supplied by hired workers, whereas on smaller farms the entire or almost the entire labor input is supplied by the owner of the farm and his family. From these observations it was concluded that on the larger farms the farmer will attempt to adjust the input of labor in such a way that the marginal value-product of labor equals the money wage, whereas on the smaller farms the input of labor will be pushed to the point where the marginal value-product of labor is equal to the subjective supply price of the farmer, which may be lower than the market wage because of the absence of alternative income opportunities for the farmer. Typically the input of labor per hectare is much larger on the small farms than on the large farms. Thus, by comparing the economic results from large and small farms one might be able to determine the marginal productivity curve for labor.

However, for a number of reasons the author's interpretation of the results is open to doubt. First, the differences between small and large farms in the input of labor are accompanied by differences in the structure of output and, in particular, the input combinations of other factors of production. Therefore the differences in economic results between small and large farms cannot be claimed to reflect the marginal productivity of labor in the usual sense of this term. Secondly, the observed differences in economic results may reflect mainly returns to scale. Finally, in order to arrive at the marginal value-product of labor, the returns to other factors of production have been deducted from gross income. These deductions include a normal return, put at five per cent, on the estimated market value of farms. The input of both labor and capital per hectare is larger on the small than on the large farms, and market values of farms are not an adequate measure of the input of real capital. The smallholder accepts a relatively low remuneration for his own labor and capital, and it cannot, without arbitrariness, be determined to what extent this reflects a less than normal return to labor and to the capital invested in the purchase of a farm.

Also based on farm accounts collected and analyzed by the Danish Institute of Farm Management and Agricultural Economics, Knud Rasmussen [29] undertook a variance analysis of farm results and the calculation of agricultural production functions. The measure of farm results used is the net return expressed as a percentage of capital. The net return arises as the differences between gross income (including rent of the farm house and the value of products consumed in the farmer's own household) and the value of all inputs including imputed wages to the farmer and his family, but excluding interest on capital. In calculating the percentage return, two statements of capital value are used. One is the book value based upon tax valuations of all physical assets. The other, the market value, is an estimate of the price which the farm as a going concern could bring in the free market. This price is estimated by the accountancy consultant assisted by the farmer and is based on the current sales prices of similar farms in the open market.

It is found that for any single year the variance of financial results is reduced little by a classification of farms into regional and size groups. For any single year the total variance may be divided into two roughly equal parts, a random element and a so-called managerial element. If the financial results for each farm are averaged for several years, the importance of the random element is very much reduced, the managerial element accounting for by far the larger part of the total variance.

Whereas the variance analysis is based upon farm accounts for the 25 year period from 1925 to 1950, the production function study covers a five year period after the war. The coefficients of two Cobb-Douglas functions were determined: one in which net product is expressed as a function of labor and capital, and one expressing gross product as a function of the inputs of labor, capital, and raw materials. In the first function the author finds elasticities of 0.86 in respect to labor, and 0.19 in respect to capital, whereas in the case of the three independent variables, the elasticities found are 0.51, 0.13, and 0.39 in respect to labor, capital, and raw materials. Thus, in both cases the elasticities add up to slightly more than unity. While the inclusion of raw materials as an independent variable in the production function affects only slightly the extent of the residual variation, it has, of course, a great effect upon the regression coefficients for labor and capital. As it is found that on most farms raw materials have been utilized in suboptimal quantities and since variations in the input of labor and capital are associated with variations in raw material input, preference is given to the production function in the form with three independent variables.

On the basis of the production function analysis, the author investigated to what extent the ratio of gross product to total costs, including a return of four per cent on capital (estimated market value), was affected by the global combination of inputs and by the managerial variance, respectively. He reached the conclusion that the main factor in determining efficiency is the managerial element.

As in the case of the study by Gad, doubts may be raised as to the appropriateness and relevance of the capital concept used. Presumably the market value of a farm reflects an estimate of its future earning power and is not closely related to the present construction cost of real capital on the farm. When it is found in the variance analysis that taking periods of several years the importance of the random element in the variation of farm results diminishes rapidly, whereas the so-called managerial element becomes preponderant, it is possible that the influence of the managerial element is understated because in part the competence of the farmer is capitalized in farm value. On the other hand, it may be that estimates of farm values differ between farms in a way unrelated to managerial ability. In the concluding analysis of the relative importance of managerial competence and input combination to the ratio of gross product to total cost (including four per cent return on capital), the result found—the dominating influence on the managerial element—may in part merely reflect the fact that since costs are incurred in changing any established combination of real inputs, its efficiency affects the farm valuation, which measures the normal earning power of the farm rather than any aggregate of physical resources.

#### IV. *Price Theory*

Bjarke Fog [3] (1958) based a study of price policies on interviews with about 150 Danish industrial firms. Full cost calculation was found to be the procedure most frequently adopted with a view to long-run profit maximization as modified by other considerations. Thus, in some cases more importance appeared to be attached to the maximization of annual turnover than to that of profits. In large part, full cost pricing might be interpreted as a defense reaction against ignorance, concerning not only conditions of demand, but also of cost. E. Rasmussen [28] discussed the implications of firms using policy parameters other than price to promote sales in 1955. Most recently, 1963, E. Lykke Jensen [17], has made an important contribution to the theory of price determination under monopoly and imperfect competition. Price determination is analyzed on the basis of a stochastic model where the producer = seller considers demand as a random variable

with price as a parameter in the distribution function. In contrast to the deterministic case, it is here possible to define a supply function by equating marginal cost to marginal increase in sales expectations. Under certain rather restrictive assumptions, it may then be proved that equilibrium will be achieved at an appreciably lower price than in the non-stochastic case.

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# SURVEY OF ECONOMIC RESEARCH IN POSTWAR JAPAN†

## MAJOR ISSUES OF THEORY AND PUBLIC POLICY ARISING OUT OF POSTWAR ECONOMIC PROBLEMS

By SHIGETO TSURU\*

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The economics profession in Japan today is rapidly expanding although the number of economists is still relatively small, probably less than 2,000 altogether, if we exclude specialists in business administration. It is no exaggeration to say that close to one-half of the economists in Japan today are of Marxist orientation.<sup>1</sup>

Academic Marxism became a substantial force in Japan in the late 1920's and the early 1930's just as the capitalist world was plunging into the Great Depression and the Japanese were moving in the direction of attempting to find violent solutions for internal ills via external aggression. Marxism offered a method of analyzing these problems in historical perspective; and in particular, it served as a unifying philosophy for a group of young, brilliant historians and social scientists who collaborated in the interpretive work on the origin and characteristics of Japanese capitalism. The dominant group among them, known

† This article was written originally in 1961 and has been brought up-to-date only at minimum points where necessary. Some paragraphs, however, were added in December, 1963, at the suggestion of the editor.

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<sup>1</sup> One could verify this assumption through laborious enumeration. However, one shortcut would be to analyze the composition of the 30-member Third Division (Economics, Commerce and Business Administration) of the Japan Science Council, body of scholars elected every three years by the registered constituency of scholars in their respective divisions. The composition of the present panel, elected at the end of 1962, appears to show a slight majority of Marxian scholars, even though the panel includes representatives of the branch of "Commerce and Business Administration" who are overwhelmingly non-Marxist.

as the *Kōzō-Ha*,<sup>2</sup> and their works were duly suppressed by the militaristic government of the time. But they were an inspiration for many intellectuals who were opposed, either openly or more often secretly, to the policy of aggression pursued by the ruling clique of Japan. Although the method of analysis was imported from abroad, these Marxist scholars wrestled with the problem of interpreting the history of their own country, and a rich crop of by-products in the form of historiographic studies for the early Meiji period emerged. During this time, a minority of neo-classical economists were largely content with the work of what, in essence, was little more than the translation of western literature in the field.

The war period changed the apparent picture greatly but hardly affected the composition of Japanese economists in terms of their methodological orientation. While a large number of non-Marxist economists collaborated with the government in one way or another, most Marxists, either in jail or at large, turned their attention either to the highly academic study of doctrinal history, especially of the classical period, or to minute research in historiography, less that of Japan than of western countries.

### *I. Growth in Economic Thought in Postwar Japan*

The end of the war, naturally, saw extreme collaborator-economists purged from academic posts and the coming into the open, now with secured positions, of a large number of Marxist-economists from hiding that had been either enforced or voluntary. To all economists regardless of ideological orientation, the concrete problems of postwar Japan presented a challenge. Immediately there was a problem of inflation which, by the time it subsided in 1949, left the general price level at about 300 times that of 1934-36. Many of the tasks of adjustment which the defeated country was called upon to make were mainly economic in character: the need to absorb upwards of six million repatriates who were forced to return home from either civilian or military jobs in the countries which Japan had invaded; the need to reorient the structure of the economy from one which depended to a high degree on the international division of labor to one, at least for several years, of autarchic character; and so on. More important, however, were the basic tasks of reform. Ideas prevalent in the minds of policy advisers attached to the Occupation authorities at the initial period after the surrender were marked by a certain historical perspective which found a logical connection between the aggressive militarism of

<sup>2</sup> See Martin Bronfenbrenner, "The State of Japanese Economics," *Amer. Econ. Rev.*, 1956, XLVI (2), 389-98; especially pp. 390-1. The article, on the whole, is an illuminating study.

Japan and the peculiar socio-economic characteristics which had developed since the days of the Meiji Restoration (1868). At the time, these ideas were shared by a large number of the Japanese themselves—although common diagnosis did not usually lead to common therapy. In particular, it was agreed that something had to be done with the peculiar monopolistic structure of prewar Japan, the *zaibatsu*, which had been in collusion, if clandestine, with the military clique. It was also agreed that the agricultural sector of prewar Japan had lagged woefully in many aspects of the task of modernization and for this reason had been exploited by other sectors of the society in many ways. It was further agreed that the legacy of feudalistic mentality and institutions had its economic counterpart in the form of certain rigidities which prevented the mobility of labor and contributed to the persistence of dual structure in employment.

These and other problems, some short-run and some long-run, called for solution, at least an attempt at solution, in the minds of economists in the early postwar years. True, the role of the Japanese during the Occupation (1945-52) was secondary, often discouragingly so. Especially after the shift in the United States' policy on Japan in early 1948—a shift "from reform to recovery"—the erstwhile atmosphere of fluidity in the patient search for solutions disappeared and there came to be crystallized within Japan a social force which identified, quite rightly, the return to "normalcy" with rapid economic recovery. Nevertheless, here was a set of crucially important economic problems that deserved the close attention of Japanese economists, whatever methodology or conviction they might have followed. And yet it is questionable if they responded to this challenge with commensurate vigor and seriousness. Such a state of affairs calls for an explanation.

The basic cause, I venture to suggest, was a kind of scholasticism<sup>3</sup> which prevailed among the majority of Japanese economists at the time. Both the Marxian and the neo-classical (including the Keynesian) economists tended to depend on derived authority of one kind or another. For the Marxian group, the source of the authority was, of course, Karl Marx himself. At least in this case the source of the authority had long since been dead; any attempt at avoiding sterile scholasticism meant the confrontation of the method with the facts of life. Efforts to pursue such a confrontation, even if not too successful, generated the remarkable vigor of the Japanese Marxian group in the 1930's. However, the enforced hibernation of almost a decade made

<sup>3</sup>I coined the expression "doctrinology" (*Keisai-gaku-gaku*) at the time. If "economics" (*Keisai-gaku*) can be thought of as "a body of doctrines relating to economic facts of life," "doctrinology" would be a study of that body of doctrines as if such a study could replace "economics" itself.

many of the leading members of this group into doctrinal specialists, and the end of the war found most of them unprepared to cope with the burning problems of the day. Modern economists,<sup>4</sup> on the other hand, had the tradition of deriving their authority from recognized authorities abroad—who, needless to say, were on the move, with new stars and new ideas rising on the horizon. This situation kept modern Japanese economists moving, seldom however in a position other than following the path of pioneers abroad. Yes, they were on the move as “doctrinologists,” but most of them did not stop, as Ricardo and Keynes did, to concentrate on current economic problems of their own country. Thus again the end of the war found this group, to the extent they survived the immediate postwar purge, unprepared to offer their service effectively.

Nineteen years have now passed since the war ended. And the economics profession in Japan today presents a different picture. The division of the profession into the two groups still remains, but they are able to communicate with each other better. The erstwhile ailment, which Bronfenbrenner called “inbreeding,” still lingers, keeping intact the guild-like association of economists around respective masters. But here too, one cannot fail to observe a new trend of “intermarriage” on the rise.

What appears to be distinctly new, with dynamism of its own, is the shattering of derived authority among modern economists. Whereas the Marxian group still derives its authority essentially from the master of 100 years ago, “doctrinological” maturity helps the elders maintain their authority. The other group, however, has outgrown the tradition of derived authority and has produced a number of young economists who can stand on their own feet in the front rank of the economics profession of the world. Hiroshi Furuya, who died young in 1957, was probably the first to appear in this sense, and he was followed by a large number of young theoretical economists adequately trained in mathematics. In fact, one is struck by a sharp contrast in the list of Japanese contributors to *Econometrica* before and after what appears to be the distinct divide of 1951-52. The April 1951 issue of *Econometrica* printed a report of the Tokyo meeting at which the Japanese branch of the Econometric Society was organized. It is odd that practically all the participants who read papers on that occasion, except Michio Morishima, have not contributed any articles to *Econometrica* since 1952, and one gets the impression that the meeting was, most ironically, an occasion for transferring the baton from the old genera-

<sup>4</sup>This is the expression we shall use throughout this paper to denote all the groups of non-Marxian economists, such as the neo-classical, the Keynesian, the mathematical, and so on.

tion to the new. The new generation of mathematical economists, led by Morishima, progressed in the subsequent period with such suddenness and brilliance that very few of the generation now above fifty years of age could follow them. Since 1954 the pages of *Econometrica* have regularly seen contributions by young Japanese economists such as Morishima, Hirofumi Uzawa, Takashi Negishi, Hukukane Nikaido, Ken-ichi Inada, and others.<sup>5</sup> By now it may be said without exaggeration that it is in this field of mathematical economics, more than in any other, that Japanese economists stand in the front rank of the world's economists.

Along with this development of mathematical economics, there has emerged, with the mutually-reinforcing growth of statistical studies, a host of econometricians who have less respect for "doctrinology" than for facts. Among the latter are: Shin-ichi Ichimura in the field of input-output studies, Miyohai Shinohara and Tadao Uchida in the manifold econometric analyses of the Japanese economy, and many others who have given a new direction to a profession that is attracting an ever-increasing number of younger people.

In other words, there is new vitality gripping modern economists, with a younger generation rapidly replacing the old. Faced with this situation, Marxists are also on the move. While they seem to find it harder to extricate themselves from the tradition of derived authority, they are vitally concerned with the broader politico-economic work to which they are peculiarly suited and which by default escapes the attention of modern economists. They are equally concerned with the new problems arising from the experiences of numerous socialist countries which now occupy one-third of the globe. If we are to address ourselves to "major issues of theory and public policy arising out of postwar economic problems in Japan," we shall find that the discussion of the major problems of reform, such as *land reform* and *anti-monopoly measures*, has been conducted largely by the Marxist economists. At the same time, the problem of the *growth-rate* has been

<sup>5</sup> Morishima's first contribution to *Econometrica* in full-length article form was "Consumers' Behavior and Liquidity Preference" in the issue of April 1952. All others started contributing since 1954. Some of the titles may be mentioned here: Morishima, "Prices, Interest and Profits in a Dynamic Leontief System" (July 1958), "Some Properties of a Dynamic Leontief System with a Spectrum of Techniques" (October 1959), "Economic Expansion and the Interest Rate in Generalized von Neumann Models" (April 1960); Uzawa, "Prices of the Factors of Production in International Trade" (July 1959), "Market Mechanisms and Mathematical Programming" (October 1960), "The Stability of Dynamic Processes" (October 1961); Negishi, "The Stability of a Competitive System: A survey Article" (October 1962); Nikaido, "On the Programming of Nonlinear Production Functions" (January 1954), "Stability of Equilibrium by the Brown-von Neumann Differential Equation" (October 1959); Inada, "Alternative Incompatible Conditions for a Social Welfare Function" (October, 1955).

mostly the concern of modern economists, with the issues related to the *dual structure* of employment commanding the attention of both. Meanwhile, the task of producing and collating *statistical series* of all kinds has fallen mainly on the shoulders of economists and statisticians within the government. The subject matter italicized above will form the topics of discussion in the pages to follow.

## II. *Land Reform*

The postwar land reform of Japan (1946-1950) is often cited as an example of the few reforms which were initiated and directed by the Occupation and yet survived "the back to normalcy" reversal of the subsequent period. The reason for this is not hard to find. Instead of having to impose alien ideas on a reluctant population, the Occupation found both academic and administrative experts on agriculture in Japan eager and competent to carry out a type of reform the Occupation had envisaged. In prewar Japan, numerous volumes of research and analysis were published that showed the mechanism through which the peculiarly parasitic character of Japanese landlordism contributed to solidifying the hold of militarism upon the country. About one-half of the cultivated land was under tenancy, each tenant family cultivating, with little incentive from modernization, slightly over two acres on the average and paying *in kind* the exorbitant rent of more than 50 percent of the gross product. The bare subsistence condition of the tenant class induced them to supply labor from their families to urban factories at an extremely low wage rate, thus setting the tone for the wage level in manufacturing in general. In other words, it was clear that *economic* democracy could not be brought about without a drastic change in the tenancy situation in agriculture. Suggestions for reform had been *in utero* among the Japanese themselves even before the war; and the role played by the Occupation, having uncontested political authority, was essentially that of a midwife.

The land reform consisted of: (a) the forced sale by landowners to the government of land above a certain maximum<sup>6</sup> at a price based upon the official price of rice in 1945;<sup>7</sup> (b) the sale by the government of thus purchased land to the tenant class as well as to the less-than-minimum landowners at a nominal price; (c) the setting of a legal maximum on rent, now to be paid in money, at a level which in effect turned out to be less than three percent of the gross produce per area; and (d) the establishment of Agricultural Land Committees in each

<sup>6</sup> Zero for absentee owners and 2.45 acres for resident owners except those in Hokkaido, who were permitted to retain 9.8 acres.

<sup>7</sup> The official price of rice rose more than one hundred times between 1945 and 1949. Black market price, of course, was higher than the official price throughout this period.

locality, with a greater representation for the former tenant class than for the landowners, to supervise the faithful execution of the reform. The reform was carried out faithfully, reducing the tenant-cultivated area to 9.4 percent of the total by 1950, and weakening markedly both the economic and political powers of the former big landowning class. Average area cultivated per family came down slightly to something less than two acres. However, it cannot be doubted that a major transformation in Japan's rural structure took place here—a transformation from a rural life largely characterized by poverty-stricken tenants to one consisting predominantly of small cultivating landowners who were given the incentive, as well as the capacity, within limits, to improve their lot.<sup>8</sup>

As soon as the reform was launched, an academic controversy arose that focused on the problem of whether the reform did imply a kind of revolution in rural areas for economic and political democracy. The erstwhile *Kōzō-Ha*, mentioned earlier, and their followers, who were the most ardent advocates of a thorough-going land reform, were apparently dissatisfied with the extent of the reform, and pointed out in particular that: (a) The Land Reform Act of 1946 did not touch the extremely skewed distribution of forest-land ownership, leaving the economic power of big landowners intact; (b) non-cultivating landowners could still exploit tenants if the ownership was within the maximum set by the Act; and (c) in any case, the average total area cultivated by a single family, often fragmented over a wide area, was still so small that there was little hope of raising one's standard of living as a farmer. Negative views of this type were rather prevalent in the early stage of the reform and are best represented by the writings of Haremaru Inoue, which constituted a part of the ambitious eleven-volume series called *Nihon Shihonshugi Kōza* ("Study Series on Japanese Capitalism").<sup>9</sup>

Against such negative views was pitted a positive one championed especially by Hyakuju Kurihara, whose *Gendai Nihon Nōgyō Ron* ("A Treatise on the Japanese Agriculture Today"), 1951, could be counted as a classic in this field. Kurihara discusses the political sig-

<sup>8</sup> The most objective, thorough-going account of the land reform can be found in Nōchi Kaikaku Tenmatsu Gaiyō ("General Account of the Instituting of the Land Reform"), edited by the Committee for Recording the Land Reform, 1951. The study was sponsored by the Ministry of Agriculture and Forestry, mobilizing several score of experts in the field. It gives an accurate account of the legislative process which gave birth to the Land Reform Act of 1946, traces in detail how the Act was carried out, analyzes the consequences of the reform, and is concluded with a discussion on the historical development of the land tenure system in Japan.

<sup>9</sup> Published by Iwanami Shoten in 1953-54. Inoue's contribution, "The Land Reform and the Semi-Feudal Character of Japanese Capitalism" is included in the fifth volume of this Series published in 1953.

nificance of the reform apart from its economic significance. Political significance, according to him, lay in "uprooting the basis for ultra-nationalistic ideology as well as in providing an effective buffer against communistic encroachment." As for the economic significance, Kurihara emphasized the consequent liquidation of the semi-feudal type of exploitation on land based upon the status-power of the big landlord class and the 50-60 percent rent extortion in kind. It was admitted that rich farmers still existed after the reform, deriving their income from large forest areas and/or from commercial and financial activities of all sorts. But their economic power came to be blended, much more so than ever before, with the interests of the capitalistic class as a whole. In other words, Kurihara maintained that through the land reform Japanese capitalism became more integrated and more rationalized.

The controversy between the two opposing views above, which dominated the scene in late 1940's and early 1950's, ended with the gradual retreat of the negative view. It is difficult now to find an expert in the field who would still maintain that the Land Reform Act of 1946 accomplished little or nothing. Meanwhile, however, the positive view, held typically by Kurihara, also has come to be modified. For one thing, while the major debate was going on, empirical research of all kinds on different aspects of Japanese agriculture was being carried out quietly by scholars, not only of Marxist orientation, but also of non-Marxist.<sup>10</sup> Then again, as reshuffling in the initial decade of the reform was completed and the rural area became stabilized with the post-reform configuration in ownership, production, and trade, a new set of problems arose. In a way, it was to be expected that this would be the case. As the agricultural scene became uniformly characterized by small proprietorship, the competition in the introduction of labor-saving machinery and other cost-reducing devices was heightened. By 1958 this trend went through a cycle as far as it could within the given

<sup>10</sup> A group of Marxists, called the *Rōnō-Ha*, which was opposed to the *Kōsō-Ha* in the 1930's had held that the Meiji Restoration (1868) was essentially a bourgeois revolution, which implied that Japanese agriculture had already liquidated the remnants of the feudal period at the time of the Restoration. If one holds this view, one is prone not to lay too much emphasis on the significance of the postwar land reform. Thus the economists who belonged to the *Rōnō-Ha* were much less active than others in participating in the controversy referred to above and instead busied themselves with empirical research of all kinds. The most notable among them is Tsutomu Ōuchi, whose *Nihon Nōgyō no Zaiseigaku* ("Japanese Agriculture and Public Finance"), 1950, is an undisputed classic. But more recently a most ambitious work of collective research has come out under the general editorship of Seiichi Tobata, *Nihon Nōgyō no Zenbō* ("A Complete View of Japanese Agriculture"), 1956-61, which in five thick volumes deals with practically all the aspects of Japanese agriculture, mobilizing again several score of experts of different ideological orientation. Among the works by non-Marxist economists we should not fail to mention the energetic contribution by Kazushi Ohkawa whose *Nōgyō no Dōtai Bunseki* ("Dynamic Analysis of Agriculture") 1954, is an exemplary sample.

production function. There is no doubt that productivity in agriculture rose markedly during the post-reform decade. But at the same time it has become clearer than ever that the limitation in the scale of operation, which on the average has shrunk to less than two acres even when the total family holding is added together, is beginning to tell. A two-acre farm can use only a limited-sized mechanical plough economically. While the marginal productivity of capital is thus showing itself to be rapidly on the decline, the low income elasticity of demand for rice and other agricultural commodities is affecting the demand side unfavorably. Theoretically, this is a situation that should induce a movement of the labor force from agriculture to industry if the manufacturing sector is growing rapidly at the same time. In fact, the latter has been flourishing at an unprecedented rate during the past decade. Yet the total number of agricultural households remained more or less stationary between 1950 and 1960, declining only slightly from 6,176,000 to 6,025,000. By 1959, it has been estimated, the average real consumption level of agricultural families rose by 45 percent compared with the prewar normal, whereas that of urban families rose by less than 30 percent. The trend now appears to be being reversed. Even with the sizable annual deficit in the special budgetary account of the Basic Food Control Bureau, which absorbs the negative margin between the producer's price and the consumer's price of grains, the agricultural class is finding it extremely difficult to maintain the *income* parity with the wage-earning class. Problems involved here are legion; and the attention of economists is directed more and more to this new focus which threatens to be of as grave dimension as that of the recent land reform.<sup>11</sup>

### III. *Anti-Monopoly Measures*

It has been observed by many that the twin economic pillars which supported Japanese militarism were the semi-feudal form of exploitation in agriculture and the unique monopoly structure called the *zaibatsu*. Thus the program for economic democratization in Japan after the war immediately placed on its agenda two basic problems of reform: the distribution of land to the landless and the dissolution of the *zaibatsu*. Of the two, the former was the easier since land reform was considered not only as a matter of social justice, but also as conducive to better economic performance in agriculture. Anti-monopoly

<sup>11</sup> The Basic Agricultural Problem Research Commission, appointed by the government, has been wrestling with this problem and has made public an interim report on their deliberation, *Nōgyō no Kihon Nondai to Kihon Seisaku* ("The Basic Problems and Policies for Japanese Agriculture"), 1960. This report became the basis for the Agriculture Standards Act of 1961.

reform, on the other hand, was much more complex. Here, for one thing, the thinking of the Occupation was not in agreement with that of Japanese experts in the field—for the very reason that the objective was different.

In the initial period of the Allied Occupation, the problem of the *zaibatsu* presented itself to the occupying authorities less as a question of hindrance to democratization or to rational allocation of resources than as an object of censure for presumably having worked hand in glove with the militarists in an unholy alliance to conquer the world. In addition, the Occupation policy was originally oriented toward keeping Japan's standard of living below that of any of the countries which she had invaded.<sup>12</sup> If the nation's standard of living was to be artificially restricted to a level below its own potential, the matter of attaining maximum economic efficiency could not be of prime consideration. Practically all the anti-monopoly measures under the Occupation were put into effect during this initial period, that is, before the end of 1947. The terms of reference were contained in the directive of the President of the United States, dated September 6, 1945, which favored "a program for the dissolution of the large industrial and banking combinations which have exercised control of a great part of Japan's trade and industry." First, the key holding companies at the apex of the *zaibatsu* structures were required to transfer their stocks to the Holding Company Liquidation Commission to be disposed of to the general public on the open market. Then came the Anti-Monopoly Law, incorporating, somewhat mechanically, many of the features of American antitrust legislation and designed to prevent reappearance of any of the steps tending toward monopolies. A Fair Trade Commission was created under this law. The last of the measures was the Elimination of Excessive Concentration of Economic Power Law which passed the Diet, under strong pressures from the occupying authorities, on December 18, 1947. The law, without setting up any clear-cut standards or criteria, conferred upon the Holding Company Liquidation Commission the authority to order the dissolution or reorganization of any corporation whose size or structure could be described as partaking of the character of "excessive concentration of economic power."

As long as the anti-monopoly measures were based on the sense of retributive justice, and to the extent they were alien in philosophy and were hastily pushed down the throats of unwilling Japanese, their effective life was destined to be short. Actually, the turning point came rather early. It was heralded by the famed speech, on January 6, 1948,

<sup>12</sup> The Far Eastern Commission raised in April, 1947, the "permissible" standard of living of Japanese to their own 1930-34 level. In real terms, this was probably about three times the level of the poorest victim nation in Asia.

by Mr. K. C. Royall, U.S. Under-Secretary of Army, in which he called for a re-examination of America's attitude toward Japan, saying that the new conditions in world politics had produced "an inevitable area of conflict between the original concept of broad demilitarization and the *new purpose* of building a self-supporting nation." (Italics added.) The speech was rapidly followed by General McCoy's statement at the Far Eastern Commission (January 21, 1948), the so-called *Strike Report* drafted for the U.S. Department of Army by the Overseas Consultants Incorporated (March 2, 1948), and the Report of the Johnston Committee (May 19, 1948) which concluded that the United States should, in its own interest, now assist in the industrial recovery of Japan. Once the tide was turned, whatever measure appeared, or was alleged by the Japanese business world, to retard the recovery was either watered down or suspended. The reparation target was drastically cut and the implementation of anti-monopoly legislation became secondary to the need of utilizing the existing organization of industries to the fullest advantage. The fact that the enactment of these laws was not voluntary on the part of Japanese hastened the atavistic process further. The Deconcentration Law was never seriously put into effect. The Anti-Monopoly Law was soon revised to permit a manufacturing corporation to hold shares of competitive firms, to raise the upper limit of shareholding by financial institutions from five to ten percent, to legalize again the system of interlocking directorates, and to widen the scope of cartels that could be formed. It was almost natural that the "back to normalcy" trend finally found its way in the consolidation and realignment of firms along the former *zaibatsu* lines. Their trade names, such as Mitsui, Mitsubishi, Sumitomo, and so on, which had been once barred from use, were revived after Japan regained its independence in 1952; and the forced separation and splintering of their erstwhile integral structures have been largely repaired. True, there are no longer holding companies at the apex which had been characteristically controlled by respective *zaibatsu* families. In their place, however, has emerged the central role played by the bank of each group, such as the Mitsui Bank for the Mitsui group, and also a new coordinating organization of presidents of firms belonging to the same group. It appears that all the effort put into the anti-monopoly aspect of the economic democratization program almost came to nought.

The lack of success in the anti-monopoly effort was probably most apparent from the standpoint of the designers of the program on the Occupation side. But those Japanese economists who concerned themselves with the monopoly issue had a different approach from the outset and thus were neither disappointed nor pleased by the "back to normalcy" outcome. In a word, their concern had been broader. Most

of them never doubted the positive contribution of *zaibatsu* to economic growth and efficiency within the context of Japanese capitalism. By internalizing external economies, by distributing risk, and by being able to afford large scale industrial research, the *zaibatsu* played a role which is almost exactly like the one Rosenstein-Rodan assigned to his suggested Trust for industrialization of underdeveloped countries.<sup>13</sup> For the Japanese economists who wrestled with the problem of monopoly in their own country, the criticism of monopoly implied necessarily a criticism of capitalism. For the Japanese economist, the alternative was not between monopolistic capitalism and competitive capitalism, but rather between capitalism and socialism. A tremendous number of professional works exist, dating back to 1930, which probe analytically and empirically into the mechanism of the dynamic interrelations of the military, the bureaucracy, and the monopoly. These works are, for the most part, Marxist-oriented. But the fact is that their analysis and prediction were borne out by the course of events in the 1930's. When such is the case, it is to be expected that the proponents of the successful hypothesis will persist in holding on to their methodology.

This is the setting in which the problem of anti-monopoly measures presented itself in postwar Japan. What we might call the Occupation ideology on this matter received relatively little attention from Japanese economists except to the extent to which a few of them were directly involved in the administration of the Holding Company Liquidation Commission and the Fair Trade Commission. A major part of the research done in this connection was by the civil servants concerned and usually consisted of a compilation of historical accounts such as *Nihon no Zaibatsu to sono Kaitai* ("The *Zaibatsu* in Japan and their Liquidation"), 1950, by the Holding Company Liquidation Commission, or a series of straightforward factual surveys similar to those published by the Fair Trade Institute.<sup>14</sup> As a slightly more interpretive work, we should probably mention *Mitsui-Mitsubishi-Sumitomo: Present Status of the Former Zaibatsu Enterprises* (in English), 1955, edited by the Mitsubishi Economic Research Institute, which despite its *zaibatsu* name has the tradition of objective economic research.

On the other hand, works by Marxist-oriented economists on the

<sup>13</sup> See P. N. Rosenstein-Rodan, "Problems of Industrialization of Eastern and South-Eastern Europe," *Econ. Jour.*, 1943, 53, 202-11.

<sup>14</sup> This is the institute which was established within the Fair Trade Commission apparently in order to avoid restrictive budgetary controls over publications, for sale, of the research done by the staff of the Commission. There exist many studies by them on concentration ratios of different industries as well as on the mode of concentration in the banking and financial sector. Since 1957, the Institute has been publishing, in English, a quarterly journal called *Fair Trade*, which often contains extremely revealing articles.

problem of monopoly are too numerous to list here. Aside from the ideological issue<sup>15</sup> relating to the character of monopoly capitalism in Japan today, which is being disputed among themselves, their major efforts appear to have been directed thus far to two groups of problems: (a) the manner in which monopolistic regrouping has been taking place and the nature of new monopolistic control devices; and (b) detailed industry studies. The latter are mainly empirical, and as such, Marxists have no monopoly on the subject. In fact, this is the field where economists with differing orientation, along with engineers and others in business, could cooperate. The outstanding example of such a cooperative work is *Gendai Nihon Sangyō Kōza* ("Study Series on Industries of Present-day Japan"), 8 vols., 1959-60, under the general editorship of Hiromi Arisawa, a distinguished economist with a Marxist orientation. "Study series on industries" suddenly became a fashion after 1958 and there are at present five others in process, with differing emphases and ideological orientation. However, it is natural that the monopoly aspect is treated as an integral part of such industry studies only in the Marxist-oriented studies.

Since the Occupation shifted its policy on Japan toward encouraging rapid economic recovery, Marxist economists predicted that the *zai-batsu* form of monopolistic structure would re-emerge. Thus they followed closely the turn of events and, step by step, the manner of regrouping and consolidation in business organizations. The focus in these studies was directed toward interfirm relations of all kinds, such as: holding of equity shares in competitive and/or complementary firms; interlocking directorates; membership in a specific coordinating organization of corporate managers; bias in financing by a particular bank toward certain firms; and so on. This type of monopoly study may be classified as "physiological" in contrast to the "anatomical" in which a major tool of analysis would be the concentration ratio in a particular industry.<sup>16</sup> The number of economists who concern them-

<sup>15</sup> What should be the politico-economic characterization of the present-day Japanese capitalism is their problem. One group, represented by *Nihon Shihonshugi Kōza*, *op. cit.*, maintains that it is essentially a dependency to the United States. Another group, represented by *Gendai Nihon Shihonshugi Taikēi* ("A Systematic View on Contemporary Japanese Capitalism"), 7 vols., 1957-58, maintains that the class conflict between the monopoly capital and the working class is the overriding feature. This group continues the tradition of the so-called *Rōnō-Ha*. A third group, represented by *Gendai Nihon no Seiji to Keizai* ("Politics and Economics of Contemporary Japan"), 4 vols., 1958-59, maintains that the threatening revival of Japanese imperialism is the key to the present situation. Naturally, each one of the three has its own unique policy implications. Thus the issue is being fought rather bitterly.

<sup>16</sup> A detailed work of the "anatomical" type, based upon the statistics supplied by the Fair Trade Commission, is best represented by Hitoshi Misonou's *Nihon no Dokusen Shihon* ("Monopoly Capital in Japan"), 1960.

selves with the "physiology" of Japanese monopolies is large,<sup>17</sup> bringing out one case study after another on the trees in the forest, but not yet, it seems, coming to a point where the entire forest can be viewed in historical perspective. At least, however, they appear to agree generally that what is emerging is not a simple replica of the old *zaibatsu* form but something more rational in interfirm relations than existed under the old regime.

#### IV. *The Debate on the Growth Potential*

It is probably no exaggeration to say that the debate on the growth potential of the Japanese economy was the most important event in the circles of professional modern economists<sup>18</sup> in postwar Japan. This discussion, which gathered momentum from 1958 onwards, involved theorists and statisticians as well as policy-oriented economists, and academic economists as well as government and business economists. Participants in the debate, in the sense of having contributed at least one article on the subject, probably number fifty or more, which implies that there are at least that many articles and/or books in print thus far. By general consensus, the star of the debate is Osamu Shimomura, a government economist at the time the debate began, who attracted general attention through his extremely optimistic views and also through the fact that he has been an important member of Prime Minister Ikeda's brain trust for a number of years.

To be exact, we must say that the debate began when the government *Economic White Paper* came out in 1957 with the catch phrase: "*mohaya sengo dewa nai!*" ("Special factors of the postwar period can no longer be relied on.") Theoretical support for this premise was supplied by Miyoei Shinohara, who, relying mainly on the statistical evidence, advanced a thesis that a country enjoying a rehabilitation boom after the war was likely to experience a sudden shift in its rate of growth. Japan's growth rate in real GNP averaged at 11.3 percent between 1946 and 1951, but dipped rather markedly to 7.7 during 1951-56, and was expected to fall, according to M. Shinohara, to 5 percent or thereabouts after 1956. The *Economic White Paper* of 1957 gave assent to this view. However, when the so-called "New Long-Term Economic Plan" was drafted toward the end of 1957 as a prospectus for the five-year period of 1958-63, the rate of growth of real GNP

<sup>17</sup> A few studies along this line may be mentioned here: Yoshio Kobayshi (ed.), *Kigyō Keiretsu no Jittai* ("Empirical Studies on Grouping of Firms"), 1958; *Nempō Keiretsu no Kenkyū* ("Annual Report on Studies on Grouping") edited by Keizai Chōsa Kyōkai (Economic Research Association); Iwao Kunitatsu, *Nihon Kogyō no Kōzō* ("The Structure of Japanese Industries"), 1960; Giichi Miyazaki, *Dokusendō Sokutei no Shomondai* ("Problems Arising in the Measurement of the Degree of Monopoly"), 1961.

<sup>18</sup> Non-Marxist economists.

was hypothesized as 6.5 percent; and most people felt that this was too ambitious. Shimomura was probably a sole exception among economists at that time to criticize the target as too low. He proposed the 10 percent annual rate as an easily feasible one.

An important part of Shimomura's reasoning was a recognition of an historical upheaval in the investment function of the Japanese business world and in the consumption function of the Japanese people. Schumpeter would probably have called such a conjuncture the coinciding of a Kondratief upswing with a Juglar prosperity. However, Shimomura formulated the original version of his theory rather crudely by using a simple Domar-type model. First, the net increase in output capacity maturing in any particular year is considered to be roughly equal to the private gross capital formation in plant and equipment in the preceding year. This implies:

$$(1) \quad GNP_t - GNP_{t-1} = \sigma \cdot PGCF_{t-1}$$

where  $PGCF$  stands for private gross capital formation in plant and equipment, and  $\sigma$  for the productivity of such addition to capital. Dividing both sides by  $GNP_{t-1}$  and substituting the assumed value of unity into  $\sigma$ , we obtain:

$$(2) \quad \frac{GNP_t - GNP_{t-1}}{GNP_{t-1}} = \frac{PGCF_{t-1}}{GNP_{t-1}}$$

which means that the rate of growth of  $GNP$  is equal to the ratio of private gross capital formation in plant and equipment to  $GNP$ . In any particular year we first obtain the projected value of  $PGCF$  and then its preliminary estimate along with the preliminary estimate of  $GNP$ . These figures provide the government with a guide to measures needed to stimulate effective demand (social overhead capital and so on) in order to obtain equilibrium growth. The only danger in the policy of making  $[GNP_t - GNP_{t-1}]$  equal to  $PGCF_{t-1}$  in a country like Japan could come in relation to the balancing of international payments. Shimomura, however, found no ground for fear in this area. His formulation of the problem was:

$$(3) \quad \begin{aligned} \Delta M &= m \cdot \Delta GNP \\ \Delta X &= e \cdot g_w \cdot X \end{aligned}$$

where  $M$  and  $X$  stand for foreign exchange debit and credit respectively,  $m$  the marginal propensity to import,  $g_w$  the rate of growth of world income, and  $e$  the elasticity of Japan's exports with respect to world income. The condition for  $\Delta X \geq \Delta M$  is obviously:

$$(4) \quad e \cdot g_w \cdot X \geq m \cdot \Delta GNP$$

If we divide both sides by  $GNP$ , we obtain:

$$(5) \quad e \cdot g_w \frac{X}{GNP} \geq m \cdot \frac{\Delta GNP}{GNP}$$

Shimomura estimated  $m$  to be about 9 percent or less,  $e$  to be between 3 and 3.5 and  $g_w$  to be at least 3 percent. If we take the lower figure of 3 for  $e$ , the condition is simplified to:

$$(6) \quad \frac{X}{GNP} \geq \frac{\Delta GNP}{GNP}$$

In other words, the upper limit of the growth rate of  $GNP$  is the ratio of exports (or gross receipts in foreign exchange accounts) to  $GNP$ , which in the case of Japan is usually above 10 percent.

It was easy enough to find fault with Shimomura's initial presentation of the problem. In particular, the specific magnitudes he assigned to parameters  $\sigma$  and  $m$  were called into question. The crude hypothesis of a lag of one year in the maturing of capacity creation was also scrutinized. The stability of the value of  $e$  was doubted. Indirect productivity effect of social overhead capital was suggested; and so on. The most lively period of debate was from 1959 to 1960, a period which coincided with the phenomenal rise in Japan's real  $GNP$  of 17 and 10 percent respectively in its annual rate. In answer to numerous criticisms, Shimomura did modify his original model somewhat. But supported statistically by the actual fact of rapid growth and politically by Ikeda, who became Prime Minister in 1960, Shimomura has retained his role as the oracle of optimism (11 percent or higher annual rate of growth in real  $GNP$  up to 1970), and is now mobilizing revised statistics to support his argument.<sup>19</sup> Meanwhile, Prime Minister Ikeda, confident of his brain trust's vision and reasoning, made it government policy to double Japan's real national income in ten years (implying a 7.2 percent annual growth-rate) and has been setting the tone of optimism in the climate of business expectation. Thus the debate is no longer academic. It has become a political one as well. It continues with new ramifications developed and different angles opened.<sup>20</sup>

<sup>19</sup> In particular, see his paper presented at the Nagoya Meeting of Japan Theoretical Economics Association in October 1960.

<sup>20</sup> Since the debate has been eminently topical, most of the publications on the subject have been in the form of articles. But two books, bearing directly on the debate, have come out: *Nihon Keisai no Seichōryoku* ("Growth Potential of Japanese Economy"), 1959, edited by Kin'yū Zaisei Jijō Kenkyū Kai (Research Association on Monetary and Fiscal Matters), incorporating the most important articles by Shimomura and his critics; and *Nihon Keisai no Seichō* ("The Growth of Japanese Economy"), 1960, supervised by Ichirō Nakayama, containing the stenographic record of a symposium held on the subject in January 1960 and also a concise summary account of the debate up to the spring of 1960.

One of the side issues in the debate from the very beginning has been the question involving disaggregation. It was asked: Assuming that it is possible to maintain a high growth rate, what will be the effect on different sectors of the economy, in particular, the effect on different classes of people? Will not a rapid growth in a capitalist country like Japan necessarily mean a high profit prosperity and an increasing skewness in the size distribution of income? Defenders of the Shimomura thesis maintained that a rapid growth would be the very thing which could solve all kinds of structural imbalance. The solution for the rush-hour crowd of commuter trains is to increase the number of cars operated—such was the simile they used. Against this, critics pointed out the structural characteristics of Japanese capitalism and the manner in which they tend to become perpetuated under the conditions of capitalistic prosperity. One such characteristic—the dual structure of employment—has long been an important subject of study and debate in Japan and therefore is to be independently dealt with in the next section.

### *V. Dual Structure of Employment*

Dual structure of employment—a term that is used rather generally—probably requires a closer description as it is applied to the Japanese context. In brief, it refers to a duality in the labor market, but certain peculiar elements in the Japanese situation are worth mentioning. Let us take the sector of manufacturing industries as our example. All the establishments in the sector can be divided, fairly distinctly, into two groups: up-to-date large scale firms on the one hand and technically-lagging small firms on the other. The former recruits workers from recent graduates of schools at a wage rate not necessarily different from the prevailing one for new entrants anywhere. Once they are hired, however, they become “permanent employees” of the establishment—not to be fired or laid off until retirement age. They become the core of the working force in a particular establishment and rise in position and pay automatically with age. Since they are not to be laid off even in time of slack business, the management limits their number to the minimum and supplements them by a substantial number of temporary hands even in ordinary times. These temporary employees are of two kinds: (a) those who are usually hired for a stretch of more than a month and remain as stand-bys for a particular firm even when they are idle, and (b) those who are hired by the day. Even these cushions may not be sufficient to meet the severity of fluctuations at times. Thus large firms usually cultivate around themselves a fair number of small firms as a type of putting-out system. (There are, of course, other reasons for the viable existence of small firms.) Workers in these small

firms, whether or not they are directly associated with a large parent firm, are usually recruited locally and/or nepotistically, they come perhaps from farms, from among older men, and sometimes from recent graduates of schools. In other words, the labor force in manufacturing can be divided more or less distinctly into two groups: (a) permanent employees of large firms, and (b) others. It is most significant that there is practically no mobility between the two. It is also to be noted that, aside from the similarity in the starting wage, the wage scale differs greatly between the two groups.

The duality referred to has been the subject of discussion for many years in Japan, and the problem of mitigating its undesirable effects, if not its dissolution, has been prominent on the agenda of the postwar Japan's democratization. It might have been thought that the mushroom growth of trade unions, coupled with the enlightened policy of the government, would have the effect of promoting progress in the modernization of the labor market. However, the achievement of this aim thus far has been less than satisfactory. For one thing, unions have touched mainly upon the category of workers belonging to "permanent employees" of large firms, and their success in raising wages has tended to widen the wage differential between the two layers of the labor market. Here is a paradoxical situation of organized workers in large firms sharing in the productivity rise with the monopoly capitalists who tend to exploit their small subsidiary firms, which in turn can survive only by exploiting their nonunion workers to the utmost. This is a paradoxical situation, indeed, especially when we reflect upon the fact that the organized workers in large firms are the most radical elements in Japanese politics, while the unorganized workers in small firms have been shown generally to share the politically conservative views of their employers. The paradox, however, is resolved once we shift our focus to the sociology of the situation. The system of "permanent employees" in large modern establishments has its roots in the patriarchal tradition of Japan, just as the same can be said of the family-like atmosphere of small firms, which inhibits the growth of trade unions requiring impersonal, matter-of-fact attitudes to defend individual rights. The duality in market structure, which may be said to have its economic logic in the fact that both the modern sector and the small-size sector complement each other for respective viable existence, goes hand in hand with the duality in employment structure which is a part of the sociological picture of present day Japan. None of the essential aspects of this entire situation can be dealt with in an isolated manner. Thus the task for labor market modernization, though superficially amenable to *ad hoc* measures narrowly directed to the purpose at hand,

unavoidably involves a whole range of problems where improvement will take time.

Such is the background against which many economists with differing interests have tackled the problem of the dual structure of employment in the postwar period. In particular, it should be noted that it was two American economists who provided an entirely fresh approach to the problem and made Japanese colleagues realize the peculiarities of the Japanese situation, which tended to be taken for granted by the Japanese themselves. They were J. Abegglen (*The Japanese Factory*, 1958) and S. Levine (*Industrial Relations in Postwar Japan*, 1958). Among the Japanese economists, on the other hand, Kazuo Ohkōchi led a school of his own, as it were, to delve into the problem both historically and statistically. He has laid special emphasis on the characteristically historical origin of Japanese wage labor, namely, that most of the men who came from farms to cities to work in factories did so as bachelors with their family roots on the farm still intact. Thus employers could hire them initially at a price that was equivalent to the subsistence of a single young man. It was customary for the employer to incur cost in training his workers in their skill, and as protection against having his trained men enticed away from his firm, the employer would make it a practice to raise wages by a seniority system. As the worker became established in a city, married and had children, his subsistence needs rose, and the adjustment in wage rate usually took the form of family allowances. It is easy to see that such a wage system induces workers, even when unionized, to be concerned mainly with bargaining with respective firms and never on an industry-wide basis—thus the so-called system of the firm-by-firm union. Kazuo Ohkōchi and his younger colleagues have been studying this entire problem in a systematic fashion and during the past decade have published a large number of theoretical works and statistical studies. Probably the most representative among these studies is S. Ujihara, W. Fujita and N. Funahashi, *Nihongata Rōdō Kumiai to Nenkō Chingin Seido* ("Trade Unions Peculiar to Japan and the Wage System by Seniority"), 1960. Ohkōchi himself has numerous articles to his credit.

Peculiarities of the labor market have their counterpart in the size-structure of firms which provide the demand for labor. The size of a firm is not only a function of the nature of the industry but is subject to a bias rooted in the monopoly structure of the economy, with both the government and banks favoring the larger firms over the smaller ones. Here is an instance, one may say, of circular causation à la Myrdal. If it is true that within one and the same industry Firm A can generally pay a higher wage than Firm B, it must be due to the fact

that Firm A's value added per worker is higher than that of Firm B; such a difference in turn must be due to a difference in capital intensity. Reasoning of this nature leads us to look into the difference in the facility with which different firms are able to obtain capital funds. The best of the studies which pursued this line of thought empirically, with highly illuminating results, are found among the research of government economists and of economists like Miyoei Shinohara, who is often an adviser to government economic agencies. In particular, we might mention *Shihon Kōzō to Kigyō-kan Kakusa* ("Capital Structure and Inter-Firm Differentials"), Research Series No. 6 of the Institute of Economic Research, Economic Planning Agency, 1960; and among Shinohara's works *Sangyō Kōzō* ("The Structure of Industries"), 1959, is representative of this approach.

The problem of the dual structure of employment has had another focus in intensive empirical studies in the postwar period, namely, problems pertaining to small and pigmy-size firms. Here the problem is concerned not only with that of employed workers but also with that of employers themselves—especially if the latter are individual proprietors. The interest in this aspect of the Japanese economy was heightened markedly in the postwar period, especially among that circle of Asian economists who saw in the prevalence of small and pigmy-size firms in fast-growing Japan a positive solution for a country that still suffers from surplus labor and yet is attempting to industrialize rapidly with modern labor-saving techniques. Most of the Japanese economists who specialized in the problems of small and pigmy-size firms were skeptical of the emulative quality of their subject matter. However, it cannot be doubted that the inquiries from abroad stimulated a renewed interest in the subject and placed it in a broader framework for discussion. Whereas in the prewar days the subject was looked upon more as a peculiarly Japanese problem—somewhat in the manner Americans would deal with the Negro problem in the South—the context has now widened into the possibility of generalizing certain aspects of the problem. In other words, the subject matter has become a concern of theoretical economists as well as of economists with a more practical approach. With such an atmosphere in the background, large numbers of studies with differing emphases have appeared in the field. Three study series in particular are worthy of note. The first is the product of the Small and Pigmy-Size Industry Committee of Nihon Gakujutsu Shin-kō Kai (Japanese Association for the Promotion of Sciences) which succeeded in mobilizing economists from both Marxist and non-Marxist groups, and so far has brought out six solid volumes on different aspects of the problem. The second series, edited by Kiichi Isobe, Ichiro Nakayama, Hiromi Arisawa, and Ichiro Oshikawa, has also been pub-

lished in six volumes with a much greater emphasis on the statistical and empirical sides, while the third series of four volumes is patently of Marxist orientation.

#### VI. *Advances in Economic Statistics*

Prewar Japan was not especially distinguished for its abundance of economic statistics. Fiscal, monetary, and foreign trade statistics were adequate enough, mainly because their compilation was an administrative by-product. To be sure, various other statistical series were available in a regular form, some of them dating back to 1878, with varying degrees of sophistication and accuracy. However, the fact that any attempt at reconstructing reasonably satisfactory national income statistics before 1930 faced almost insurmountable difficulties is an indication of a serious gap in economic statistics as a whole.<sup>21</sup>

The termination of the war brought a new era in the compilation of statistics in Japan. Probably the most important was the deliberate forcing of the pace by the Occupation. For example, it was a matter of administrative necessity to have accurate statistics on such matters as crops, industrial production of all kinds, and price movements in grey and black markets. Thus the Occupation brought into Japan a special group of statistical experts headed by Dr. Stuart Rice—twice in five years (December 1946 and March 1951)—to advise the Japanese government in the task of modernizing the administrative structure and of improving methodology in relation to governmental statistical work in general. However, even before Dr. Rice came on his first visit, the Occupation administration, handicapped as it was by lack of qualified personnel, gropingly began the work of innovation in data-collecting and methodology. In this, there were factors, other than the policy orientation of the Occupation, which facilitated the undertaking. Over the decade preceding the end of the war, the small-sampling technique had made remarkable progress, and when the Occupation came, there were among the Japanese themselves a number of experts whose services could immediately be put to use. Probably more important, there had arisen in the immediate postwar period a strong and widespread interest among younger Japanese economists in econometric research of all kinds. For modern economists in particular, the erstwhile hold which "doctrinology" had over the profession was weakened. Economists like Miyoehei Shinohara and Shinichi Ichimura, as was mentioned earlier, led the younger group in the energetic application of new tech-

<sup>21</sup> There were exceptions to this rule. The most notable is the family expenditure survey which started in 1925 and was continued annually from 1930 to 1942. Initially, 7800 families were included in the sample, chosen mainly on a voluntary basis to report in detail their monthly expenditures in the prescribed accounting book.

niques to the mass of data pertaining to Japan which, at the outset, was inadequate and lean. The econometricians' enthusiasm and the statisticians' zeal started raising the level of both quality and quantity in the supply of economic statistics in Japan. It should also be mentioned in this connection that the publication of the first *Economic White Paper* in 1947 under the Katayama Cabinet started a kind of fashion among several economic ministries of issuing factual survey reports annually, and thus provided a further stimulus for the improvement and proliferation of statistical series compiled by government agencies. A so-called "White Paper" is now made public annually, not only by the Economic Planning Agency, which was the first, but also by the Ministry of International Trade and Industry, the Ministry of Labor, the Ministry of Agriculture and Forestry, the Ministry of Welfare, the Ministry of Transportation, as well as by specialized agencies within the Ministries, such as the National Income Unit of the Economic Planning Agency who published the *National Income White Paper*.

Once the Japanese became interested in statistics, it appears that there has been no stopping in the proliferation of such data. Through the legislative enactment of the Statistics Act of 1947, all the statistical activities of the government agencies came to be unified. A system of the "designated statistics" was established for important series on the one hand, and a system of compulsory reporting of other numerical facts to the Bureau of Statistics Standards on the other. Once a certain series becomes a "designated statistics," the agency concerned is empowered to make the filing by subjects obligatory and to enter the premises needed for the purpose; in turn, the agency must protect the confidential character of the filing by subjects and make public the results of the investigation as quickly as possible. As of September, 1959, there were on the official register altogether 57 sets of such "designated statistics," ranging from the population census to the nation-wide consumer behavior survey which is the latest addition. It will be too cumbersome to enumerate them all; but the list can be found in *Nihon Tōkei Hattatsu Shi* ("History of Development of Statistics in Japan"), 1960, compiled by the Nihon Tōkei Kenkyūsho ("Japan Statistical Research Institute"). As has been remarked by Shinohara, the econometrician with the most extensive knowledge of current economic statistics of Japan, the proliferation of statistical series has now reached a stage in Japan such that, even with the tremendously larger number of econometricians now at work as compared with prewar Japan, one finds it difficult to keep one's head above the deluge. The econometricians' complaint two decades ago has been answered by statisticians with vengeance.

This, however, is true only in so far as the current data are con-

cerned. As for the historical statistics, an attempt at radical improvement has been painfully slow. The work of compilation of what had already existed was naturally the first stage; and in this regard Yūzo Yamada's *Nihon Kokumin Shotoku Suikei Shiryō* ("Data Book for the Estimation of Japanese National Income Statistics"), first edition, 1951, was a pioneering work, that more by revealing the lacunae and limitations than by displaying positive results, acted as a stimulus for further research. This task was begun by the Institute of Economic Research, Hitotsubashi University, as a long-term project led mainly by Kazushi Ohkawa; and the work of digging out new data and of applying new methods to old data, altogether involving a most laborious and painstaking research activity, is still in progress. Interim results of the Ohkawa team, however, have been made public already in the form of *The Growth Rate of the Japanese Economy since 1878*, 1957; and Henry Rosovsky's *Capital Formation in Japan*, 1961, also may be considered, in a broad sense, as a product of the Ohkawa group. The Center for Japanese Economic Statistics at the Hitotsubashi Institute was recently established and studies of quantitative Japanese economic history will no doubt gain further momentum.